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"To the solid ground
Of Nature trusts the mind that builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, MAY 2, 1872

THE CIVIL ENGINEERS' BANQUET

WE do not grudge our friends the Civil Engineers their annual felicitations, nor Mr. Gladstone his congenial moral reflections. It were hardly worth while to dissect after-dinner rhetoric, however full of fallacies. But those ever-watchful teachers of mankind, the daily press, have pounced upon the speeches delivered on Wednesday week, and have made them an occasion for propounding solemnly what was spoken hilariously; and this deserves looking to.

The *Times*, of course, armed at every point, does battle valiantly for decentralisation of science, because that notion seemed to find favour with the notabilities of the evening. Mr. Hawksley, president of the Civil Engineers' Institute, in toasting Her Majesty's Ministers, complimented them on the "performance of the negative duty of letting his profession alone," adding, with unconscious satire, that what the engineers had done "they had achieved, not through, but in spite of, all Governments." These two sentiments are quite intelligible and quite true; but the conclusion of the speech, which informs us that "the Civil Engineers of this country approached the Government with perfect reliance on its purity," conveyed a needless truism; to the pure all things are pure—to pure engineers even a British Government is pure, of course. But why dwell on so obvious a fact?

Well, this put Mr. Gladstone on his mettle. At once absorbing the tribute to their inaction—rather a novel compliment to his Administration—he evolved out of it this syllogism: Engineering is science; engineering likes being left to its own devices; therefore all science should be left alone. This sort of logic is quite good enough for dinner talk, but not for breakfast reading. What may be excused to convivial excitement in a flattered guest cannot always be permitted to a journalist after an interval for reflection.

The *Times* of the 26th ult., after expatiating on the advantages of non-intervention in things in general, thus disposes of science. "If," it says, "under such conditions of Government, the State does nothing for science, it cannot be helped; nor need it be much lamented, considering how very little science stands in need of the aid. If," it goes on to say, "the Institution of Civil Engineers had been a creation of the State, fostered by State bounties, and favoured by State protection, its members would never have acquired such a position in the country as they justly boast of holding now. As it is, they have fought their way and been the founders of their own fortune; and so their president has the pleasure of telling the Prime Minister of the Kingdom that he and his constituents have been and are independent of all Governments whatever."

We give the sentence entire, because the nonsense it contains does not admit of greater condensation. Let us examine it a little. How is the engineering profession typical, as here asserted, of science? What is that profession? Simply, and without any intended offence, a profession for making money. Men put their sons into it, and have them trained, rather imperfectly in England it must be confessed, and push them forward in it, solely because the contracts, commissions, and fees, are enormous, and the chances of making a fortune pretty fair. We by no means object to this in a broad way. Other professions, held in high honour—the law, for instance—are exactly in the same category. If men possessed, or believed to be possessed, of special knowledge, find that a high price is put on that knowledge in the market, they are of course justified, as long as they perform honestly what they undertake, in demanding the highest price they can get for it. But in this respect how do they differ, not merely from lawyers, but from manufacturers and even from tradesmen? Is there any difference between making and selling so many yards of calico and so many miles of railway, between supplying customers with patent umbrellas and a patent locomotive? All are results of science, and may be products of brains other than those of the vendors. If engineers are able, by vending bridges, railways, and graving docks, to amass in a few years colossal fortunes, as we know they do, what Government help can they need more than the successful cotton lord, or the enterprising grocer, who also manage their business without external aid?

This being granted, why, asks the *Times*, should not the principle of non-intervention be applied to science? Simply because what is generally understood by the term "Science" is not a remunerative occupation. We do not say that no one ever made money by pure science; some men have done so, as is well known, by means such as telegraphic inventions and chemical processes applied to the arts; but even these, and they are very few, have not become the millionaires, of whom not a small number are to be found amongst engineers and manufacturers. Putting aside these rare exceptions, science does not bring wealth to its cultivators. For instance, who ever made money by astronomy? What did the discovery of Neptune, the highest scientific achievement of this age, bring to Professor Adams but tardy fame? Are investigations of the properties of light, sound, gravity, magnetism, profitable pursuits? Was the inventor of a new calculus ever made rich thereby?

Shall we contend, therefore, that Government should supply the shortcomings of a scientific career, and place those who adopt it on a par pecuniarily with successful engineers? By no means. We must look a little deeper for the reason why State should aid Science. We shall find, if we examine the whole domain of Science, that there are extensive tracts which require for their vigorous cultivation very costly appliances and a long expenditure of time. Poor men cannot afford the one at all, and cannot live if they devote themselves to the other without remuneration. Hence, if such branches of science do not receive aid from without, they must languish, if not be entirely neglected. But this cannot happen without depriving the community of some addition to its material advantages which it might otherwise possess. It is notorious, not, perhaps, to all the writers of the *Times*, but to those conversant with the state of science in England, that this evil is with us in full operation with constantly increasing force. Rich men do, it is true, sometimes devote themselves to science. But no individual can do more than labour during his lifetime; and what is chiefly wanted now, in several of the most important lines of investigations, is uninterrupted continuity during immense periods of time. Will our friends, the Engineers, apply some of that "lavish liberality and unbounded enterprise," of which we have heard so much, in this direction? We suppose not. Whatever may have been the love of knowledge for its own sake which distinguished the first founders of the profession, the modern "leading engineer" knows better than to put his money and time into so unprofitable a business.

England is at this moment behind every other civilised nation in the means afforded for the cultivation of those branches of science which do not yield immediate profit. But there are men, not connected with either the Government, the *Times*, or the Civil Engineer Institute, who are alive to the peril of prolonging this neglect, and who will not rest until they have opened the eyes of their countrymen to its imminence and magnitude, at present beyond the ken of their governors and their teachers. When they have attained their object, the *Times* will proffer them its aid.

THE ERUPTION OF MOUNT VESUVIUS

THE great eruption of Mount Vesuvius, with the telegraphic accounts of which the readers of the daily papers have been familiar for the past week, is undoubtedly one of the most considerable of modern times. Whether the worst is yet over seems still uncertain while we are writing; but even if this be the case, the mass of molten lava ejected, and the amount of damage done, will appear to bear comparison with those of almost any recent eruption. One account speaks of it as the grandest eruption since 1631.

The telegraphic accounts at present received are necessarily vague, and to a certain extent conflicting, and of course it is at present impossible to do more than chronicle. Scientific results must follow afterwards.

The correspondent of the *Daily News* says, under date of Sunday afternoon:—"I have just returned from visiting the spot where the victims of the eruption perished. Streams of lava, thirty metres wide and nearly ten metres in height, were still fiery and smoking. The detonations continue as before. Part of San Sebastiano is entirely destroyed, with everything near. King Victor Emmanuel and Signor Lanza were on the spot distributing money to the sufferers. All the people fled at the time, but some were beginning to return with their goods. The shower of cinders had ceased, the sky was blue, and Mount Vesuvius, with a colossal column of smoke above it, had a grandly imposing appearance."

San Sebastiano is a village on the north-western slope of Vesuvius, almost exactly in the direction of Naples. None of the earlier eruptions extended in this direction, but a stream of lava flowed almost close to the village in one of the early eruptions of this century. The lava is here spoken of as being sixteen feet deep in places, and in other despatches San Sebastiano and the neighbouring village of Masso di Somma are spoken of as having been nearly destroyed. This stream of lava is described as having several times changed its direction, and it is probably a branch of the same which has threatened Portici and Persina (no doubt Resina, almost close to Herculaneum) near the sea-coast, and caused the abandonment of these villages.

It must be a different stream which has partially or entirely destroyed Torre del Greco, one of the beautiful villages which lie on the shores of the Bay of Naples, in a south-westerly direction from the centre of the mountain. This village, or its immediate neighbourhood, has been overwhelmed several times within the last two centuries. The statement that "the lava now reaches from Torre del Greco to within five kilometres of the eastern coast, and threatens several other communes, the inhabitants of which have, in consequence, fled from their homes," is quite unintelligible.

The effect of the eruption at Naples up to Monday night is described as follows:—"Cinders fell all last night, and they still continue to fall at this moment as I send off the present despatch. A thin rain is also falling. Near Cercola the shower of scoriæ has compelled the soldiers to build huts in which they may obtain shelter. The church of San Giordano at Cercola has been destroyed. A number of people have been surrounded by the lava close to San Sebastiano. At Ottoiano a heavy shower of enormous blocks of lava has commenced. The railway is crowded with foreigners and Neapolitans hurrying away from Naples. A fresh eruption is feared, as loud explosions were heard last night even in this city."

The fall of cinders, even at Naples, is spoken of as so heavy that the sky seemed hidden by them, and they fell everywhere like rain. The plantations were covered with them, and people were walking with umbrellas to protect themselves from the downpour. The rain of burning scoriæ has reached as far as Scafati and Palermo. Readers of Prof. Phillips's work on Vesuvius will recollect that he throws considerable doubt on the correctness of the popular idea that Vesuvius emits *flame* during an erruption, though he thinks there are one or two authentic instances of this rare phenomenon. The accounts at present received do not throw much light on this point. One narrator states "Mount Vesuvius is displaying terrible electric phenomena, marked by flashes of lightning and vibrations of the earth;" and another, that "flames are bursting through several craters." Other accounts speak merely of the eruption of glowing lava and smoke brightly illuminated by it, and this may readily have been mistaken for flame.

Great credit is due to Prof. Palmieri, who has remained at his post at the Observatory to watch the eruption, and from whose observations a great advance of science may be anticipated. On Monday at noon he telegraphed as follows:—"Scoriæ in great abundance have fallen in the direction of the Observatory. The instruments at the Observatory are very much disturbed. The projectiles from the volcano rise to a height of more than a kilometre. The lava has ceased to flow."

It is satisfactory to know that recent letters speak of the first reports of the loss of life as having been somewhat exaggerated. Eighty persons are now stated to be missing; all Italians. Thirteen wounded were taken to the hospital; of these six are dead. No English or Americans are reported dead or wounded.

The latest telegrams received at the moment of going to press speak of an enormous column of "fire" being visible from Naples. Explosions, accompanied by shocks like those of an earthquake, were constantly occurring. Prof. Palmieri telegraphed from the Observatory on Tuesday that the roar of the volcano had ceased. Numerous flaming projectiles continued to be launched into the air, but with less force than previously. The smoke had decreased, and the shocks, though frequent, were not of a dangerous character.

SCIENCE PRIMERS

Science Primers: "Chemistry," by Prof. Roscoe; "Physics," by Prof. Balfour Stewart. (London: Macmillan, 1872.)

THESE little books illustrate an imperfectly accepted truth, that systematic elementary teaching is a late and not an early product of educational energy. The best headmasters of our schools have discovered the fallacy latent in our ancient belief that the ablest men are required to teach the oldest boys, and have in one or two famous cases acted on their discovery. It is easy for a young man fresh from University honours to pour his knowledge into minds which have been well prepared, and which approach more or less to the level of his own; but to teach a class of little boys, to realise their difficulties and to appreciate their ignorance, to understand the perplexity which oppresses them in the presence of statements long since axiomatic to ourselves, requires a mature and versatile intelligence, a mind which can communicate childish knowledge as readily and as joyously as it solves recondite problems; a combination of rare gifts with long and conscientious training.

And thus it is that the zeal for scientific teaching and the gathered scientific experience of the last fifteen years have only issued now in the books which form the subject of our notice. Scientific class-books hitherto have been either too difficult or too easy. They have been unavailable for beginners without the intervention of a practical teacher; or in their effort to be popular and simple they have abdicated half their value as instruments of educational discipline. In these books both extremes are avoided. Every stage of their teaching is based upon experiment; no law is enunciated till it has been proved. From first to last the student finds himself in immediate contact with Nature. His empirical knowledge of external things is systematised; simple every-day phenomena reveal to him their principles and *rationale;* he walks forth with a new eye to discern the meaning and the beauty of familiar sights and sounds, and with a mind upon the stretch for fresh discoveries. And, on the other hand, no previous training is essential to the teacher who adopts them as his guide. Any man, ignorant even of the first principles of chemistry and physics, yet fairly dexterous and intelligent, who will patiently master the books, and try each experiment for himself, is in a position to transmit their contents successfully and clearly. The officer may lecture to the soldiers of his regiment, the clergyman to the artisans of his parish, the national schoolmaster to the children of his school. Managers of middle schools, deterred as yet from including science in their course through lack of teachers and of text-books, will find their difficulty removed. The higher schools need no longer confine their scientific teaching to the senior forms, but may place the "Science Primer" along with the Latin grammar, in the hands of their youngest boys.

The expense of apparatus need not be considered formidable. A complete set for the course of Chemistry is set down at 5*l.* 10*s.*, for Physics at 19*l.* 3*s.* 8*d.* This last, however, includes such costly implements as the air-pump, balance, Grove's battery, and electrical machine. Leaving these to be obtained by special donation or borrowed for the occasions of their use, and deducting such further instruments and utensils as a handy man can make or convert at trifling cost, the outlay for the two courses may be bought considerably under 10*l.* And since the apparatus once established will require rare and slight renewal, one may hope that a moderate number of pupils with a moderate scale of fees would always provide this sum, more especially if the South Kensington authorities, in the presence of these manuals and of the revolution they may be expected to work, can be induced to extend the limits under which they furnish educational materials at half-price.

The names appended to the books guarantee their scientific accuracy, and their embodiment of the latest knowledge; but from the teacher's point of view they exhibit some few statements which are not quite clear, and which may deserve reconsideration. In the Chemistry Primer (Experiment 3, p. 7) the caustic soda is left unsupported in the tube. The description is probably compelled to follow the engraving; but most lecturers would, as is suggested in the appendix, use the U tube in such a case. Exp. 5, p. 10, does not clearly show that the heat due to chemical union is independent of the heat caused by the lamp; while statements 6 and 7, if taken together, produce in the pupil's mind a confusion between cause and effect. In pp. 23, 25, the allusions to acid and alkali, both new names to the student, might easily be accompanied by a marginal reference to p. 65, where the terms are explained. In Exp. 17, p. 27, it is not made evident in words that the hydrogen has passed from the one bottle into the other. Exp. 22, p. 34, and Exp. 40, p. 68, would gain in value if the action of the text, as well as its result, were described, such explanation of the first experiment being actually given later on at p. 87. In Exp. 29, pp. 44, 45, the diction of the first two paragraphs is confused and confusing, and it is doubtful if any lecturer would be able to conduct Exp. 31, p. 48, so as to retain the heated filings on the magnet. In Exp. 35, p. 56, the numbers on the drawing do not tally with those of the description. The explanation of the Davy lamp, p. 57, to which further reference is made in Physics, p. 86, is, to say the least of it, incomplete: and that of the safety matches on p. 72 is quite unintelligible. The definition of an Element, p. 58, and the phrase "difficultly fusible" on p. 99, suggest purely verbal criticisms.

In the Physic Primer, p. 2, force and motion should hardly be called "qualities" of dead matter. In Exp. 13, p. 22, the "simple arrangement" for moving the horizontal piston might be indicated. In p. 23 the large and small piston are not lettered in the description. In Exp. 17, p. 26, a shrewd pupil would inquire why the upward pressure should not, from all that appears in the text, expel water from the higher aperture as strongly as the downward pressure expels it from the lower aperture. In pp. 40, 41, some allusion to the Aneroid, if not to the Hypsometer, might fairly be expected, and in p. 46 it is not easy to see why the Syphon is described if its principle is not to be explained. In p. 65 there is an allusion to "the mercury in the bulb of glass," which is, in fact, there mentioned for the first time, and is it not true that in Exp. 52, p. 105, a principle of converse action is laid down on the evidence of a specific and almost a solitary instance?

It is possible that to criticise these points as blemishes suggests stupidity to the critic; if so, his stupidity is probably typical, and the authors would be the first to wish that their explanations should be self-sufficing, even to the obtuse. In any case we tender them our hearty thanks for work which marks a stage in the advance of scientific education. Its lingering progress hitherto has been owing to the want, not of zealous champions, but of united action. The labours of its advocates are now beginning to converge. The leaders of science and the leaders of education are drawing close together—on the one side eager to impart, on the other ready to receive, advice and guidance. By the publication of these books the most serious of the obstacles which have kept them separate is removed.

W. TUCKWELL

OUR BOOK SHELF

Astronomy and Geology compared. By Lord Ormathwaite. (London: J. Murray, 1872.)

THIS little volume is the product of a thoughtful and observant mind. Its main object is to contrast the certainty of the conclusions of astronomy, the exactitude with which eclipses can be foretold, and with which other astronomical phenomena recur, and the vagueness which hangs round many geological theories, as, for instance, those connected with the age of the various strata. A large portion of the volume is directed against the theories of Mr. Darwin in natural science, and Mr. Buckle in morals, theories which the author considers, in consequence of the vagueness of geological conclusions, to rest upon insufficient data. With the general mode in which the argument is conducted, we have little fault to find. Occasionally, however, Lord Ormathwaite's zeal on behalf of orthodox theology betrays him into injustice, as when he says:—"There is one feature common to the writings of Mr. Darwin and Mr. Buckle which is to be regretted—they both of them seem to ignore, if they do not altogether deny, the existence of a First Cause. Secondary causes are always with them the only springs of motion." With this we may contrast the following sentence from the "Origin of Species":—"To my mind it accords better with what we know of *the laws impressed on matter by the Creator*, that the production and extinction of the past and present inhabitants of the world should have been due to secondary causes, like those determining the birth and death of the individual." Lord Ormathwaite pleads bodily infirmities as an excuse for any inaccuracies or mistakes in the book, and we very willingly allow the plea.

The Use and Origin of the Arrangements of Leaves in Plants. By Chauncey Wright. (American Academy of Science and Arts.)

THIS is an elaborate and ingenious attempt to apply the principles of Natural Selection, or the Survival of the Fittest, to the observed phenomena of Phyllotaxy, or the arrangement of leaves on the stems in plants. Stating in the outset very clearly the distinction between this theory, according to which every organ, and every arrangement of organs, must be of some practical (though possibly undiscovered) utility to the plant, and that of "types," which requires no such hypothesis, Dr. Wright proceeds to investigate how the origin of the phenomena under investigation can be accounted for on the former theory. It must be assumed in the outset that the two principal modes of the arrangement of foliar organs, of which all others are modifications, the spiral and the verticillate, are modifications of a single original type. Investigating the actual arrangements on mathematical principles, he finds that the various angular distances of leaves on the stem are resolvable into the general form of the continued fraction

$$\cfrac{1}{a+\cfrac{1}{1+\cfrac{1}{1+,\ \&c.,}}}$$

in which a may have the values 1, 2, 3, or 4. The actual fractions thus resulting are when

$a=1$ . . $\frac{1}{2}\ \frac{2}{3}\ \frac{3}{5}\ \frac{5}{8}\ \frac{8}{13}$, &c.
$a=2$ . . $\frac{1}{2}\ \frac{1}{3}\ \frac{2}{5}\ \frac{3}{8}\ \frac{5}{13}$, &c.
$a=3$ . . $\frac{1}{3}\ \frac{1}{4}\ \frac{2}{7}\ \frac{3}{11}\ \frac{5}{18}$, &c.
$a=4$ . . $\frac{1}{4}\ \frac{1}{5}\ \frac{2}{9}\ \frac{3}{14}\ \frac{5}{23}$, &c.

each fraction being obtained by adding together the numerators and denominators in the two preceding fractions. Practically it is found, however, that certain only of these fractions occur in nature, while of those that are found some are much more frequent than others. The approximate ultimate value (k) of this continued fraction, when $a=1$, is 0·6180, k possessing the property that any power is equal to the difference between the two next lowest powers, or $k^n = k^{n-2} - k^{n-1}$. On this peculiar arithmetical property of k depends the geometrical one of the spiral arrangement which it represents, namely, that such an arrangement would effect the most thorough and rapid distribution of leaves around the stem. The latter part of this valuable paper consists of an attempt to show that the modes of phyllotaxy which result from the use of the different forms of the fraction are either directly serviceable to the plant by affording the best distribution, either for absorbing the sap from the roots or for exposing it to the action of air and light, or have been so at some period of the ancestry of the plant, when its structure was of a simpler character. The typical or unique angle of the theory of phyllotaxy the author regards to be the goal towards which the special forms tend, by the action of the principle of natural selection, rather than as the origin of the spiral arrangements. A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Law of Total Radiation

In Nature for April 25 Captain Ericsson attacks the calculation of Pouillet as to the sun's temperature, as being founded on an erroneous law of radiation. Had he contented himself with saying that the extension of Dulong and Petit's law so far beyond its experimental foundation to temperatures approaching that of the sun was "mere theory," and inconsistent with his own experiments, his position might have been impregnable. But not satisfied with this, he goes on to question the applicability of Dulong's law even below the boiling point of mercury, and asserts that Newton's law is much nearer the truth. The only objection that he gives to the method of the French experimenters is that they erroneously confuse the *surface* temperature of their thermometers with the average temperature of the contained mercury. The observed radiation is really due to the first, though attributed to the second. Now, without asserting that the objection is entirely without force, I submit that, if Newton's be the real law of radiation, it is impossible in this way to account for the observations.

In the first place, if the rate of cooling for a body at a variable temperature t, surrounded by another at a fixed temperature t_0, be proportional to $t - t_0$, it follows from the theory of exchange (than which there are few things better established) that the radiation between two bodies at any temperatures t, t', is proportional to $t - t'$. The rate of cooling of the thermometer contained in an enclosure would thus depend only on the *excess* of temperature, in flat contradiction to Dulong's observations. Nor would this result be altered, even though the material of the thermometer were so badly conducting in relation to its size as to allow the surface temperature to fall considerably below that of the interior. Whatever may be the relative temperatures after a given time of a system composed of a conducting mass, originally at a uniform temperature of 100°, surrounded by an enclosure maintained at 0°, the same after the same time will be the differential temperatures of the corresponding parts of another similar system, whose interior mass had originally a uniform temperature of 200°, with a case maintained at 100°. In fact, according to Newton's law, and with a constant conductivity, the superposition of any constant temperature over the whole system alters none of the conditions.

If it be objected that in the interior of a thermometer heat is distributed, not merely by conduction, but is convected by currents in the mercury, even this, I believe, will make no difference. The convection currents are a consequence of differences of density, and these are approximately proportional to the differences of temperature. The addition of a constant temperature to the whole alters nothing.

Judging from the evidence at present before us, it is impossible to avoid the conclusion that within the limits of Dulong's experiments Newton's law of cooling cannot be even approximately true. If Capt. Ericsson, by bringing forward fresh experiments, and by proving the fallacy of old ones, can establish the truth of Newton's law, he will lay Science under a great obligation. Speaking as a mathematician, I could even wish him success.

With regard to high temperatures it seems certain that Dulong and Petit's law fails; for it is impossible to believe that the sun is no hotter than 1,500° Cent., at least if the estimates hitherto made of terrestrial temperatures are substantially correct. It must be remembered, however, that according to Fizeau the sun is only about 2½ times brighter than the electric arc, which does not even imply a higher temperature; because, while the sun must give us nearly the whole radiation due to his temperature, the electric arc is probably transparent.

J. W. Strutt

Terling Place, Witham, April 29

Solar Halo

This morning, at 9.20, I observed a strongly-marked halo round the sun. Roughly extemporising a sextant with a post-card and paper-vector, I took three observations on the semi-diameter, and found the mean to be 22°·6. So I conclude this to be the ice-halo, whose deviation is 23°, being formed of hexagonal crystals. Two facts render the halo noteworthy—(1) the morning (after a heavy gale from the south) was exceptionally warm; (2) the halo exhibited the extreme colours in the proper order. I am told halos do not exhibit colours. Surely they ought to; and if not, why not? Let some of your readers answer me this. The halo was visible till nearly 10 o'clock.

Bournemouth, April 26 C. M. Ingleby

Help us to save our Birds

All praise be given to those who have made a stand for the preservation of British birds. With a spirit of patience they have had to encounter the crass prejudice that sometimes saturates even the rural mind, and to prove that if the small bird takes its toll from, it also greatly assists in preserving the store of, the farmer. They have had to combat the sporting instincts of the excited townsman, so joyous with his escape from the smoky labyrinths of his brick-built prison that even a feathered shuttlecock would almost seem like game. Last and greatest feat, they have had to question the right to worship the national idol—gain, and to teach people, that even if, by the wholesale slaughter of feathered tribes, some persons scraped up gold, still that occupation, however praiseworthy, was against the general good. Truth at last dawned on the mind of the people, and so Parliament shielded, amongst other fowl, the pretty kittiwake from destruction, and preserved fashionable women from one more barbarism.

Those who have thus worked to educate the public need not rest on their oars for lack of employment, let them look farther afield, let them fearlessly step across national boundaries, and lend their strength to assist in arresting the impending destruction of many species of the most beautiful and interesting orders of animated nature in any quarter of the globe.

New Zealand, so long left by science to slumber on the calm bosom of the Pacific, has disclosed, amongst her birds, forms that have surprised the naturalist as much as they have excited the speculation of the philosopher. The remains of birds, of orders other than the gigantic Struthiones, giving us hints about strange *lost* forms of animal life that have lingered in these islands, perchance, almost to our own times, are now and then exhumed from the hidden shores of swamp and morass. We raise a cry for help in behalf of the mass of birds that yet remain *near* us (we had almost said *with* us), in the hope that the attention of naturalists in Europe may be called to the peril of extermination that hangs over many interesting indigenous species. For the preservation of our birds we require some assistance from abroad, our time is so crowded with occupations of many kinds, that without some pressure from without, little attention would be likely to be paid to the subject. This is said not without reason, not without some experience; in 1868, in Parliament, the writer tried to secure the conservation of our magnificent forests, a resolution was passed by the House to that effect, official inquiries were made;—*cui bono?* Our forests are now being damaged and destroyed, where not protected by climate, in so ruthless a manner, that no further evidence is needed to prove our wasteful style of settlement. Will not some one having authority in such matters speak a word in due season for our birds? I believe nearly every living species that we number could be preserved with proper care. If that is a fact, is it not interesting enough to naturalists to induce them to stimulate us to efforts more likely to give better results than our present legal enactments?

We encourage planting, the labour and capital therein expended may yield returns after the lapse of generations; we, at the same time, allow timber, the growth of ages, to be swept by fire by any one who owns a box of matches, and looks on firing as the best means of subduing the wilderness. We import with great difficulty insectivorous birds, and allow the Apterygìnæ and other insectivorous genera to be destroyed without mercy.

Fearing to occupy too much space, I will only glance at our worst raptorials from which our birds suffer. First, the *bonâ fide* settler in his "new chum" phase, before "he has eaten his tutu" (as we say); next, the digger, who kills kivi, kallapo, kalla, and pigeon, without any respect to season; his dog, like that of the settler, being a more fatal enemy to birds than himself. Lastly, the collector, the provider of rarities for museums, &c. There is no fence month with him; if spring or summer plumage is interesting, so also is that of winter; eggs, young, the adult, alike he preys on all. He is heedless of the Mosaic promise; he cares not to have his days prolonged, so that he gets good specimens.

Could we be persuaded to try and avert what will some day be a great reproach to this country, the destruction of so many species of our feathered tribe, D'Urville's Island might be found most useful. Wingless species, and birds of feeble powers of flight, might there find a refuge for some of their representatives. Resolution Island might be placed under tapu from molestation by dog and gun. THOMAS H. POTTS

Ohinitahi, New Zealand, February 2

The State and Science

FROM the position taken by Mr. Gladstone with regard to the Dublin University Bill, from Mr. Lowe's speech at Halifax, and from other indications, it would appear to be the policy of the Government, not to render accessible to all, without sectarian distinction, the professorships and other endowments of the Universities, and to assign to modern culture a fair share thereof, but to abolish all such endowments, and to withdraw all State aid from both literature and science. In favour of such a policy it has been urged not only that it is in accordance with sound political economy to leave every pursuit to seek for itself its own reward, but also that the system of endowment and artificial aid has proved a signal failure. Now, if by this last argument it is meant that the large rewards which have been given for classical knowledge and for mathematical attainments have not been productive of numerous Bentleys, Porsons, and Newtons, the truth and validity of the argument must be admitted The rewarding of mere acquired knowledge was little likely to show its results in original work. The capacity for acquisition, literary, mathematical, or scientific, is a very different thing from the power of original production, or of extending the boundaries of human knowledge. Probably in some cases the latent spark of genius has even been stifled and smothered by the load of "cram" necessarily superimposed to meet the requirements of exacting examiners. It would be, however, I think, a mistake not to allow some considerable reward to more exact knowledge. But it is with regard to original work and the proved capacity for doing it that external encouragement and reward is absolutely necessary. Such work, in most departments of literature and science, cannot possibly, in a commercial sense, pay. It is this work, however, which confers especial honour and advantage on the State. Therefore it is in accordance alike with justice and sound policy that the doing of such work should be munificently encouraged and rewarded by the State. Such a policy might be reasonably expected to issue in results very different from those which have attended the endowment of "cram." Previous failure cannot be objected, for the attempt can scarcely be said to have ever been made. It behoves, therefore, literary and scientific men to look to it that, in any redistribution of the University or other endowments, the true interests of both science and literature—and especially the encouragement and reward of original research—are duly regarded by the Government. But a certain superficial political economy may object that such a policy would be of dangerous tendency, inasmuch as it would recognise the existence of the State as a unity, which, being honoured and benefited, should encourage and reward. The man of original thought and the discoverer of Nature's secrets must be left, each for himself, to seek such recompense as he can in the ordinary market. Sir Isaac Newton would not be rewarded by the present Government with the Mastership of the Mint. They have abolished that office. No, they would guard him in his enjoyment of the copyright of the "Principia!" Such, it would seem, according to Messrs. Lowe and Gladstone, is the dictate of common sense, of justice, and of the "sound political economy" of ADAM SMITH

Brilliant Meteor

I NOTICED in your number for last week the account of a brilliant meteor, observed in Cumberland on April 19. Now I had reported to me a very similar meteor at nearly the same time (about 8.40 P.M.), an account of which I forwarded, with my other results of the night's watch, to Mr. A. S. Herschel, who would gladly receive any further report of the same; unfortunately, I have not that number of NATURE at hand, and therefore cannot make a personal application to your correspondent. On the same evening, about 10.7, I myself saw an exceedingly brilliant meteor, which fell to a point just S. of Vega. It is curious that both of these come from the radiant situated about R.A. 155, D + 47, or rather from one of the group of radiants there situated, M_8 of Heis, 56 and 52 of Schiaparelli. It would be an interesting point of investigation whether the meteors from that radiant are of peculiar brightness.

20, Bootham, York, April 30 J. EDMUND CLARK

EXPERIMENTAL ILLUSTRATIONS OF MUSICAL TONE

REFERENCE was recently made in these columns to an educational lecture on "Musical Tone," delivered by me on March 14, at the Taunton College School. Among the experiments, several were specially arranged in connection with this lecture, and these I should be glad to put on record as simple and inexpensive means of illustrating important points.

For the purpose of displaying the relation between the "quality" of musical tones and the kind of vibration producing them, a series of magic-lantern slides were shown. These were prepared in the ordinary way, being smoked glass plates on which vibration-lines were traced by points attached to tuning-forks, piano-wires, &c. Each tone being sounded as its vibration-line was shown, the audience was enabled to appreciate clearly the difference between the simple tone of the tuning-fork and the clangs of a stringed instrument, played on musically and also made to shriek and rasp. For an extreme illustration, to show the relation of an irregularly discordant clang to an irregularly bent and jagged vibration-line, a toy popularly known as a "Bismark's Whistle" was made, larger than the usual size. It consisted of a tin-plate canister, near the centre of the bottom of which a piece of gut, knotted at the end, was passed through a small hole. Well-resined fingers drawn with a tight grip along the gut caused this infernal machine to emit a hideous sound, the vibration-line of which was shown as taken off on the smoked glass from a pointed wire soldered to the bottom of the canister.

The pictorial representation of a beat is of course indispensable to explain Helmholtz's theory of harmony and dissonance. As, however, neither the plan used by Prof. Helmholtz of taking off the beats of two organ pipes by means of a vibrating membrane, nor the splendid arrangement of Lissajou's method employed by Prof. Tyndall, were readily available, I found it necessary to contrive a simpler and coarser method. Accordingly, two stout piano-wires were stretched side by side on a board about three inches apart, and connected near the middle by a bent yoke of thinner wire, terminating upwards in a point. The two wires being tuned so as to give beats at a convenient rate, the alternate phases of addition and subtraction of the vibrations of the two wires, corresponding to the beats, were well shown by the central pointer, from which pictures of the beats were taken off on the smoked glass, forming admirable slides.

Mr. R. Knight, of Wellington, who superintended the making of the apparatus, devised a neat arrangement for showing the lengths of resonant tubes (See Tyndall, Lecture V.). It consists of a 4-ft. length of 3-inch zinc tube mounted upright on a foot and nearly filled with water, so as to form a cistern in which a 2-in. tube is raised and lowered, answering at the proper height with powerful resonance to a large tuning-fork. This apparatus will, I expect, come into use in future as the most convenient means of demonstrating the principle of lengths of organ-pipes. When the instrument is used for class purposes, it may be recommended that the tube be graduated to quarters of an inch, so that the pupils may be practised in calculating the wave-length, and thence the pitch, of any tuning-fork tested by the resonant tube. Further, with reference to the theory of musical pipes, it may be worth while to mention that an 8-ft. length of ¾ in. iron gas tube serves well to produce the overtones of open pipes. It is best to fit some kind of trumpet mouth-piece at one end, by means of which the most elementary musical scale, that of simple trumpet-music, may be effectively given.

No ready way being found of displaying Sir. C. Wheatstone's kaleidophone experiments on a large scale, they were shown afterwards as table experiments. Since then, however, a Chinese joss-stick has supplied the means of showing to an audience the path of the end of a vibrating rod. A piece of the lighted stick attached to the end of an umbrella rib shows beautifully convoluted figures several inches across. Any other means of attaching a bright permanent spark may of course be used, and the plan serves also to show the path of a point on a long vibrating wire. The experiment of waving a large tuning-fork to and fro while in vibration, which Mr. Sedley Taylor described in NATURE, vol. v. p. 321, had also been noticed by us. For want of means of making the result visible at a distance, it was not shown in the lecture. An inch of lighted joss-stick, however, fixed transversely near the end of one leg, shows well the contrast between the line of light traced by waving the fork in its quiescent state, and the series of dots of light into which this line is resolved when the fork is waved or swung while in vibration, its counteracting movements bringing it to momentary rest.

E. B. TYLOR

ON THE SULPHUROUS IMPURITY IN COAL GAS*

THE lecturer commenced by stating the origin of the sulphurous impurity in coal gas to be the iron pyrites which is contained in coal, and that in the manufacture of gas, when the coal is strongly heated, the sulphur of the iron pyrites not only combines with hydrogen to form the gaseous sulphuretted hydrogen, but also with carbon, to form the very volatile liquid bisulphide of carbon. Little need be said of the desirability of removing the sulphur from coal gas, for in many of our large libraries, such as that of the Athenæum Club, the injurious effect of the sulphurous and sulphuric acids produced by the combustion of gas containing sulphur, seems to be plainly manifest, more especially on the leather binding of the books. The gas, after leaving the retorts in which the coal is heated, is cooled down, and passed through towers filled with coke, over which water is kept trickling. By these means a considerable proportion of the sulphur is removed in the form of sulphide of ammonium. It was shown by experiment that this washing with water could only be employed to a limited extent; as by excessive "scrubbing," as it is technically termed, the gas is greatly deteriorated as to its illuminating power. The sulphuretted hydrogen remaining in the crude gas is easily removed; but the removal of the bisulphide of carbon is attended with so many difficulties that up to the present time no satisfactory process has been devised to effect this purpose. The lecturer exhibited strikingly the two methods used for the removal of the sulphuretted hydrogen, one by passing the gas over lime, and the second by passing the gas over oxide of iron, and stated that it is comparatively rare to find any of this impurity in the gas as supplied to consumers. Up to the present time no process is used for the removal of the bisulphide of carbon. Mr. Harcourt has, however, found that by heating a mixture of bisulphide of carbon vapour and hydrogen to redness, the former is decomposed into sulphuretted hydrogen. It will be thus seen that the removal of the bisulphide of carbon from coal gas is rendered possible, for by simply heating the gas to redness the sulphur combines as before with hydrogen to form sulphuretted hydrogen, which can be easily removed by passing through a purifier containing oxide of iron. In this way, by passing coal gas, which contained 30 grains of sulphur in 100 cubic feet, through a red hot tube, and then through an iron purifier, the sulphur was reduced to about 5 or 6 grains in 100 cubic feet. It might be imagined that the passage of coal gas through a red hot tube would deteriorate its quality; but Mr. Harcourt's experiments show that the contrary is the case, for by passing gas of 14·91 candles rapidly through a tube heated to moderate redness, the illuminating power was found to be 15·1 candles, and after passing through a tube heated to bright redness, its illuminating power was increased to 16·66 candles. A parallel case to this is seen when marsh gas is decomposed into hydrogen and carbon by a series of electric sparks, the gas which is obtained occupies almost twice the original volume of the gas, but possesses a far greater illuminating power than that of the original marsh gas, owing to the presence of a small quantity of acetylene or some such body. It will be seen that these experiments offer what certainly seems to be a feasible process for the great reduction of the amount of sulphur contained in coal gas.

PROPOSED OBSERVATORY IN NEW ZEALAND

ON Dec. 16, 1850, the first ship-load of emigrants, under the auspices of the Canterbury Association, landed at Port Lyttelton and commenced the foundation of the present province of Canterbury. On Dec. 16, 1871, the settlement attained its twenty-first year, and it was felt by a large number of gentlemen here that it would be well to celebrate the majority of the province by some permanent memorial. A meeting was held on that day, attended by a number of the most influential residents; and it was unanimously resolved to form an association for the establishment of an astronomical observatory near Christchurch. It was remarked by several speakers that this province possesses considerable advantages for such an institution. Between the ocean on the east and the great range of the Southern Alps on the west, there stretches an expanse of unbroken plain more than 100 miles in length and 50 in breadth. The remarkable clearness of the atmosphere, joined to this large extent of level land, renders it possible to observe a much larger area of the heavens than is usually the case. The meeting fully endorsed the remarks of the promoters of the movement; a temporary committee was formed, and lists of subscribers and donors were at once commenced.

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* Abstract of a Lecture delivered at the Royal Institution on February 19, by A. Vernon Harcourt, F.R.S.

By a curious coincidence, the telegram announcing the formation of the Society no sooner came under the notice of the Colonial Government, than they informed the committee that they had, a short time before, received a communication from the Imperial authorities on the subject. The Astronomer Royal had intimated his desire to form a station in New Zealand for the observation of the Transit of Venus in 1874, and had recommended Canterbury as suitable for the purpose. This announcement was naturally most encouraging to the committee; steps were immediately taken by them to communicate, through His Excellency the Governor, with the Imperial Government. The Governor and his advisers have informed the Society that they cordially approve of its object, and will assist it as far as possible.

The Provincial Council of Canterbury was in session a few days later. Petitions were presented to it by the Society, praying for a grant of 1,000*l.* towards buildings, &c., and 5,000 acres of the waste lands of the province as an endowment towards the permanent maintenance of the Observatory. The Council, by a majority of twenty-five to eleven, voted the sum of 1,000*l.*, and 200*l.* for a site; but they declined at present to grant the endowment in land. The money grant of 1,200*l.* was likewise made conditional on the agreement of the Colonial Government to undertake the maintenance of the Institution. The great interest taken in the movement by His Excellency the Governor and his Ministers leads the Society to hope that a satisfactory arrangement may be made, so that before this time next year we may be placed in a position actually to commence operations.

Although only a month has elapsed since the first meeting, the Society has been warmly supported in this province. Additional subscriptions are received every day, and since January 22, when the temporary committee presented their report, the number of annual members has been increased to nearly 200.

I enclose the report of the temporary committee, which details the work actually done by them so far. You will see that the committee have attempted to commence the practical work of founding the Society by asking the Astronomer Royal to send out a gentleman to advise as to a proper site and other initiatory work. It is our hope that our object may receive a cordial concurrence from scientific men at home. We desire to co-operate with similar institutions in the old world by performing work which may not only be of practical use to our community here, but may, if possible, add something, however small, to the results of labours of older workers in the field of science. We trust that even one of England's youngest daughters may be of some assistance in this respect to her more favoured sisters.

W. M. Maskell, Hon. Sec.
Observatory Society of Canterbury.

A NEW MODE OF TAKING CASTS

Mr. Boyd Dawkins, F.R.S., has recently exhibited to the Manchester Philosophical Society a number of casts in plaster of Paris of various objects of natural history, and explained the process by which any one can make them for himself. The material of the mould is artists' modelling wax, which is a composition akin to that which is used by dentists. And as it becomes soft and plastic by the application of heat, though in a cold state it is perfectly rigid, it may be applied to the most delicate object without injury. As it takes the most minute markings and striations of the original to which it is applied, the microscopic structure of the surface of the original is faithfully reproduced in the cast. The method is briefly this:—1. Cover the object to be cast with a thin powder of steatite or French chalk, which prevents the adhesion of the wax. 2. After the wax has become soft either from immersion in warm water or from exposure to the direct heat of the fire, apply it to the original, being careful to press it into the little cavities. Then carefully cut off the edges of the wax all round, if the under cutting of the object necessitates the mould being in two or more pieces, and let the wax cool with the object in it, until it be sufficiently hard to bear the repetition of the operation on the uncovered portion of the object. The steatite prevents the one piece of the mould sticking to the other. The original ought to be taken out of the mould before the latter becomes perfectly cold and rigid, as in that case it is very difficult to extract. 3. Then pour in plaster of Paris, after having wetted the moulds to prevent bubbles of air lurking in the small interstices, and if the mould be in two pieces, it is generally convenient to fill them with plaster separately before putting them together. 4. Then dry the plaster casts either wholly or partially. 5. Paint the casts in water colours, which must be *fainter* than those of the original, because the next process adds to their intensity. The delicate shades of colour in the original will be marked in the cast by the different quantity of the same colour which is taken up by the different textures of the cast. 6. After drying the cast, steep it in hard paraffin. The ordinary paraffin candles, which can be obtained from any grocer, will serve the purpose. 7. Cool, and polish the cast by hand with steatite. The result of this process is far better than that obtained by any other. The whole operation is very simple, and promises to afford a means of comparison of natural history specimens in different countries, which has long been felt to be a scientific need. It has been already introduced into America and India by Mr. Dawkins, and samples of the casts are to be seen in the British Museum, as well as in that of the Geological Survey, and of Oxford, and of the Queen's College. Casts of type specimens may be multiplied to any extent at a small cost of time and money, and are as good as the original for purposes of comparison, and almost as hard as any fossil.

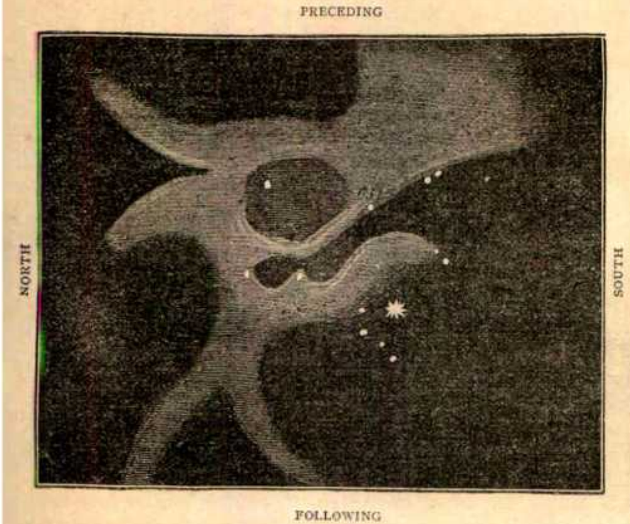
The modelling wax can be purchased from Messrs. Lechertier, Barbe, & Co., artists'-colourmen, Regent Street.

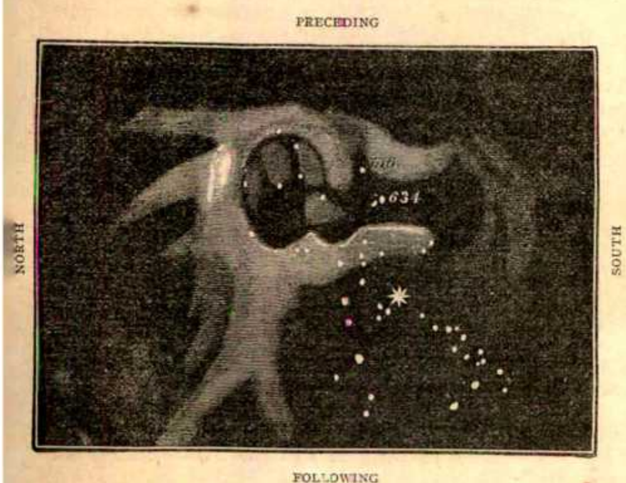
THE NEBULA ROUND η ARGUS

A paper, accompanied by five drawings, has recently been read to the Royal Society of Victoria by Mr. McGeorge, on the star η Argûs and the great nebula near it, from which we select a few statements which appear to establish the occurrence of sensible changes in that region of the sky. After noticing the value of Sir John Herschel's drawing and description of the nebula, Mr. McGeorge remarks that from 1838 to 1869 no trustworthy observations of the nebula could be made, for want of instruments of sufficient power; though Mr. Tebbutt and others have contributed valuable information about η itself. A single glance is sufficient to show the complete inapplicability of Herschel's drawing or description to the present appearance of the nebula in the Great Melbourne Telescope. This, however, might be attributed to the great power of the instrument, whose light exceeds that of the 20-ft. reflector as much as that surpassed the other telescopes with which the nebula has been observed.

But the changes indicated in the present paper cannot be so explained, for they have nearly all been traced in the *Great Melbourne Telescope itself*, and are described in detail, with reference to drawings and observing notes. A few may be mentioned here.

η, which Herschel saw involved in dense nebula, was in April 1869 seen on the bare sky, the nebula having disappeared for some distance around it. Drawing No. 1, which accompanied the paper and which is here reproduced, then made, shows that the southern loop of Herschel's Lemniscate had bulged out into the vacuity, forming an isthmus which trended north and joined the northern loop. The *second* drawing, January 1870, shows that within six months this isthmus had detached itself from the north side of the Lemniscate, through 90″, to form a broader peninsula. The third drawing, April 1870, shows the outline of a gulf or cleft, commencing at H 634. This star is one of the "landmarks" described in 1838 as being "near the margin of the Lemniscate." It is now in mid-channel. These three drawings were made by Mr. Le Seur. No. 4 was made by Mr. McGeorge at the close of December in the same year. It confirms the existence of the Gulf; another of the "landmarks," H 616, is now nearly clear of nebula. A promontory shown in No. 2 has detached itself to form an island of nebula with a starry nucleus. This and the neighbouring outline of the Lemniscate have the same hard definite outline given by Herschel; at one part, however, the outline seems encroaching upon the Lemniscate, and leaving an oval patch of thinner nebula; southward the outline seems to be curdling and breaking up. A fifth drawing, which is also here given, was made in June 1871, which amongst other changes shows that the island has shifted; the nucleus is now detached from it, and proves to be a triple star.





Mr. McGeorge finds, as Lord Rosse did, that high powers on a good night bring out details of nebula invisible with lower powers. On one occasion he speaks of using 1,300, whose definition, he says, "was magnificent for an hour." He notices a sort of stereoscopic effect which, particularly with the high powers, makes the Lemniscate look "like a huge snowy cave with uneven woolly sides."

In December 1869 the spectrum of η Argûs showed bright lines; but in January 1871 there was no trace of them; but Mr. McGeorge thought that with a wide slit he detected absorption bands in the position of the usual nebular lines. Distinct nebulosity was then visible round the star, most condensed near it, chiefly in the direction of the Lemniscate.

It is perhaps unnecessary to remark that Mr. McGeorge has seen nothing in the way of coloured stars at all to be likened to κ Crucis; one or two are reddish.

It is the intention of the Melbourne astronomers to pursue unremittingly the study of this nebula, which seems already to have given them such remarkable results. But it is evidently a most laborious task which they have imposed on themselves. At present they are confining their attention to the vicinity of the Lemniscate, but even there the field of labour is immense, for already they have noticed three times as many stars as were seen by Herschel.

They have with them the best wishes and sympathy of all astronomers. T. R. ROBINSON

Armagh, April 21

BRITTANY DOLMENS AND LINES

MR. JAMES FERGUSSON, in his interesting volume on "Rude Stone Monuments in all Countries,"[*] which will doubtless become a text-book on that section of archæology which pertains to Megalithic structures, has made one or two unintentional misstatements, discrepancies, or errata, which perhaps he will allow me to correct through your columns, in hopes that they may be in time for the second edition, which is probably called for, if not accomplished. I will state them as briefly as possible.

I. Carnac (p. 349): "No stone in the neighbourhood of Carnac is hewn or even fashioned beyond splitting, and *no sculptures of any class have been traced*" (italics are mine). Will Mr. Fergusson forgive me if I point out that the tumulus of Kercado, situate in the grounds of the Chateau of the same name, and marked in the map of the neighbourhood of Carnac given in his volume Fig. 135 as "Kercadio Tums. 2" has well marked sculpture on at least three of its stones, one of the figures, viz. that on the under-surface of the capstone, being evidently of the same type as the *Hatchet (?)* in the roof of Dol-au-Marchand or Table de César, see Fig. 149 (where by-the-bye I never could make out the so-called plume), and is identical with one in Bé-er-groah (Locmariaker). This tumulus, or dolmen-mound, as I prefer to call it, is much nearer to the lines of Kermario and Kerlescant than Mont S. Michel is to the lines of Menec. I should add that M. René Galles figures two of the sculptured stones, but not the hatchet.

But this is not the sole example of sculptured stones in the neighbourhood of the Carnac amorpholiths. In the curiously arranged dolmens called the "Grottes de Kerozille," are distinct traces of former sculpture (in which, if I mistake not, some traces of some coloured pigment have been discovered by W. Lukis), of which M. Galles gives but imperfect representations. Doubtless all the stones were covered with similar ornamentation, which has disappeared from the weathering of the stone surface. The "Grottes de Kerozille" are situate to the north of Menec, about two miles distance, marked Dols. 11 and 12 in MM. Blair and Ronald's map as given by Fergusson. There are in reality three dolmens, the centre one at right angles to the other two, and almost connecting them with traces of a fourth: all have been covered under one mound. In "Les Grottes de Plouharnel," where the gold ornaments were found, are traces of rude sculpture. I need hardly add that none of these archaic markings are in relief, as is the case with the celts shown in Sir Henry Dryden's drawing from Gavr' Innis, Fig. 152. Flowing labyrinthical lines seem characteristic of the Kerozille dolmen-mound, whilst straighter lines forming network are peculiar to Kercado. On the summit of the neighbouring dolmen of Runusto are some cup-markings which bear a very fair resemblance to the constellation of the Great Bear together with the Pole Star. The *tolmen entrances* in the long barrow close to and north of the Kerlescant alignments, one of which is figured (*vide* Fig. 139), were doubtless fashioned artificially, at least I think Mr. W. Lukis, who has described them, will bear me out in this assertion. It is indeed a monstrous pity that the Société Polymathique du Morbihan should have permitted such an interesting structure to be destroyed.

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[*] *Vide* NATURE, Vol. v. p. 386.

I should not have taken the trouble of bringing the foregoing notes to the notice of the public in your pages had it not been for the great stress laid by Mr. Fergusson on the fact of the marked distinction made by him between the Locmariaker monuments and those in the neighbourhood of Carnac, the latter of which he asserts *cannot* be dissociated from the Carnac alignments.

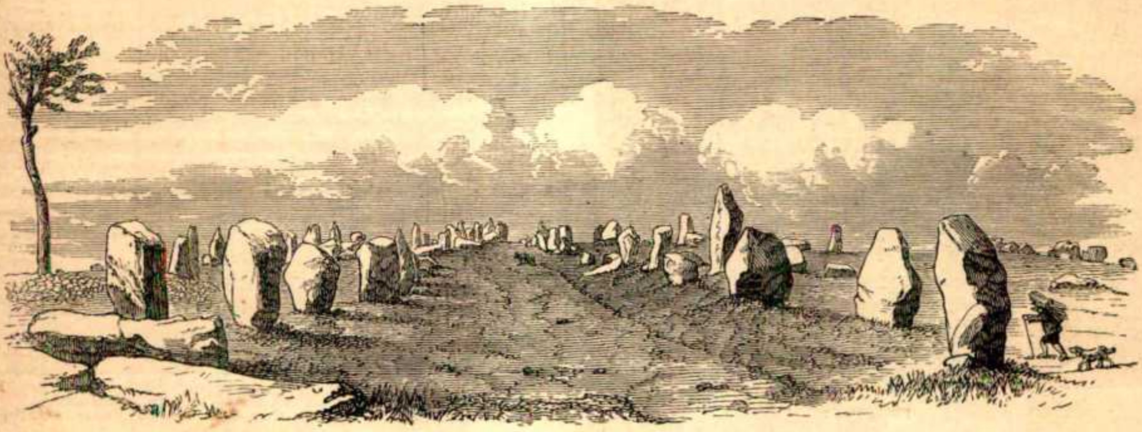

FIG. 1.—ALIGNMENTS OF AMORPHOLITHS, KERMARIO.
From Sketch by Capt. S. P. Oliver, R.A.

In a communication addressed to the Anthropological Institute, I endeavoured lately to prove that the hewn and sculptured stones of Locmariaker were of a different type from the rough and shapeless blocks of Carnac, which latter I ventured to distinguish by the name of "Amorpholiths," and for that very reason disassociated the dolmen-mounds, such as Kerlescant, Kercado, and Mont S. Michel, from the lines and avenues, excepting the *unchambered* barrow at the western extremity of the Kerlescant lines which appear to lead up to it. An endeavour to classify the Dolmen-mounds of Brittany is appended herewith.

II. As to the fallen menhir, which Fergusson asserts *belongs to* the dolmen named *Dol-au-Marchand*, and which, in his idea, was in reality two *obelisks* and not one; the accompanying trustworthy plans and elevation of the renowned monolith ought to prove to the most sceptical that the remains in question are without doubt fragments of one huge monolith, which was, moreover, artificially fashioned, and, possibly, originally actually polished.

I confess that I was disappointed when I found that the Carnac lines were summarily disposed of by Fergusson in nine pages, whilst over fifty are devoted to Avebury and Stonehenge! (although he terms the former the most remarkable group of megalithic remains, not only in France, but perhaps in the whole world); also at not finding a single illustration of the said lines beyond the maps, which, valuable as they are, give no idea to the reader who has not actually visited the spot, of the size and style of the amorpholiths. I enclose a view of the Kermario avenues, looking west, premising that the more distant stones are the largest, and that they decrease in size towards the foreground, the perspective diminishing the effect of this difference in the size of the stones.

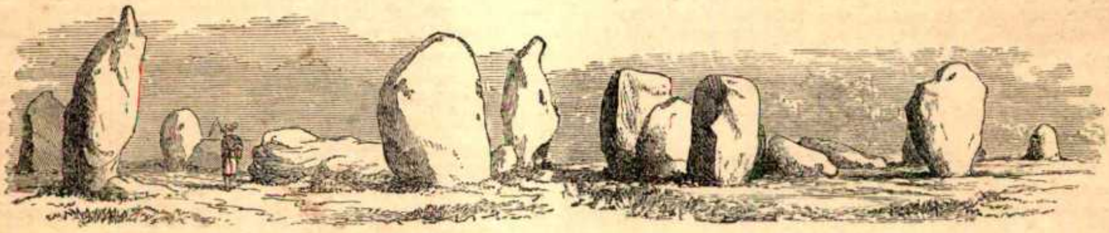

FIG. 2.—PORTION OF THE KERMARIO ALIGNMENTS FROM THE NORTH.
From Sketch by Capt. S. P. Oliver, R.A.

III. Why does the celebrated dolmen (Fig. 126) of *Kercouno* appear under the name of *Krukenho?* Louis Galles gives the etymology of this place as "*village du souvenir.*"

IV. Not much faith can be placed in Mahé's (not Malé) representation of an *ideal* demi-dolmen, Fig. 129. In his "*Antiquités du Morbihan,*" it is ideal, and has no local habitation or name.

V. P. 349. The *Veneti* are styled *Venetes*, and p. 356 *fibrolite* * is printed *tribolite*—printer's errors! With regard to this last rare mineral substance, to which Fergusson never alludes beyond mentioning its existence, I pointed out some time since that seventy-five per cent. of the celts found in the sepulchral dolmen-mounds of Brittany were composed of this material.* Can Mr. Fergusson inform us how he accounts for the presence of this substance, as well as the significance of its predominance in association with sepulture? It may be noted that all the fibrolite celts are small and nearly perfect, with sharp edges, and show no signs of use; whilst the diorite and other celts of a larger type show evident signs of use and are all *purposely* broken before deposition in the dolmen.

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* La fibrolite est un silicate anhydre d'alumine; elle doit par ses caractères être rattachée à la sillimanite (des Cloizeaux). Couleur blanc-laiteuse, souvent jaunâtre et marbrée de veines et de tâches grises ou couleur de rouille. A peu près opaque; quelques échantillons montrent une certaine translucidité.

VI. P. 356. More strictly the jade and turquoise should be termed *jadéite* and *callais*, respectively. Mr. Fergusson is right in quoting "jade" and "turquoise" if he goes by M. René Galles' account, and I quoted the same materials myself in an article on Dolmen Mounds in the *Quarterly Journal of Science* last January; but these same articles now in the museum of Vannes are described by the curators Messrs. de Cussé, L. Galles, and D'Aultdumesnil as *jadéite*† and *callaïs*.‡

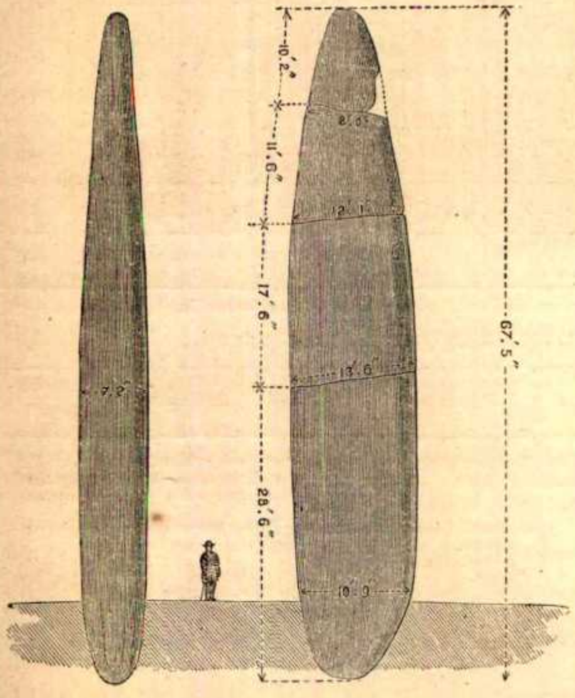


FIG. 3.—LE GRAND MENHIR, Restored by Sir Henry Dryden, Bart., showing its similarity to a Stone Celt.

VII. How the barrow north of Kerlescant lines can be said to be related in any way (p. 356) to Mont S. Michel (which is situate south-east of the Menec lines, and at least a mile distant), either in position, size, or structure, I cannot imagine. The former is, or rather was, a long but small structure with fashioned entrances, covered over with a mound which is only visible a few yards off, whilst the latter is an immense tumulus, visible from afar, covering one if not more kists of insignificant structure.

VIII. Mr. Borlase's late discoveries in the dolmen-mounds of Trevelgie Head, Cornwall, give additional reasons for supporting Mr. Lukis' theory that both dolmens and cromlechs are merely the skeletons of original chambered tumuli.

IX. P. 389. "Only one drawing of a dolmen in Portugal has as yet, so far as I know, been published."

In 1868 M. Da Costa figured twenty Portuguese dolmens, and eight stone implements found in connection with them.*

"In the Peninsula the cromlechs, when denuded, are known under the name of *Antas* (a term about which there has been much disputing, but which, after all, seems to signify ancient altars used as landmarks); those partially enveloped in the tumulus, or on the summit of a mound, are termed *Mamunhas* (corruption of Mamua or Mamôa—tumulus); and when covered in, as the *allées* and *grottes* of Brittany, they are termed *Furnas*.

"In the year 1734 over three hundred of these remains are mentioned as existing in Portugal, but in 1868 M. da Costa could only enumerate forty-two, of which twenty-eight are in the province of Aleutejo, twelve in Beira, two in Traz-os-Montes, two in Minho, whilst none remain either in Estramadura or D'Algarve.

"The largest aggregation of these antas appears to be at Contado d'Alcogulo, the property of M. Le Cocq, where there are five remaining together. The only stone implements described by M. da Costa were found here, and consist of half a dozen rude greenstone celts and a quartzite muller. With the exception of four, all the above are denuded and ruined antas; the exceptions are two furnas near Vizella in Minho, the Mamunha de Mamaltar in Beira, and the Mamunha de Carrazedo in Traz-os-Montes. This last is chiefly remarkable from the curious hollow circular mark, presumedly artificial, on one of its supports.

"There is also one curious monument mentioned, as composed of two rows of stones, near a menhir between Cepaes and Fafe, in Minho. As this is the sole description of the monument, and no dimensions are mentioned, it is difficult to judge of its composition. It may be analogous to two rows of small vertical stones in the long barrow at Kerlescant, already mentioned, or there may formerly have existed an avenue of stones. Unfortunately it appears that the monument has been destroyed, and the stones made use of in the construction of the neighbouring convent of Pombeiro."†

The above remarks are not written in a carping spirit, but offered merely as additional information to what Sir John Lubbock aptly terms "a rich and trustworthy storehouse of facts," collected with such labour and care by Mr. Fergusson during the last eighteen years.

I will refrain from analysing Mr. Fergusson's theory that the dolmens date from a post-Roman period in the present paper.

Structure à fibres fines, soyeuses, très seerées, contournées, et comme entreacées en divers sens; c'est de là que lui vient son excessive tenacité. Raie e verre et le feldspath, rayée par le quartz. Infusible au chalumeau. Les haches en fibrolite affectent plus ou moins la forme d'un parallélogramme allongé, souvent celle d'un trapèze dont la plus grande base offre un tranchant. La fibrolite étant une substance lamelleuse, toutes les haches faites de cette matière n'ont qu'une épaisseur faible et irrégulière.

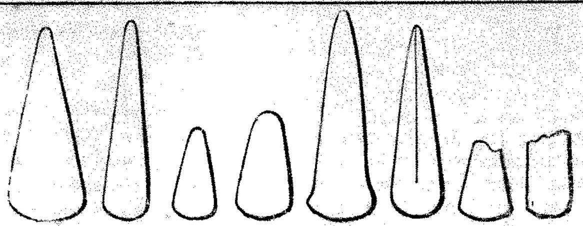
* See table.

† La jadéite doit être rattachée à la famille des wernérites (silicates anhydres). Couleur de diverses nuances de vert, de gris verdâtre, de gris jaunâtre, structure cristalline, fibro-lamellaire, quelquefois un peu schistoïde, rayant le feldspath et le jade oriental; rayée par le quartz. Très tenace. Facilement fusible. Une mince écaille, exposée à l'extrémité de la flame d'une lampe á alcool, se fond aisément en un verre jaunâtre ou grisâtre, demi-transparent.

‡ La couleur de cette matière est le vert-pomme, se rapprochant du vert de l'émeraude. Quelques échantillons sont comme marbrés de parties blanches et de parties bleuâtres; d'autres sont maculés de veines et de tâches brunes ou noires, par suite d'un mélange accidentel de matières argileuses. Le minéral est translucide, à peu près autant que la chrysoprase. Sa cassure est compacte comme celle de la cire. Il raie le calcaire, mais il est facilement rayé par une pointe d'acier. Sa poussière est blanche, infusible au chalumeau. Cette substance est un phosphate d'alumine hydraté comme la turquoise orientale, mais elle en diffère sensiblement, aussi bien par les proportions de ses principes constituents que par ses caractères. M. Damour, d'après les différences appréciables qui existent entre ces deux matières, les sépare dans la classification des espèces. Il emprunte à pline le nom de *callaïs*, qu'il applique à notre minéral, et réserve celui de turquoise à la pierre précieuse de couleur bleu de ciel si connue en joaillerie.

* "Descripçao de Alguns Dolmins ou Antas de Portugal," par F. A. Pereira Da Costa. Lisbon, 1868.

† *Vide* "The Dolmen-Mounds and Amorpholithic Monuments of Brittany," Part I., by S. P. Oliver, Capt. R.A., *Quarterly Journal of Science*, January 1872.



CELTS FROM DOLMEN-MOUNDS IN BRITTANY.

NAME OF TUMULUS.	MATERIAL.																										TOTAL.
	FIBROLITE.					JADEITE.						CHLORO-MELANITE.						DIORITE.				APHANITE.			PROTO-GYNE.		
	Type.*			Irreg.	Total.	Type.					Total.	Type.					Total.	Type.			Total.	Type.		Total.	Type.	Total.	
	A	K	L			A	B	C	E	F		A	B	C	E	F		A	B	D		B	L		B		
Mnané-er-H'roëk (*Locmariaker*).	1	58	31	1	91	4	...	...	2	2	8	...	1	...	...	1	2	...	2	...	2	...	...	...	1	1	104
Mont S. Michel (*Caruac*).	...	15	10	...	25	10	...	1	...	...	11	...	...	...	1	...	1	1	...	...	1	...	...	...	...	...	38
Tumiac (*Arzon*).	...	7	8	...	15	5	2	...	1	1	9	3	3	...	...	...	6	...	...	...	...	1	1	2	...	...	32
Total	1	80	49	1		19	2	1	3	3		3	4		1	1		1	2			1	1		1		
	Total 131					28						9						3				2			1		174

* Types of celts adopted by the *Société Polymathique du Morbihan*, aud alluded to in the above table.

DOLMEN-MOUNDS OF BRITTANY AND THEIR INTERIOR STRUCTURES

		No.	DESCRIPTION OF TUMULUS.	INTERIOR STRUCTURE.	EXAMPLES.	REMARKS.
UNCHAMBERED.		1	Long barrow.	Nil.	Kerlescant (?).	Comparatively rare; there is an erect menhir at west extremity of the examples at Kerlescant, and also a few traces of a revetment.
CHAMBERED.	UNSCULPTURED.	2	Round ditto.	Simple kist. C.*	Carnoet Finistere. Kerlevit, near Douarnenez.	Frequent. In the two examples given, bronze implements were found.
		3	Ditto ditto.	Ordinary megalithic dolmen, consisting of single chamber, with narrow entrance covered way at passage. C.	*Passim.*	This is the commonest type of tumulus, and forms the basis of which the following are variations.
		4	Ditto ditto.	Many-chambered megalithic dolmen, consisting of several chambers with a single narrow entrance. C.	Keriaval. Klud-er-yer.	Most frequent variation of above.
		5	Ditto ditto.	Many-chambered dolmen. Chambers of dry walling, and vaulted narrow entrance, megalithic and ceiled.	Rosmeur.	Not frequent.
		6	Long ditto.	Long, narrow, megalithic chamber, sometimes divided, sometimes with dolmen entrance. C.	Garren-dol. Parc ar Dolmen. Kerlescant. Kerlêarec.	There is a peristalith at Kerlescant. The two latter examples have tolmen entrances.
		7	Round ditto.	Megalithic dolmen, with passage at a sharp angle with chamber. C.	Kergonfals.	A rare example; the entrance passage descends into the chamber, and was found divided by walls of loose masonry.
	SCULPTURED.	8	Ditto ditto.	Megalithic dolmen, with curved gallery or passage. C.	Le Rocher. Pierres Plates.	Le Rocher is yet covered with its tumulus, and is a most unique and perfect specimen. The type of ornamentation (Pierres Plates is denuded) in both is similar.
		9	Ditto ditto.	Two or more megalithic dolmens, either parallel, or at right angles to one another. C.	Grottes de Kerozille. Do. Plouharnel. Kerlan.	At Kerozille, the centre dolmens at right angles to the others; in the other instances the dolmens are parallel to each other.
		10	Ditto ditto.	Square megalithic chamber, and long straight avenue, with elaborate ornamentation. C.	Gavr' Inis.	The most elaborately decorated dolmen known.
		11	Long ditto.	Megalithic dolmen at one extremity, and also other smaller kists, ceiled and vaulted.	Manné-Lud. Le Moustoir.	In the first, besides the large dolmen, is a vaulted kist and curious arrangement of stones. In the latter is one dolmen and three other structures, partially megalithic and partially vaulted.
		12	Immense tumuli, more or less elliptical.	Comparatively insignificant. Kists partly megalithic, partly ceiled, and partly vaulted. *C=Ceiled.	Tumiac. Manné-ér-H'roëk. Mont S. Michel.	Tumiac is conical. Inhumation relics. Manné-ér-H'Roëk longer and more truncated, contained no human relics. M. S. Michel long and narrow mound, showed signs of incineration of human remains.

The Citadel, Plymouth

S. P. OLIVER

NOTES

THE following lectures in pure Science are being delivered this term in Oxford. Prof. Phillips on the Heat of the Interior of the Earth, Ancient Climate, Earthquakes and Volcanos. Prof. Story Maskelyne two courses of lectures;—the first course of four lectures "On the projection of crystals, and on the relations of morphological symmetry to the distribution of physical and especially of optical characters;" second course "On lithology mineralogically considered." Prof. Lawson on Structural and Physiological Botany. He also proposes to make arrangements with his classes for botanical walks. The Professor of Astronomy will give practical instruction in the use of Astronomical Instruments at the Observatory at the Museum. The Chemical and Physical Laboratories are each open as usual, special courses of instruction being given in each. The Professor of Anatomy and Physiology proposes to form classes for practical instruction in Physiology and Anatomy. The Hope Professor of Zoology is engaged in the classification of the Hope, Burchell, and other collections. Dr. Lee's Reader in Chemistry is lecturing on the Non-metallic Elements, the Lee's Reader in Anatomy is lecturing on Comparative Anatomy, and the Lee's Reader in Physics on the Mechanical Theory of Heat, and on Hydromechanics.

AT the last meeting of the Royal Geographical Society, the President announced the Royal and other awards for the year 1872, made by the Council, as follows:—*Founder's Gold Medal*—Colonel H. Yule, C.B., for his important geographical works "Cathay and the Way Thither," and "Marco Polo." *Patron's Gold Medal.*—Mr. R. B. Shaw, for his adventurous journey to Yarkand and Kashgar, and his observations for fixing the longitude of the former place. *A Gold Watch.*—Lieut. G. C. Musters, R.N., for his journey in Patagonia. 25*l.*—Karl Mauch, for his discoveries in South-East Africa. *Schools Prizes.*—PHYSICAL GEOGRAPHY.—*Gold Medal*—S. E. Spring Rice (Eton College). *Bronze Medal*—A. S. Butler (Liverpool College). *Honourably Mentioned*—C. Penrose (Haileybury); E. Dickson (Cheltenham); J. R. White (Liverpool Institute); H. De Vere Vane (Eton). POLITICAL GEOGRAPHY.—*Gold Medal*—W. G. Collingwood (Liverpool College). *Bronze Medal*—W. C. Graham (Eton). *Honourably Mentioned*—R. H. Sayle (Uppingham); W. L. Kingsford (Rossall); H. E. Dickson (Rossall).

THE *conversazione* of the Royal Society on Saturday evening last was a highly successful one. The company, notwithstanding the counter attraction of the Duke of Edinburgh's reception at the Royal Albert Hall, was a brilliant one, and many of the objects and apparatus exhibited were of great interest. There was a beautiful series of photographs of landscape scenery, the geysers, and mud-springs in the regions of the Yellowstone River explored by Prof. Hayden, exhibited by Mr. A. Tylor; and some of the early photographs of M. Niêpce de St. Victor, showing effects of colour; and a collection of Madreporaria dredged up by Count Pourtales from the sea-floor in the course of the Gulf Stream, exhibited by Prof. Duncan. Mr. Browning and Messrs. Elliott Bros. had, as usual, a number of telescopic and spectroscopic instruments.

THE Senate of the University of London has this year re-elected all the old examiners in the Faculties of Science and Medicine, with the exception of the two in Forensic Medicine, the new examiners being Dr. Arthur Gamgee and Prof. Henry Maudsley.

AN Inaugural Meeting of the French Association for the Advancement of Science was held last week at Paris under the presidency of M. Claude Bernard; when the committee of management was elected as follows: MM. Claude Bernard, president; Broca, Delaunay, d'Eichthal, de Quatrefages, Wurtz, Cornu, secretary, and G. Masson, treasurer. It is still undecided whether to hold the first meeting, in August next, at Lyons, Bordeaux, or Lille. M. Wurtz delivered an address on the character and objects of the Association, in which he announced that the requisite guarantee fund of 100,000 fr. is already subscribed, and will probably be considerably exceeded. The movement appears to be meeting with a hearty response from all the leading men of science in France; it is welcomed as an aid in the regeneration of their country, by promoting a spread of scientific knowledge and a love of science throughout the provinces; and we trust it will also have the effect of cementing a closer alliance between French science and that of the other countries of Europe.

THE *Engineer* announces the death, at his residence near London, on April 15, at the age of 84, of Mr. Augustus Siebe. Born in Saxony, he served early in life against the French in the Prussian army; and after the return of peace employed himself first in watch-making, and afterwards in engineering. The perfection of the diving apparatus in particular engaged his attention, and to him are due many of the improvements now in constant use. In this capacity he was sole maker to the Admiralty, his apparatus having been used with great success in the removal of the wreck of the *Royal George.*

DR. PETTIGREW, F.R.S., will deliver a course of lectures on the Physiology of the Circulation in the Lower Animals and in Plants, at the Royal College of Surgeons, Edinburgh, each Friday afternoon, at 4 o'clock, during May, June, and July.

PROF. MAX MULLER gave a public lecture on April 24, at the Taylor Institution, Oxford, "On Darwin's View of Language." There was a large attendance.

THE old Ashmolean Society at Oxford, which was revived last term after a lengthened period of quietude, met on Monday last in the University Museum, when communications were made to the Society by Rev. R. Main, F.R.S., "On the Breaks of Continuity in the Mean Daily Temperature in the months of April and May," and by Mr. A. G. Vernon Harcourt, F.R.S., "On the Sulphur Compounds in Goal Gas, and the means of removing them."

THE garden of the Acclimatisation Society of Paris, ruined during the siege, is about to be reopened. A great part of the damage has been repaired, the ornamental and horticultural parts have been replaced, and there are already many animals in the park.

AT a recent meeting of the French Acclimatisation Society, M. de Grandmont called attention to the project of the Government to establish a grand piscicultural establishment at Montbéliard, to replace that at Huningue, now passed into the hands of the Germans. Very successful establishments of a similar character are now in operation at La Buisse and at Clermont Ferrand (Puy-de-Dôme); the latter, under the direction of M. Rico, furnishing annually not less than 30,000 ova of trout for replenishing the various streams in the department.

THE following excursions are arranged by the Geologists' Association during the present month:—Saturday, May 4, excursion to Erith and Crayford, under the direction of Mr. J. Logan Lobley. The party will leave Cannon Street Station by the North Kent train at 2·10 P.M. for Erith, and will, upon arriving at Erith, inspect the fine section of the Thanet Sands overlying the chalk, exposed at that place. Subsequently the party will proceed to Crayford, and visit the extensive excavations in the Pleistocene Deposits from which have been derived the large collection of Mammalian Remains in the possession of Dr. Spurrel, and inspected by the Association last session.—Saturday, May 11, visit to the British Museum, guided by Mr. Henry Woodward. Members will meet at the Museum, Great Russell

Street, at 2 P.M., and proceed to the Palæontological Department, where a portion of the collection will be described. —Whit-Monday and Tuesday, May 20 and 21, Excursion to Bath (one or two days); directors, Mr. Charles Moore, and the Rev. H. H. Winwood. Monday: Assemble at the Half Moon Hotel (near the Railway Station) at 1 o'clock; proceed to the Museum, the valuable and exceedingly interesting palæontological contents of which will be described by the founder, Mr. Charles Moore; then visit Weston and Twerton, and inspect, under the guidance of the Rev. Mr. Winwood, sections of Lower Lias (*Ammonites Bucklandi* and *A. angulatus* zones). Proceed to Newbridge Hill, where are sections of White Lias and the Rhætic series.—Tuesday: Hampton Down—fine sections of the Great Oolite, and a probable representative of the Bradford Clay; upper beds very fossiliferous (*Terebratula cardium, Terebratella Buckmani, Crania Antiquorum*, &c., with Corals and Sponges). Dundas in the Bradford Valley —sections showing Marlstone, Upper Lias, and Inferior Oolite. North Bank of Canal—section of Inferior Oolite (*Rhynchonella spinosa* zone), and Fuller's Earth. Cross the Avon to Freshford —Mammaliferous River deposits, yielding *Ovibos moschatus*.

THE prophetic announcements of Prof. Agassiz in regard to the discoveries he intended to make during his proposed deep-sea dredgings in the southern waters continue, according to *Harper's Weekly*, to be realised, as we learn from a letter to Prof. Peirce, dated at Rio on the 12th of February last. The weather had not been favourable for dredging for some time; but a suitable occasion presenting itself, the work was prosecuted for one day, with very interesting results. The first discovery mentioned by the professor was that of a living *Pecten*, very similar in general appearance to a fossil form known as *P. paradoxus*, found in Germany, and which he had been inclined to consider a distinct genus, on account of certain peculiarities which are not shared by any living shells known up to this discovery. The specimen found is, however, strictly referable to the same genus as the *paradoxus*, especially as it has the same prominent radiating ribs arising on the inner surface of the shell valve, to which the fossil is indebted for its specific name. Although of very small dimensions, being scarcely two-thirds of an inch in diameter, it is yet a specimen of very great significance. The second discovery was that of a very remarkable crustacean, and is, in part, the realisation of the expectation of finding "genera reminding us of some amphipods, and isopods aping still more closely the trilobites than *Serolis*." A specimen answering fully to this statement was taken in forty-five fathoms, and at first sight seemed like an ordinary isopod, with a broad, short, flat body. This, however, is not referable to any of the orders or families of Milne-Edwards or Dana, and, for reasons adduced, it has very striking relations to the trilobites, and is, indeed, like them, one of those types combining the structural features of several independent groups. It resembles the trilobites in the fact that the head is distinct from the thoracic regions; and the large faceted eyes and the facial suture across the cheeks connect it so closely that but for the presence of antennæ, which project from the lower side of the anterior margin of the buckler, the resemblance would amount to an absolute identity in structure with the trilobites. The character of the mouth is also that of the trilobite; while the antennæ cause its reference to the isopods. For this new genus the name of *Tomocaris Peircei* is proposed.

DETAILS have been received of the earthquake which devastated Antioch on April 3, to which we referred last week. The Greek church, a strong stone-arched structure, built only a few years ago, and capable of holding 500 or 600 persons, is utterly ruined —one side and the entire roof are gone. The American Protestant church and premises are also greatly injured, and four persons of their small community were killed, though the Mission families are all safe. The number of killed and injured cannot be ascertained with any approach to accuracy, and, of course, flying rumours are abundant, one man saying that he thought there must be 1,000 killed, while another said 500, and a third 250, which is, perhaps, within the truth. There was time from the beginning of the first shock to its close for many to escape the falling houses or walls. Several smaller and lighter shocks occurred for an hour or two afterwards, but not sufficiently strong to shake down buildings. These shocks continued at intervals through the next night; and another, more distinct and wave-like, was felt to shake the house with a loud, hollow, rumbling noise, about half-past six the next morning. The first shock was immediately preceded by a rumbling and creaking of the joints of the window and door frames, to which a louder noise, like thunder, succeeded, and then walls and buildings fell. The old Roman bridge of four arches is rent in several places until the water can be seen through it from above; a part of the parapet wall has also been shaken off, and the arch above the city door at its east end has been hurled down, and lies almost whole. Much damage has been done to houses in the lower part of the town, and many of the inhabitants are now to be seen encamping around in the fields or plain. The shocks appear to have continued, with less severity, for several days. One man declared he counted forty-four shocks within twenty-four hours after the first one. They were all accompanied by a noise like distant thunder or artillery, and produced a tremor of the ground; but no fresh ruin has been made by any of them, except the first great shock about 8 A.M. of the 3rd ult.

EARTHQUAKES are becoming almost as frequent in the Murrumbidgee district in Australia as in New Zealand. The Wagga *Express* reports that recently (in January last) a smart shock was experienced at Crabtree station and several other places on the Upper River. Since June 8—when the first and, with one exception, the heaviest of the shocks was experienced—at least a dozen distinct ones have been felt in this district.

THE Italian Society of Spectroscopists is already doing work which must command the attention of the scientific world. The special object of the Society is to collate the observations of all Italian astronomers, so as to study daily the number, position, size, and form of the protuberances, spots, and faculæ. Three numbers are already published of their *Memorie*, containing the following papers:—An Introduction by Prof. Tacchini, of the Observatory of Palermo, expounding the object and resources of the Society; a memoir by M. Lorenzoni, of the Observatory of Padua, on the spectral analysis of the protuberances; numerical tables to convert into heliocentric coordinates the apparent position of protuberances or spots; a memoir by P. Secchi on a new micrometer for measuring the height of the protuberances; an article by Prof. Tacchini on the comparison of the observations of protuberances made simultaneously in July 1871, at Palermo by himself, and at Rome by Secchi; observations on the solar protuberances and their distribution, by P. Secchi, spectroscopic images of the solar margin, made at Rome, Palermo, and Padua, by Secchi, Tacchini, and Lorenzoni on December 11 and 12, 1871, with a coloured plate. This youngest outcome of solar physics is deserving of the heartiest support of men of science in this country.

ACCORDING to the Sydney *Herald*, the schooner *Surprise* has lately made a visit to the coast of New Guinea, penetrating fifteen miles up the Manoa River. Contrary to the general impression, the natives, who were hitherto supposed to be ferocious in their character and opposed to the visits of strangers, were found to be mild and gentle in disposition. They were of the Malay stock, and had never seen white people before. On the departure of the schooner, under Captain Paget, they exhibited every demonstration of sorrow, the women weeping and the men accompanying the party to a considerable distance

HISTORY OF THE NAMES CAMBRIAN AND SILURIAN IN GEOLOGY*

IT is proposed in the following pages to give a concise account of the progress of investigation of the lower palæozoic rocks during the last forty years. The subject may naturally be divided into three parts: (1) the history of Silurian and Upper Cambrian in Great Britain from 1831 to 1854; (2) that of the still more ancient palæozoic rocks in Scandinavia, Bohemia, and Great Britain up to the present time, including the recognition by Barrande of the so-called primordial palæozoic fauna; (3) the history of the lower palæozoic rocks of North America.

I.—*Silurian and Upper Cambrian in Great Britain.*

Less than forty years since, the various uncrystalline sedimentary rocks beneath the coal-formation in Great Britain and in continental Europe were classed together under the common name of graywacke or grauwacké, a term adopted by geologists from German miners, and originally applied to sandstones and other coarse sedimentary deposits, but extended so as to include associated argillites and limestones. Some progress had been made in the study of this great Graywacke formation, as it was called, and organic remains had been described from various parts of it; but to two British geologists was reserved the honour of bringing order out of this hitherto confused group of strata, and establishing on stratigraphical and palæontological grounds a succession and a geological nomenclature. The work of these two investigators was begun independently and simultaneously in different parts of Great Britain. In 1831 and 1832, Sedgwick made a careful section of the rocks of North Wales from the Menai Strait across the range of Snowdon to the Berwyn hills, thus traversing in a south-eastern direction Caernarvon, Denbigh, and Merionethshire. Already, he tells us, he had in 1831 made out the relations of the Bangor group (including the Llanberis slates and the overlying Harlech grits), and showed that the fossiliferous strata of Snowdon occupy a synclinal, and are stratigraphically several thousand feet above the horizon of the latter. Following up this investigation in 1832, he established the great Merioneth anticlinal, which brings up the lower rocks on the south-east side of Snowdon, and is the key to the structure of North Wales. From these as a base, he constructed a section along the line already indicated, over Great Arenig to the Bala limestone, the whole forming an ascending series of enormous thickness. This limestone in the Berwyn hills is overlaid by many thousand feet of strata as we proceed eastward along the line of section, until at length the eastern dip of the strata is exchanged for a westward one, thus giving to the Berwyn chain, like that of Snowdon, a synclinal structure. As a consequence of this, the limestone of Bala re-appears on the eastern side of the Berwyns, underlain as before by a descending series of slates and porphyries. These results, with sections, were brought before the British Association for the Advancement of Science at its meeting at Oxford in 1832; but only a brief and imperfect account of the communication of Sedgwick on this occasion appears in the Proceedings of the Association. He did not at this time give any distinctive name to the series of rocks in question (L. E. and D. Philos. Mag. (1854) IV., viii. 495).

Meanwhile in the same year, 1831, Murchison began the examination of the rocks on the river Wye, along the southern border of Radnorshire. In the next four years he extended his researches through this and the adjoining counties of Hereford and Salop, distinguishing in this region four separate geological formations, each characterised by peculiar fossils. These formations were moreover traced by him to the south-westward across the counties of Brecon and Caermarthen; thus forming a belt of fossiliferous rocks stretching from near Shrewsbury to the mouth of the river Towey, a distance of about 100 miles along the north-west border of the great Old Red Sandstone formation, as it was then called, of the West of England.

The results of his labours among the rocks of this region for the first three years were set forth by Murchison in two papers presented by him to the Geological Society of London in January 1834 (Proc. Geol. Soc. ii, 11). The formations were then named as follows in descending order:—(1) Ludlow, (2) Wenlock, constituting together an upper group; (3) Caradoc, (4) Llandeilo (or Builth), forming a lower group. The Llandeilo formation, according to him, was underlain by what he called the Longmynd and Gwastaden rocks. The non-fossiliferous strata of the Longmynd hills in Shropshire were described as rising up to the east from beneath the Llandeilo rocks; and as appearing again in South Wales, at the same geological horizon, at Gwastaden in Breconshire, and to the west of Llandovery in Caermarthenshire; constituting an underlying series of contorted slaty rocks many thousand feet in thickness, and destitute of organic remains. The position of these rocks in South Wales was, however, to the north-west, while the strata of the Longmynd, as we have seen, appear to the east of the fossiliferous formations.

In the "Philosophical Magazine" for July, 1835, Murchison gave to the four formations above named the designation of Silurian, in allusion, as is well known, to the ancient British tribe of the Silures. It now became desirable to find a suitable name for the great inferior series, which, according to Murchison, rose from beneath his lowest Silurian formations to the north-west, and appeared to be widely spread in Wales. Knowing that Sedgwick had long been engaged in the study of these rocks, Murchison, as he tells us, urged him to give them a British geographical name. Sedgwick accordingly proposed for this great series of Welsh rocks, the appropriate designation of Cambrian, which was at once adopted by Murchison for the strata supposed by him to underlie his Silurian system. (Murchison, Anniv. Address, 1842; Proc. Geol. Soc. iii., 641.) This was almost simultaneous with the giving of the name of Silurian; for in August 1835, Sedgwick and Murchison made communications to the British Association at Dublin on Cambrian and Silurian Rocks. These, in the Volume of Proceedings (pp. 59, 60) appear as a joint paper, though from the text they would seem to have been separate. Sedgwick then described the Cambrian rocks of North Wales as including three divisions: 1. The Upper Cambrian, which occupies the greater part of the chain of the Berwyns, where, according to him, it was connected with the Llandeilo formation of the Silurian. To the next lower division, Sedgwick gave the name of Middle Cambrian, making up all the higher mountains of Caernarvon and Merionethshire, and including the roofing-slates and flagstones of this region. This middle group, according to him, afforded a few organic remains, as at the top of Snowdon. The inferior division, designated as Lower Cambrian, included the crystalline rocks of the south-west coast of Caernarvon and a considerable portion of Anglesea, and consisted of chloritic and micaceous schists, with slaty quartzites and subordinate beds of serpentine and granular limestone; the whole without organic remains.

These crystalline rocks were, however, soon afterwards excluded by him from the Cambrian series; for in 1838 (Proc. Geol. Soc. ii., 679) Sedgwick describes further the section from the Menai Strait to the Berwyns, and assigns to the chloritic and micaceous schists of Anglesea and Caernarvon a position inferior to the Cambrian, which he divides into two parts; viz., Lower Cambrian, comprehending the old slate series, up to the Bala limestone beds; and Upper Cambrian, including the Bala beds and the strata above them in the Berwyn chain, to which he gave the name of the Bala group. The dividing line between the two portions was subsequently extended downwards by Sedgwick to the summit of the Arenig slates and porphyries. The lower division was afterwards subdivided by him into the Bangor group (to which the name of Lower Cambrian was henceforth to be restricted), including the Llanberis roofing-slates and the Harlech grits or Barmouth sandstones; and the Festiniog group, which included the Lingula-flags and the succeeding Tremadoc slates.

In the communication of Murchison to the same Dublin meeting, in August 1835, he repeated the description of the four formations to which he had just given the name of Silurian; which were, in descending order, Ludlow and Wenlock (Upper Silurian), and Caradoc and Llandeilo (Lower Silurian). The latter formation was then declared by Murchison to constitute the base of the Silurian system, and to offer in many places in South Wales distinct passages to the underlying slaty rocks, which were, according to him, the Upper Cambrian of Sedgwick.

Meanwhile, to go back to 1834, we find that after Murchison had, in his communication to the Geological Society, defined the relation of his Llandeilo formation to the underlying slaty series, but before the names of Silurian and Cambrian had been given to these respectively, Sedgwick and Murchison visited together the principal sections of these rocks from Caermarthenshire to Denbighshire. The greater part of this region was unknown to Sedgwick, but had already been studied by Murchison, who interpreted the sections to his companion in conformity with the scheme already given; according to which the beds of the Llandeilo were underlain by the slaty rocks which appear along their north-western border. When, however, they entered the region which had already been examined by Sedgwick, and reached the section on the east side of the Berwyns, the fossiliferous beds of Meifod were at once pronounced by Murchison to be typical Caradoc, while others in the vicinity were regarded as Llandeilo. The beds of Meifod had, on palæontological grounds, been by Sedgwick identified with those of Glyn Ceirog, which are seen to be immediately overlain by Wenlock rocks. These determinations of Murchison were, as Sedgwick tells us, accepted by him with great reluctance, inasmuch as they involved the upper part of his Cambrian section in most perplexing difficulties. When, however, they crossed together the Berwyn chain to Bala, the limestones in this locality were found to contain fossils nearly agreeing with those of the so-called Caradoc of Meifod. The examination of the section here presented showed, however, that these limestones are overlain by a series of several thousand feet of strata bearing no resemblance, either in fossils or in physical characters, to the Wenlock formation which overlies the Caradoc beds of Glyn Ceirog. This series was, therefore, by Murchison supposed to be identical with the rocks which, in South Wales, he had placed beneath the Llandeilo, and he expressly declared that the Bala group could not be brought within the limits of his Silurian system. It may here be added that in 1842 Sedgwick re-examined this region, accompanied by that skilled palæontologist, Salter, confirming the accuracy of his former sections, and showing moreover by the evidence of fossils that the beds of Meifod, Glyn Ceirog, and Bala, are very nearly on one parallel. Yet, with the evidence of the fossils before him, Murchison, in 1834, placed the first two in his Silurian system, and the last deep down in the Upper Cambrian; and consequently was aware that on palæontological grounds it was impossible to separate the lower portion of his Silurian system from the Upper Cambrian of Sedgwick. (These names are here used for convenience, although we are speaking of a time when they had not been applied to designate the rocks in question.)

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* Reprinted from advance sheets of the *Canadian Naturalist*.

This fact was repeatedly insisted upon by Sedgwick, who, in the Syllabus of his Cambridge lectures, published very early in 1837, enumerated the principal genera and species of Upper Cambrian fossils, many of which were by him declared to be the same with those of the Lower Silurian rocks of Murchison. Again, in enumerating in the same Syllabus the characteristic species of the Bala limestone, it is added by Sedgwick: "all of which are common to the Lower Silurian system." This was again insisted upon by him in 1838 and 1841. (Proc. Geol. Soc. ii. 679; iii. 548.) It was not until 1840 that Bowman announced the same conclusion, which was reiterated by Sharpe in 1842. (Ramsay, Mem. Geol Sur. iii., part 2, p. 6)

In 1839 Murchison published his "Silurian System," dedicated to Sedgwick, a magnificent work in two volumes quarto, with a separate map, numerous sections, and figures of fossils. The succession of the Silurian rocks, as there given, was precisely that already set forth by the author in 1834, and again in 1835; being, in descending order, Ludlow and Wenlock, constituting the Upper Silurian, and Caradoc and Llandeilo (including the Lower Llandeilo beds or Stiper-stones), the Lower Silurian. These are underlain by the Cambrian rocks, into which the Llandeilo was said to offer a transition marked by beds of passage. Murchison in fact declared that it was impossible to draw any line of separation, either lithological, zoological, or stratigraphical, between the base of the Silurian beds (Llandeilo) and the upper portion of the Cambrian, the whole forming, according to him, in Caermarthenshire, one continuous and conformable series from the Cambrian to the Ludlow. ("Silurian System," pp. 256, 358) By Cambrian in this connection we are to understand only the Upper Cambrian or Bala group of Sedgwick, as appears from the express statement of Murchison, who alludes to the Cambrian of Sedgwick as including all the older slaty rocks of Wales, and as divided into three groups, but proceeds to say that in his present work (the "Silurian System") he shall notice only the highest of these three.

Since January 1834, when Murchison first announced the stratigraphical relations of the lower division of what he afterwards called the Silurian system, the aspect of the case had materially changed. This division was no longer underlain, both to the east in Shropshire and to the west in Wales, by a great unfossiliferous series. His observations in the vicinity of the Berwyn hills with Sedgwick in 1834, and the subsequently published statements of the latter had shown, that this supposed older series was not without fossils; but on the contrary, in North Wales, at least, held a fauna identical with that characterising the Lower Silurian. Hence the assertion of Murchison in his "Silurian System," in 1839, that it was not possible to draw any line of demarcation between them. The position was very embarrassing to the author of the "Silurian System," and for the moment, not less so to the discoverer of the Upper Cambrian series. Meanwhile, the latter, as we have seen, in 1842 re-examined with Salter his Upper Cambrian sections in North Wales, and satisfied himself of the correctness, both structurally and palæontologically, of his former determinations. Murchison, in his Anniversary Address as President of the Geological Society in 1842, after recounting, as we have already done, the history of the naming by Sedgwick in 1839 of the Cambrian series, which Murchison supposed to underlie his Silurian system, proceeded as follows:—"Nothing precise was then known of the organic contents of this lower or Cambrian system, except that some of the fossils contained in its upper members in certain prominent localities were published Lower Silurian species. Meanwhile, by adopting the word Cambrian, my friend and myself were certain that whatever might prove to be its zoological distinctions, this great system of slaty rocks being evidently inferior to those zones which had been worked out as Silurian types, no ambiguity could hereafter arise. . . . In regard, however, to a descending zoological order, it still remained to be proved whether there was any type of fossils in the mass of the Cambrian rocks different from those of the Lower Silurian series. If the appeal to nature should be answered in the negative, then it was clear that the Lower Silurian type must be considered the true base of what I had named the protozoic rocks; but if characteristic new forms were discovered, then would the Cambrian rocks, whose place was so well established in the descending series, have also their own fauna, and the palæozoic base would necessarily be removed to a lower horizon." If the first of these alternatives should be established, or in other words, if the fauna of the Cambrian rocks was found to be identical with that of the Lower Silurian, then, in the author's language, "the term Cambrian must cease to be used in zoological classification, it being, in that sense, synonymous with Lower Silurian." That such was the result of palæontological inquiry, Murchison proceeded to show, by repeating the announcements already made by Sedgwick in 1837 and 1838, that the collections made by the latter from the great series of fossiliferous strata in the Berwyns, from Bala, from Snowden, and other Cambrian tracts, were identical with the Lower Silurian forms. These strata, it was said, contain throughout "the same forms of *Orthis* which typify the Lower Silurian rocks." It was further declared by Murchison in this address that researches in Germany, Belgium, and Russia led to the conclusion that the "fossiliferous strata characterised by Lower Silurian Orthidæ are the oldest beds in which organic life has been detected." (Proc. Geol. Soc. iii. 641, &c.) The Orthids here referred to are, according to Salter, *Orthis calligramma*, Dalm, and its varieties. (Mem. Geol. Survey iii, part 2, 335-337.)

Meanwhile Sedgwick's views and position began to be misrepresented. In 1842 Mr. Sharpe, after calling attention to the fact that the fossils of the Bala limestone were, as Sedgwick had long before shown, identical with those of Murchison's Lower Silurian, declared that Sedgwick had placed the Upper Cambrian, in which the Bala beds were included, beneath the Silurian, and that this determination had been adopted by Murchison on Sedgwick's authority. (Proc. Geol. Soc. iv. 10.) This statement Murchison suffered to pass uncorrected in a complimentary review of Sharpe's paper in his next annual address (1843.) In his "Siluria," 1st edition, p. 25 (1854), he speaks of the term Cambrian as applied (in 1835) by Sedgwick and himself "to a vast succession of *fossiliferous* strata containing undescribed fossils, the whole of which were supposed to rise up from beneath well-known Silurian rocks. The Government geologists have shown that this *supposed* order of superposition was erroneous," &c. The italics are the author's. Such language, coupled with Mr. Sharpe's assertion noticed above, helped to fix upon Sedgwick the responsibility of Murchison's error. Although the historical sketch which precedes clearly shows the real position of Sedgwick in the matter, we may quote further his own words:—"I have often spoken of the great Upper Cambrian group of North Wales as inferior to the Silurian system, . . on the sole authority of the Lower Silurian sections, and the author's many times repeated explanations of them before they were published. So great was my confidence in his work, that I received it as perfectly established truth that his order of superposition was unassailable. . . . I asserted again and again that the Bala limestone was near the base of the so-called Upper

Cambrian group. Murchison asserted and illustrated by sections the unvarying fact that his Llandeilo flag was superior to the Upper Cambrian group. There was no difference between us until his Llandeilo sections were proved to be wrong." (Philos. Mag. IV. viii. 506.) That there must be a great mistake in Sedgwick's or in Murchison's sections was evident, and the Government surveyors, while sustaining the correctness of those of Sedgwick, have shown the sections of Murchison to have been completely erroneous.

The first step towards an exposure of the errors of the Silurian sections is, however, due to Sedgwick and McCoy. In order better to understand the present aspect of the question, it will be necessary to state in a few words some of the results which have been arrived at by the Government surveyors in their studies of the rocks in question, as set forth by Ramsay in the Memoirs of the Geological Survey. In the section of the Berwyns, the thin bed of about twenty feet of Bala limestone, which (as originally described by Sedgwick) they have found outcropping on both sides of the synclinal chain, is shown to be intercalated in a vast thickness of Caradoc rocks; being overlain by about 3,300 and underlain by 4,500 feet of strata belonging to this formation. Beneath these are 4,500 feet additional of beds described as Llandeilo, which rest unconformably upon the Lingula-flags just to the west of Bala; thus making a thickness of over 12,000 feet of strata belonging to the Bala group of Sedgwick. A small portion of rocks referred to the Wenlock formation occupies the synclinal above mentioned. (Memoirs, III. part 2, 214,222.) The second member, in ascending order, of the Silurian system, to which the name of Caradoc was given by him in 1839, was originally described by Murchison under the names of the Horderly and May Hill sandstone. The higher portions of the Caradoc were subsequently distinguished by the Government surveyors as the Lower and Upper Llandovery rocks; the latter (constituting the May Hill sandstone, and known also as the Pentamerus beds) being by them regarded as the summit of the Caradoc formation. In 1852, however, Sedgwick and McCoy showed from its fauna that the May Hill sandstone belongs rather to the overlying Wenlock than to the Caradoc formation, and marks a distinct palæontological horizon.

This discovery led the geological surveyors to re-examine the Silurian sections, when it was found by Aveline that there exists in Shropshire a complete and visible want of conformity between the underlying formations and the May Hill sandstone; the latter in some places resting upon the nearly vertical Longmynd rocks, and in others upon the Llandeilo flags, the Caradoc proper or Bala group, and the Lower Llandovery beds. Again, in South Wales, near Builth, the May Hill sandstone or Upper Llandovery rests upon Lower Llandeilo beds; while at Noeth Grug the overlying formation is traced transgressively from the Lower Llandovery across the Caradoc to the Llandeilo. These important results were soon confirmed by Ramsay and by Sedgwick. (Ibid, 4, 236). The May Hill sandstone often includes, near its base, conglomerate beds made up of the ruins of the older formation. To the north-east, in the typical Silurian country, it is of great thickness and continuity, but gradually thins out to the south-west. There exists, moreover, another region where not less curious discoveries were made. About forty miles to the eastward of the typical region in South Wales appear some important areas of Silurian rocks. These are the Woolhope beds, appearing through the Old Red Sandstone, and the deposits of Abberley, the Malverns and May Hill rising along its eastern border, and covered along their eastern base by the newer Mesozoic sandstone. The rocks of these localities were by Murchison in his "Silurian System" described as offering the complete sequence. When however it was found that his Caradoc included two unconformable series, examination showed that there was no representative of the older Caradoc or Bala group in these eastern regions, but that the so-called Caradoc was nothing but the Upper Llandovery or May Hill sandstone. The immediately underlying strata, which Murchison had regarded as Llandeilo, or rather as the beds of passage from Llandeilo to Cambrian, and had compared with the north-west passage of the Caermarthenshire sections (Syl. Sys. 416), have since been found to be much more ancient deposits, of Middle Cambrian age, which rests upon the crystalline hypozoic rocks of the Malverns, and are uncomformably overlain by the May Hill sandstone. We shall again revert to this region, which has been carefully studied and described by Prof. John Phillips. (Mem. Geol. Sur. II., part 1.)

T. STERRY HUNT

(*To be continued*)]

SCIENTIFIC SERIALS

THE *Révue Scientifique*, Nos. 33—42. The first article in these numbers is by Prof. Huxley, on Yeast.—The conclusion is arrived at of M. de Quatrefages' course of lectures on Anthropology, at the Museum of Natural History at Paris.—M. de Fonvielle contributes an article on Balloon Observations. In No 34. M. Léon Le Fort, Professor to the Faculty of Medicine at Paris, furnishes an account of military surgery in the Austrian army.—Among the contributions from foreign sources is a report of M. Vogel's spectroscopic observations of the planets made in 1871 at the observatory of Bothkamp.—No. 35 contains an interesting lecture, delivered at the University of Friberg-in-Brisgau, by M. Ecker, on the struggle for existence in the character and in the life of nations.—In No. 36 is commenced a report, continued in subsequent numbers, of M. Claude Bernard's course of lectures at the College of France on Animal Heat. A translation is given of a paper by Prof. Harting, of Utrecht, on the artificial production of organic calcareous formations. There are also reports of the proceedings of English and other foreign scientific societies.—In No. 37 the most interesting paper is one by Dr. Onimus, on the consecutive phenomena attendant on the removal of the brain, and on the movements of rotation, illustrated with drawings of frogs and birds, on which the operation had been performed, to show the action.—No. 38 contains a report of Prof. Virchow's address to the Congress of German Naturalists and Physicians at Rostock, on Science in the national life of Germany. Sir William Thomson's paper on the Size of Atoms is translated from an early number of this journal. M. Papillon has an article in support of M. Wurtz's aphorism, "Chemistry is a French science, constituted by Lavoisier," in reply to English and German attacks.—No. 39 commences with an important article by M. P. Lorain, on Reform in the Higher Instruction. A translation is given of M. R. Wolf's lecture at the University of Zurich on Solar Spots, and of Neumayer's paper contributed to the Vienna *k. k. Geologische Reichsanstalt* on the Jurassic Basins.—In No. 40 M. Le Fort supplements his previous paper by an additional one on Military Surgery in the Prussian army. This and the following number are partially filled with further reports of the Rostock meeting of German naturalists and physicians.—In No. 41 we find also a lecture by M. Lereboullet at the School of Military Health at Montpellier on the Spinal Column.—No. 42 contains a report of M. Blanchard's address to the Annual Congress of the Learned Societies of the Departments at the Sorbonne. M. Paul de Saint Robert contributes a paper with the suggestive title, "Qu'est ce que la force?" There are also a number of reports of the proceedings of foreign societies.

THE *Journal of Botany* for April commences with an interesting article by Prof. Babington on the *Anacharis alsinastrum* or Canadian water-weed. He shows clearly that there are two series of plants closely resembling each other in appearance, of which one series has perfect triandrous flowers, and the other has incomplete diœcious flowers, of which the males are nearly or quite sessile, and have the curious habit of becoming detached when the pollen is ripe, and floating freely about on the surface of the water, and shedding their pollen there. To the first series belongs the *Elodea* of South America; to the second, *Anacharis* and *Hydrilla*. *Anacharis* would therefore appear to be the true genus to which the English (introduced) plant belongs, as given by Hooker, but not by Bentham and Syme.—Mr. Baker gives one of his useful synoptical revisions of the Cape species of *Anthericum*; and Prof. Thiselton Dyer a history of the substance known as "Australian Caoutchouc," and a valuable account of the mode of germination of *Tropæolum*, which is characteristic rather of endogens than of exogens.

Annalen der Chemie und Pharmacie, January 1872. This number commences with a paper by Max Ascher on the trisubstitution derivation of benzine; this is an attempt to establish the constitution of some of these bodies, but at present, however, it has not been entirely successful.—The next paper is by Linnemann and Zotta "On the reduction of formic acid to formaldehyde and methyl alcohol;" the authors followed exactly the same process as was described by Lieben and Rossi some time since, and which has already been noticed in these pages. The next three communications are by Linnemann "On normal propyl alcohol, its compounds and its conversion into isopropyl alcohol." The author prepared the normal alcohol in two ways—by the action of nascent hydrogen on propionic anhydride, and by obtaining the aldehyde by distillation of calcic propionate and formiate, and subsequent treatment with sodium amalgam. The normal alcohol was converted into isopropyl alcohol by preparing from it the iodide, and from this the cyanide, which by treatment with potash yields propylamine. Propylamine hydrochloride on heating with argentic nitrate and water yields isopropyl alcohol, having the characteristic properties and reactions of this body.—Hugo Schiff follows "On the constitution of æsculin;" this is an important theoretical paper.—Quinke contributes the second part of his memoir, "On a new class of aromatic hydrocarbons." By the action of benzyl chloride on toluol, benzyl toluol $C_{14}H_{14}$ is formed with elimination of hydrochloric acid. By oxidation, this hydrocarbon yields benzylbenzoic acid $C_{14}H_{10}O_2$; this by the action of nascent hydrogen yields benzhydrylbenzoic acid $C_{14}H_{12}O_2$; and by the treatment with hydriodic acid the latter yields benzylbenzoic acid $C_{14}H_{12}O_2$.—Translations of two papers, which have already appeared in the French journals, and also a paper by Maly, complete this number.

THE *Journal of the Franklin Institute* for February, after the usual editorial paragraphs descriptive of novelties, has an article by Prof. H. B. Thurston, on Experimental Steam Boiler Explosions, containing a report of a series of experiments undertaken by a committee of the Institute, in conjunction with several railway engineers, for the purpose of testing the conditions under which steam boilers explode—Mr. F. A. Genth continues his paper on the Mineral Resources of North Carolina; and Mr. J. P. Cooke his Chemical Theory of the Voltaic Battery, the portion in the present number being devoted to Electricity.—Prof. P. E. Chase gives a Table of Cyclical Rainfalls at Lisbon, in support of the theory already advanced by him of the lunar influence on the weather. The other papers in this number are mostly of a purely mathematical character.

The first original article in the number for March is chiefly of local interest—"The Locomotive Engine, and Philadelphia's share in its Early Improvements," by J. Harrison, jun., the last recipient of the Rumford Gold Medal.—Mr. James Emerson gives a Report of Water-wheel Tests at Lowell and other places, illustrated with plates.—In another paper on Experimental Steam Boiler Explosions, Prof. Thurston discusses the third experiment at Sandy Hook, New York.—Mr. Joseph S. Smith describes the construction of the Keokak and Hamilton Bridge across the Mississippi, connecting the States of Iowa and Illinois.—Dr. Richard Owen, of the Indiana State University, details a series of experiments on Terrestrial Magnetism.—Prof. R. E. Chase gives tables of the Monthly Rainfall at San Francisco.

THE first article in the *American Journal of Science and Arts* for March is a most interesting account, by Prof. Hayden, of the Hot Springs and Geysers of the Yellowstone and Firehole rivers, accompanied by illustrations and maps. The description of these geysers, which throw those of Iceland completely into the shade, will not bear epitomising, but is well worth reading as a whole.—Two other geological articles are by Prof. Dana, on the Quartzite of the Green Mountains, and by Prof. Silliman, Geological and Mineralogical Notes on some of the mining districts of Utah territory.—Prof. A. E. Verril contributes two instalments of his contributions to zoology, from the Museum of Yale College, on the affinities of Palæozoic Tabulate Corals with existing species, and recent additions to the molluscan fauna of New England.—The other article of greatest interest is by Prof. H. A. Nicholson of Toronto, on the genera *Cornulites* and *Tentaculites*, and on a new genus *Conchicolites*.

THE *Quarterly Journal of Science* for April commences with an article by Mr. R. A. Proctor on Meteoric Astronomy. In reference to the researches of Signor Schiaparelli, he gives an account of the meteoric systems through which the earth passes in August and November, and seeks in meteoric phenomena and the associated phenomena of comets the explanation of some of the features presented by the solar corona. The zodiacal light he explains also on the theory of its being the outer portion of the sun-surrounding meteor families.—Mr. James Douglas, of Quebec, gives a description of the copper mines of Chili, and of the geological and mineralogical features of the country where they occur.—An article on Natural and Artificial Flight describes the results arrived at by Dr. Pettigrew in the case of the flight of birds and insects, and consists to a large extent of extracts from that gentleman's memoirs contributed to the Transactions of the Linnean Society and of the Royal Society of Edinburgh.—Mr. William Topley, in an article on the Geology of the Straits of Dover, illustrated by sections, advocates the project proposed by Hawkshaw and Low of a submarine tunnel, to run from near the South Foreland to a point between Sangatte and Calais, which he believes will pass entirely through the chalk, and will not be likely to be attended by any insuperable engineering difficulties.—An article on the Gold Coinage, and a short one by Captain Oliver on Recent Changes in British Artillery Material, complete the original portion of the number.—Only four books are reviewed this quarter, and more than half the space allotted to reviews is occupied by a notice, by Mr. A. R. Wallace, of Mr. Dale Owen's "The Debatable Land between this World and the Next," in which the reasonableness of the alleged phenomena of modern Spiritualism is advocated.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 25.—"On a supposed Periodicity in the Elements of Terrestrial Magnetism, with a period of 26⅓ days." By George Biddell Airy, Astronomer Royal, P.R.S.

In a paper published in the "Proceedings of the Imperial Academy of Sciences of Vienna," vol. lxiv., Dr. Karl Hernstein has exhibited the results of a series of observations which appeared to show that the earth's magnetism undergoes a periodical change in successive periods of 26⅓ days, which might with great plausibility be referred to the rotation of the sun.

It appeared to the author that the deductions from the magnetic observations made at the Royal Observatory of Greenwich, and which are printed annually in the "Greenwich Observations," or in the detached copies of "Results of Magnetical and Meteorological Observations made at the Royal Observatory of Greenwich," would afford good materials for testing the accuracy of this law, as applicable to a series of years.

The mean for declination in 1870, and, still more remarkably, the mean for horizontal force in 1870, appear to exhibit an increase about the fourteenth day. But the author does not remark in the other means, either as given in numerals or as projected as curves, anything to support the idea of an inequality periodical in the 26⅓ days. It might almost be suspected that the secular changes used in the period 1850-1852 are too large; but no alteration of these renders the inequality of 26⅓ days more probable. Dr. Hornstein's investigation was limited to observations made in 1870.

Royal Geographical Society, April 22.—General Sir Henry C. Rawlinson, K.C.B., president, in the chair. "On Recent Explorations of the North Polar Region, by Captain Sherard Osborn, R.N. Captain Osborn commenced by alluding to his advocacy of a Polar Expedition *via* Smith Sound, in 1865, and stated that the Duke of Somerset, then First Lord of the Admiralty, though apparently sufficiently favourable to the general proposal of a Government Expedition, urged upon him by a deputation from the Society who waited on him in that year, declined to assume the responsibility of recommending an expedition, owing to the difference of opinion which then reigned with regard to the best route to be followed. The alternate route to Smith Sound was that by the seas of Spitzbergen, advocated by Dr. Petermann and others, on the ground that the Gulf Stream, flowing in that direction, maintained an open sea to the Pole. He (Captain Osborn) and the promoters of the Expedition were content to wait the result of efforts made soon after by the Swedes and Germans to carry out the views of the German geographer. Seven years had elapsed, and we were now in a position to say that the advocates of the Spitzbergen route had been proved entirely wrong, whilst those who believed Smith Sound to be the best route were right. Captain Koldeway, who commanded both the German Expeditions, states, as the result of all his efforts, that "one can hardly resist the conviction that the hope of attaining the North Pole by ship, or of finding an open sea around the Pole, are alike among the most improbable of things. I confess that I myself was misled by representations in Dr. Petermann's 'Geographische Mittheilungen,' and held it to be at least possible by following a line of coast to penetrate by ship far into the central Arctic region, and then certainly to make one's way to the Pole. A winter in East Greenland, the most careful observation of these mighty masses of ice, their movements and formation, and of the whole condition of temperature, have radically cured me and all my companions of this idea. . . . If the principal object be the nearest possible approach to the Pole, I am quite of Osborn's opinion that the best way appears to be through Smith Sound. Here one can penetrate by ship every year to the 78th parallel, and then one has a continuous line of coast running north, which has been sighted as far as the 82nd parallel. Along this coast one would have to work one's way in spring with dog-sledges. I consider it a wild undertaking to penetrate towards the Pole by ship between Spitzbergen and Nova Zembla." No one could undo the effect of evidence so honest and conclusive as this. The Duke of Somerset rested his decision to delay action on the importance of first being furnished with the results of the Swedish Expedition, then on its way to Spitzbergen. The Swedes during the last seven or eight years had sent no less than four expeditions to the verge of the Polar region; and the conclusion of their scientific leader, Von Nordenskiöld, is that in summer it is not possible to penetrate by ship through the pack, and that an open Polar Sea is a mere hypothesis destitute of foundation. The Swedish authorities further state that the only way to approach the Pole is that proposed by the English Arctic officers, of exploring on sledges in the spring. Here, then, are the results for which the First Lord of the Admiralty in 1865 desired to wait. After a review of the voyage of the Austrian Lieutenants Payer and Weyprecht last summer, in which they found open sea a little to the north and west of Nova Zembla, and which discovery is to be followed up by a second expedition in the present summer, Capt. Osborn concluded by an eloquent appeal to the English people not to allow the final laurels of Polar discovery to be wrung from them by the sailors or explorers of any other nation. In the discussion which followed, Dr. J. D. Hooker spoke of the important questions in the science of botany which a North Polar Expedition alone could elucidate; such as the extension nearer the Pole of fossil plants like those of Disco in Greenland, which indicate a former temperate climate in 70° north. Dr. Carpenter advocated a Polar Expedition as a necessary complement to the one the Government were about to despatch to the Pacific to investigate the deep-sea ocean currents, and so forth. Accurate investigations of current-temperature, &c., of the Polar Ocean were of the highest importance to the right comprehension of the true theory of oceanic movements. Admiral Sir George Back stated that he entirely approved of the Smith Sound route as the one best to be adopted for a North Polar Expedition. Sir Leopold M'Clintock also spoke to similar effect. Admiral Richards explained the interest attaching to the completion of the geography of Greenland, which ought to be achieved by the English. He was strongly of opinion that a Government expedition, and by the English, was alone competent to finish the work of Arctic discovery. Mr. R. H. Scott read a letter from Von Nordenskiold, in which he stated that a Swedish expedition would start for Spitzbergen this summer, winter in the islands to the north, and attempt a journey towards the Pole in May, 1873, with reindeer-sledges.

Anthropological Institute, April 8.—Sir John Lubbock, Bart., president, in the chair. Mr. Hyde Clarke read a note on the Hamath Inscriptions. The remainder of the evening was occupied by an exhibition and description, by Mr. Edward Charlesworth, of certain objects from the Crag of Suffolk simulating human workmanship. A long and animated discussion ensued, and the question was postponed until such time as Mr. Charlesworth could lay before the Institute, in the form of a paper, his matured opinion based upon reliable evidence.

April 22.—Dr. Charnock, vice-president, in the chair. Mr. Hyde Clarke contributed a further note on the Hamath Inscriptions and their comparison with Himyarite and Lybian.—A paper by Dr. Barnard Davis, F.R.S., was read "On the Hair and some other peculiarities of Oceanic Races." The paper was illustrated by a large and beautiful series of specimens of hair showing all the varieties of dressing, ornamentation, preparation, bleaching, &c., employed by a great number of races and tribes.—Dr. Henry Blanc also exhibited a specimen of long hair from the head of a Hindustanee.—A paper by Dr. Rink "On the Descent of the Esquimaux" was read, in which the author showed from traditionary and historical evidence that that race was truly American, and not Asiatic in its origin, as some ethnologists had maintained.—Dr. Charnock read a paper "On Le Sette Communi." The district lay nearly north of Vicenza. The people were the remnants of those Germans who obtained an asylum in that country after having been vanquished by Theodoric, King of the Ostrogoths, who died A.D. 526. There had been many marriages with the Italians, and the people more resembled the latter than the Germans There were, however, many with fair hair and German features. The people were simple in their manners, honest, poor, dirty, and superstitious. The author noticed no cases of goître or cretinism. The paper concluded with a vocabulary and ample remarks on the grammar of the dialect, which resembled the Hochdeutsch of the 13th century, still spoken in Southern Bavaria. It had some words from the Italian.

Meteorological Society, April 17.—Dr. Tripe, president, in the chair. A paper was read "On the Temperature of Hill and Valley," by Mr. G. Dines. The observations in the valley were made at Cobham, and those on the hill at Denbies, the difference in height being about 600 feet; both the thermometer stands are those known as "Glaisher's," and the instruments are by Casella. The observations extend over eighteen months. The air on the hill is colder in the day and warmer at night than in the valley; and the daily range of temperature at the higher station is not so great as at the lower, the average being only about 4½°. In cold weather it is found that the air on top of the hill is never so cold as that in the valley. The rainfall also on the hill is 40 per cent. greater than in the valley. It has been said that "the air on top of a hill is drier and colder than in the valley," but the results arrived at in this paper show that the contrary is the case. In the discussion which followed, Mr. Glaisher said that he had always found in his balloon ascents that the temperature decreased as he ascended, and was colder and more uniform the higher he went, but at night he found that the temperature was warmer than on the ground, and it was this that led him to place thermometers at the height of 4 ft., 22 ft., and 50 ft. above the ground, and the results obtained show that the air is sometimes 5° or 6° colder at 50 ft. than at 4 ft. in the day time, and 3° or 4° warmer at night time. Colonel Strange said that the temperature was colder on mountain tops both in day and night than in the valley. Dr. Mann thought that the temperature of the air directly above the earth in a balloon, and the temperature on top of a hill at the same height, would be quite different. The Rev. F. W. Stow had made several observations which showed that the air was warmer at the upper station and colder at the lower. Mr. Strachan remarked that unless the thermometers were protected from radiation the readings would be too high. Mr. Gaster said that solar radiation was out of the question, because the air is colder on top of the hill than in the valley when the sun is shining, and warmer at night when it is not shining, and he thought the more abrupt the hill the more would the observations coincide with those taken in a balloon at the same height.—The next paper was by Mr. C. O. F. Cator "On Certain Defects in Anemometric Registration." The author said that correct records cannot be obtained by the present method, that the sheets should be much longer, and made to move more quickly. A correct register of the velocity of the wind is not obtained because the cups cannot take up the motion directly at each gust, and in a lull the cups revolve too quickly on account of the momentum received from the previous gust. In registering the pressure of the wind, the sheets and the scale should be longer, because at present the very small amounts are scarcely shown, and in gales the paper is completely black from the constant movements of the pencil, but if the sheet were moved more quickly each separate pressure might be recorded.

PARIS

Academy of Sciences, April 22.—M. Camille Jordan read a note on the forms reduced from congruences of the second degree.—M. de Saint-Venant presented a paper on a complement to be given to one of the equations presented by M. Levy for plastic movements which are symmetrical around an axis.—A note was read by M. J. Montier on the internal work which accompanies the escape of a gas without variation of heat; and one by M. P. Desains on the reflection of heat at the surface of polished bodies.—Several notices more or less closely relating to auroras were read, namely, a note by M. Fron, presented by M. Delaunay, on the auroral period from the 10th to the 16th April, 1872, and its relations to the movements of the atmosphere; a portion of a letter from M. Donati to M. Delaunay, relating chiefly to phenomena of terrestrial magnetism observed during the time of manifestation of auroras; a claim of priority in proposing the theory of the solar origin of magnetic auroras, by M. H. Tarry; a continuation of M. J. Silbermann's paper on the relations existing between terrestrial meteorology and the movements of celestial bodies; and a note by M. Duponchel, in which that gentleman ascribes the origin of auroras to the modification of the calorific waves after sunset; as these then cease to traverse the atmosphere and become tangential with it, especially in the neighbourhood of the poles, he supposes them to produce there effects of light and perhaps of electricity.—M. Bellanger forwarded a note on the change which takes place in the boiling point of water when mixed with more volatile fluids.—M. Faye presented a note on the photographic studies of the sun which have recently been undertaken at the observatory of the Infante Don Luiz, at Lisbon.—A letter from Father Secchi on some peculiarities of the constitution of the sun was read; as also a note by M. W. de Fonvielle on the hypothesis of the magnetisation of the sun.—M. V. Raulin presented a note on the pluviometric observations made at Athens from 1859 to 1871, including tables of monthly mean rainfalls and other data.—A note by M. de Lafollye, on a mode of determining copper by means of cyanide of potassium, was read. This is a volumetric process performed by adding a standard solution of cyanide of potassium to a solution of a salt of copper coloured blue by ammonia, until the complete decolorisation of the latter.—M. E. Ferrière presented a note on the action of sulphuric ether upon iodides, in which he states that, by the addition of sulphuric ether to the solution of an iodide, the iodine is gradually entirely set free. He ascribes this action to the slow but continuous formation of an unstable hydriodic ether.—M. Wurtz communicated a note on the synthesis of orcine, by MM. G. Vogt and A. Henninger. This body was obtained by the authors by the action of potash in fusion upon the sulpho-conjugate acid of chlorinated toluene.—M. C. Robin presented a note by M. Rabauteau, on the physiological properties possessed by the various proximate principles of opium.—A note by M. Sanson, on the hybrids produced between the hare and the rabbit, was communicated by M. Milne-Edwards. The author stated that the production of fertile hybrids between these two species had certainly been effected in 1868 by M. E. Gayot, who had furnished him with individuals of the sixth generation, representing two varieties which have been established and called by the breeder the *Léporide ordinaire* and the *Léporide longue-soie.* The former is identical with the rabbit in its specific characters; the latter closely approaches the hare.—M. Milne-Edwards also presented a note by M. A. F. Marion on Rotatoria parasitic upon *Nebaliæ*. The author noticed the occurrence upon *Nebalia straussii* of a *Saccobdella* distinct from that observed by Van Beneden and Hesse on *N. geoffroyi.*—A note by M. H. Sicard on the respiratory apparatus of *Zonites algirus* was also presented by M. Milne-Edwards; as also a note on the termination of the vertebral column in the Pleuronectidæ by M. H. E. Sauvage.—M. de Quatrefages communicated a note by M. E. T. Hamy on the proportional development of the humerus and radius in man, in which the author noticed the relative lengths of these bones at various ages, and gave a table showing the gradual and very considerable diminution in the proportional length of the radius to the humerus on reaching the adult condition.—M. F. Garrigou presented a note on the unity of composition of the Pyrenees proper, and the chain commonly called the *Petites Pyrénées.* He maintained that no distinction can be drawn between them.—M. de Quatrefages communicated an extract from a letter by M. E. B. des Essards on sea shocks.

VIENNA

Imperial Academy of Sciences, March 7.—Prof. E. Hering, of Prague, transmitted a memoir by M. F. Hofmeister, containing investigations upon the connective tissue in the testes of the Mammalia.—Prof. V. Graber, of Graz, transmitted a preliminary report on the propulsory blood apparatus of insects, and on the occurrence of a true elastic fibrous net in the Hymenoptera.—Prof. L. Gegenbauer, of Krems, forwarded a memoir entitled "The Universalised Dirchlet's Integral;" and Dr. A. Boué delivered a discourse upon geological chronology.

March 14.—Prof. L. Gegenbauer transmitted a memoir on Definite Integrals.—Dr. F. Wallentin communicated a memoir on the Serial Development of Functions, and its employment in Algebraic Analysis, as well as in the Integration of Differential Equations.—A paper on the Destruction of the *nervus facialis* and its consequences, by M. Schauta, was presented by Prof. Brucke. The experiments were made on two young rabbits.—Prof. von Lang communicated a note by Prof. Handl, on the absolute intensity and absorption of light.—M. E. Priwoznik presented two communications, of which the first contained a chemical investigation of the coating formed upon an antique bronze implement found in an old Celtic grave, near Hallstatt; and the second the results of a series of experiments made upon the formation of the sulphides of copper, silver, tin, nickel, and iron, to which the former investigation gave rise. The crust which covered the bronze consisted of three layers, of which the outermost and thickest was formed of blue sulphide of copper; the second was blackish-gray, and composed of disulphide of copper with 15 per cent. of tin; and the third, or innermost, was formed of a black powder containing 23·2 per cent. of tin, together with the accidental constituents of the bronze, arsenic, antimony, and nickel.

BOOKS RECEIVED

ENGLISH —Corals and Coral Islands: J. D. Dana (S. Low, Son, and Co).—Introduction to Biology: H. Alleyne Nicholson (Blackwoods)—Natural Philosophy: E Atkinson (Longmans)—Mountaineering in the Sierra Nevada: C. King (S Low. Son, and Co)—Lecture Notes to Chemical Students, Vol. ii, Organic Chemistry: E. Frankland. 2nd edition (Van Voorst)—The Principle and Practice of Canal and River Engineering: D. Stevenson. 2nd edition (A. and C Black).

AMERICAN.—Gateways to the Pole: S. Bent (Studley, S. Louis).

DIARY

THURSDAY, MAY 2.

ROYAL SOCIETY, at 8.30.—On a new Great Theodolite (illustrated by the instrument) to be used on the Great Trigonometrical Survey of India, with a short Note on the Performance of a Zenith Sector employed on the same work: Col. Strange, F.R.S.—On some Elementary Principles in Animal Mechanics—V. and VI.: Rev. Prof. Haughton, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—Exhibition of Early Christian Rings: C. D. E. Fortnum, F.S.A —Vortigern, not Hengest, the Invader of Kent: H. C. Coote, F.S.A.—Excavations of Tumuli at Trevalga: W. C. Borlase, F.S.A.

LINNEAN SOCIETY, at 8 —On *Alibertia edulis*: Señor Correa de Mello.

CHEMICAL SOCIETY, at 8.—On the Manufacture of Iron and Steel: E. Riley.

ROYAL INSTITUTION, at 3 —On Heat and Light: Prof. Tyndall, F.R S

FRIDAY, MAY 3

ROYAL INSTITUTION, at 9 —On Optical Phenomena produced by Crystals when submitted to Circularly Polarised Light: W. Spottiswoode.

GEOLOGISTS' ASSOCIATION, at 8.—On Columnar Basalts: J. Curry —On a Visit to the Diamond Fields of South Africa, with Notices of Geological Phenomena by the wayside: J. Paterson.

SATURDAY, MAY 4.

ROYAL INSTITUTION, at 3.—The Star-Depths: R. A. Proctor.

GOVERNMENT SCHOOL OF MINES, at 8.—On Geology: Dr. Cobbold, F.R.S.

MONDAY, MAY 6.

ENTOMOLOGICAL SOCIETY, at 8.—On the Longicorn Fauna of Nicaragua: H. W. Bates.

ANTHROPOLOGICAL INSTITUTE, at 8.—Peculiarities of the Australian Cranium: Mr. Bradley.—A Scaphoid Skull: Dr Barnard Davis, F.R.S —The Basque Race: Rev W. Webster and Mr Menteath —Marm, its Names and their Origin: Mr. Jeffcott.—Queensland Dialects: Mrs. Barlow.—Preservation of Australian Dead: Mr. M'Donald.

ROYAL INSTITUTION, at 2.—General Monthly Meeting.

TUESDAY, MAY 7.

ZOOLOGICAL SOCIETY, at 9.—On Dinornis (Part XVIII) containing a description of the pelvis and bones of the leg of *Dinornis gravis*: Prof. Owen, F.R.S.—Appendix to a List of Birds known to inhabit the Island of Celebes: Viscount Walden.—On the habits of the Swallows of the genus *Progne*, met with in the Argentine Republic, with notes by P. L. Sclater: W. H. Hudson.

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.

ROYAL INSTITUTION, at 3.—On the Development of Belief and Custom amongst the Lower Races of Mankind: E. B. Tylor, F.R.S.

WEDNESDAY, MAY 8.

GEOLOGICAL SOCIETY, at 8.—Notes on Atolls or Lagoon Islands: S. J. Whitnell —On the Glacial Phenomena of the Yorkshire Uplands. J. R. Dakyns.—On Modern Glacial Action in Canada: Rev. W. Bleasdell, M.A. —On a Sea-coast Section of Boulder Clay in Cheshire: D. Mackintosh.

SOCIETY OF ARTS, at 8 —On the Use of a Revolving Rabble in the common Puddling Furnace: F. A. Paget.

THURSDAY, MAY 9.

ROYAL INSTITUTION, at 3 —On Heat and Light: Prof. Tyndall, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.

MATHEMATICAL SOCIETY, at 8.

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THURSDAY, MAY 9, 1872

MENTAL DARKNESS IN HIGH PLACES

WE have long ago been told in the most forcible language and by the highest authority what will be the fate of a people whose leaders are blind; and the same authority has likewise informed us that the worst possible form of darkness is that in which we think we see. To have a one-sided, a short-sighted, or a distorted view of any principle or policy is frequently worse than not to see it at all, more especially in those who claim to wield the national power or to direct the national will. To have a short-sighted steersman is very bad, especially if the navigation be dangerous; but what must be the fate of the vessel if her steersman and look-out are both equally short-sighted?

It is at the present moment a point of vital importance that our leaders should be well informed as to the true nature of the claims advanced by Science for a better recognition by the Government of this country. Yet we question very much whether the leaders of Government have a just conception as to what is demanded by men of science, and why it is demanded, nor do we think that some of our leading journals are much better informed.

We quote the following extract from the *Times* of April 26, referring to a speech made by Mr. Gladstone at the annual banquet of the Civil Engineers:—

"'A fair field and no favour' is the maxim of English administration. A field so fair, so extensive, and so promising, that all industry may find its place, and such an absence of favour that one as well as another may hope for success. If under such conditions of Government the State 'does nothing for Science,' it cannot be helped, nor need it be much lamented, considering how very little Science stands in need of the aid."

We will do Mr. Gladstone and the *Times* the justice to own that these sentiments embody much that is true. To be meddling and muddling in private concerns is certainly not the province of any Government; and some think that the present Government were wrong when they undertook the management of the telegraphs.

The State should neither on the one hand attempt too much, nor on the other hand should it neglect to perform its obvious duties. What, then, are the principles which should guide a conscientious and intelligent Government as to its action in such matters? There can, we think, be no doubt as to these principles. If a certain course of procedure be for the obvious benefit of the whole people, and if its accomplishment be beyond the power of private associations, but not beyond that of Government, then surely it ought to be undertaken.

Let us test the truth of this maxim by one or two illustrations. It is for the obvious benefit of this country that it should have good steam communication with the continent of Europe and with America. Nevertheless our steam packets ought certainly to remain as they are, in the hands of private companies. Natural laws may here be left to themselves, and they will doubtless work in such a manner that the companies will on the one hand receive a handsome profit, while the public will on the other hand be supplied with efficient steam communication.

Again, it is of great importance that the country should be well furnished with animal food, and here, as before the task of supplying it may with advantage be left to private enterprise.

But in connection with this supply, we come to a case in which Government have very properly interfered. It is very important that the meat should be good and wholesome, and that diseased cattle should not be imported. On the other hand the people themselves, apart from Government, have no power of stopping the importation of such cattle, and therefore Government have very properly come forward and lent their aid in securing to the people a thoroughly wholesome supply of animal food.

It will at once be seen from these and similar instances, that legislative interference is uncalled for wherever natural laws are at work to perform the required objects.

Such natural laws are in operation in all, or nearly all, of the arts and industries of life. To meddle with carpenters, or bricklayers, or shoemakers, is entirely beyond the province of Government. If a man has a genius for improving shoes, he must not expect Government to start him in business; but he must look around for the co-operation of a capitalist; in fact, he must carry his genius to market and dispose of it to the highest bidder.

But what if the man have a genius for discovering natural laws? Will Mr. Gladstone or the *Times* be good enough to indicate the whereabouts of the market in which his genius will be rewarded? We have just been told that it is not at the Treasury; well, but where is it? Or will they tell us that such a discovery will never be of any practical advantage? Hardly so; the time for saying such things is past. It will in all probability be of immense importance to all industries, and they will all derive much profit in consequence of this man's discovery; yet he himself will derive none.

But we need not here attempt to prove that the advancement of Science is a question of national importance. This has been already demonstrated very conclusively by Col. Strange and others who have recently devoted much attention to the subject. We pass on to consider whether its advancement can be undertaken by private bodies, such as the Royal Society or the British Association.

The recent actions of these bodies speak for themselves. The former has just discontinued a series of sun observations taken under its superintendence; while the latter has given up the maintenance of the Kew Observatory—both on the plea of want of funds. Again, the maintenance of meteorological observatories has already been undertaken by Government as a thing beyond the means of private individuals.

In all directions the spread of Science is cramped by this want of money; to illustrate which we shall conclude by giving a short account of the recent attempt made by the British Association to establish a series of electrical standards on a scientific basis. One of the most important, and at the same time most difficult, determinations was that of the unit of resistance. In order to establish this unit upon the principles proposed by Weber and Thomson, it was necessary to associate together in an experimental investigation a scientific electrician, a mathematician, a metallurgist, and the director of a magnetic observatory. It was necessary first of all to determine the best kind of wire in which to embody the

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standard, and this required numerous experiments by a metallurgist; it was necessary to know what changes took place in the magnetism of the earth during the experiments, and this required the attendance of the director of a magnetical observatory; a scientific electrician presided over the experiments, and associated himself with a mathematician who was well versed in the theory of electricity. The unit of resistance thus determined by the British Association has now been universally adopted by practical engineers; men of science have laboured, and the Postmaster-General has quietly entered into the fruits of their labour; but the experiments in connection with other units are not yet finished; in fact such researches, requiring as they do great skill and time for their accomplishment, must necessarily hang fire if the men who can perform them do not receive some support which will enable them to devote their best energies to the conduct of these and similar experiments.

We have now said enough to establish our point that the extension of Science is of national importance, and that in its present state this extension is beyond the means of private individuals, but not beyond the means of the State. Before concluding, we ought to mention (more especially since it cannot be gathered from the *Times* report) that one of our most distinguished men of science, Dr. Joule, was present at the banquet to which we have alluded, and in returning thanks for Science took the opportunity of stating that he trusted Science would soon obtain that recognition which it imperatively required. If men of science will be true to themselves and to their noble cause, we feel confident that sooner or later they will prevail.

HOOD ON BONE-SETTING

On Bone-Setting. By Wharton P. Hood, M.D., M.R.C.S. (Macmillan and Co., 1871.)

TO any but the professional reader this title is not attractive; and yet we are greatly mistaken if the book itself does not prove to the full as attractive and as lastingly interesting to the intelligent non-professional as to the professional reader; and this, not because the subject is lowered to the level of general comprehension, or written in what is called a "popular" style, but simply because the subject itself is of such wide and varied interest, and its whole treatment in the present little volume is so frank, so clear, and so convincing.

It will be asked, What is bone-setting, who are the bone-setters, and who are their patients? And it will be readily answered, Why, of course, bone-setting is the art of setting bones that have been broken, or joints that have been dislocated, and this is done, doubtless, by surgeons; and equally doubtless and of course, their patients are persons whose bones are fractured, or whose joints are dislocated—

> There needs no ghost come from the grave to tell us that.

Perhaps not, but the answer is quite wrong for all that; quite the reverse indeed of what is actually the case, for bone-setting is *not* the art of re-setting broken bones or dislocated joints; bone-setters are *not* surgeons, or regular practitioners in any sense of the title; and their patients, even when they have suffered injury to joint or bone, have been pronounced by the regular practitioner *cured* before seeking the help of the bone-setter.

Having stated this triple paradox, let us hear what Dr. Hood has to say in explanation.

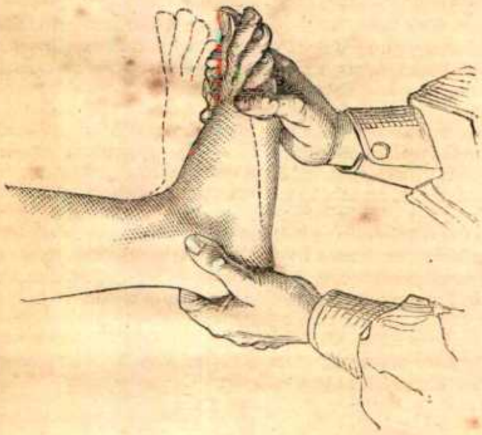
"A healthy man sustains a fracture of one or both bones of the fore-arm, and applies at a hospital, where splints are adapted in the usual way. He is made an out-patient, and the splints are occasionally taken off and replaced. After the lapse of a certain number of weeks the fracture becomes firmly united, the splints are laid aside, and the man is discharged as cured. He is still unable to use either his hand or his fore-arm, but is assured that his difficulty arises only from the stiffness incidental to long rest of them, and that it will soon disappear. Instead of disappearing, however, it rather increases, and in due time he seeks the aid of a bone-setter. . . . The bone-setter would then by a rapid manipulation, hereafter to be described, at once overcome the stiffness of the fingers, and enable the patient to move them to and fro. The instant benefit received would dispel all scruples about submitting the wrist and the elbow to manipulation, and these would be set free in their turn. The man would go away easily flexing and extending his lately rigid joints, &c."

Now what was the cause of stiffness in the foregoing typical case? What was the nature of the impediment to normal movement, ignored by the surgeon and overcome by the bone-setter? The impediment above indicated, it is argued, might arise from articular inflammation, producing adhesions between surfaces "resting in apposition," and that such adhesions, if so placed as to restrain movement, will cause pain and irritation whenever they are rendered tense; and, moreover, that inflammation sufficient to produce these adhesions may be insidiously set up in a joint by extension from neighbouring structures—as in the above-recited case. Again, it is argued, "possibly in some cases, the proper ligaments may become contracted or rigid, or adherent to neighbouring parts; in others, internal or external adventitious fibrous bands may be formed; in others, muscles may have undergone shortening. Again, effusion may have become solidified, and thus movement be impaired, as if by a state of things analogous to a rusty hinge." And further on: "If we consider the amount and character of the effusion which takes place after some sprains and injuries, in some gouty and rheumatic affections, and in some cases of suppuration occurring in bursa, or beneath deep fascia, we cannot doubt that such effusion may easily assume forms in which it will tie down muscles, tendons, or even articular nerves themselves."

The art of bone-setting, then, is the art of overcoming these impediments in joints, these conditions of arrested or impaired freedom which not unfrequently supervene on the curative processes of treatment in use by surgeons in cases of fracture or dislocation; or which may arise from, and be observed only after the subsidence of, active rheumatism, gout, ganglionic swellings, or other local affections; and this brings us to the question, How is it done? how are these stiffened joints set free? how are these impediments to healthy action overcome? The answer of the regular practitioner is that which has been already quoted, namely, to rest it—advice which usually entails a distressing failure; the answer of the irregular practitioner, *i.e.*, the bone-setter, is precisely the opposite, namely, that freedom can only be restored to the stiffened joint by movement, by manipulation, and manipulation too of the most formidable kind, nothing less than suddenly and forcibly rupturing, tearing asunder, the adhesions formed between the articulating surfaces of the affected joint, an operation which is so frequently successful that it forms the very basis of the bone-setting craft.

It is here that the bone-setter steps in front of the scientific surgeon, and we must confess to a feeling of disappointment that their relative positions are not reversed, that the surgeon is not called in to rectify the malpractices of the quack, instead of the latter being sought out to complete the shortcomings of the former. Let us see how this manipulation is performed. The bone-setter has a clearly defined system of treatment for each separate joint, if not for each specific affection to which each joint is subject. Next to the list of authenticated cases of successful treatment, this is, perhaps, the most valuable part of the treatise; for in addition to the ample and detailed modes of procedure with each joint, diagrams showing the act of manipulation are given, taking in succession those of the upper and lower limbs, and also of the several regions of the spinal column. One example may here be given :—

> "The proximal side of the affected joint being firmly held, and the thumb-pressure made in the ordinary way, the tarsus is so grasped as to give the greatest attainable leverage, the foot twisted a little inwards or outwards, then sharply bent up upon the leg and again straightened. As a rule it is desirable to execute this manœuvre twice over with an inward and once with an outward twist, and also to take care that the movements of the joint are free in all directions."



Bone-setters, we are told, are for the most part uneducated men, wholly ignorant of anatomy and pathology; but we are not told what we greatly wish to know, and that is, the manner and method in which the secrets, the mysteries, and the traditions of their craft, are communicated to each other. No doubt there exists a freemasonry in the craft, so that when individual members meet revelations are made and notes compared, but we are not informed of any regular or organised system of instruction, either for the maintenance and extension of the craft, as a craft, or for the enlightenment of the separate and detached members of the fraternity. The most celebrated, we may even say distinguished, bone-setter of our day was the late Mr. Hutton, whose successful treatment of cases that had baffled the skill of the foremost surgeons now living, cases related in detail by Dr. Hood, and about the accuracy of which there can be no question or doubt, is little short of marvellous; and the question is ever recurrent while we read, "How and where was this skill acquired?" for a bone-setter of Mr. Hutton's calibre could put his finger on the spot where lurked the seat of an affection that had crippled a patient for half-a-dozen years, and had defied the scientific treatment of the ablest surgeons of our time; nay, he could point to this spot without ever seeing the limb affected, guided merely by observing the attitude, gait, or action of the patient. Now, whence comes this undoubted skill of these illiterate men? It appears to be obtained solely by observation of symptoms and results of treatment, the accumulated knowledge of from day to day experience; and as we often see that one sense is quickened and functional power increased by the loss or impairment of some other sense, so perhaps the narrowing of the field of instruction, the limiting of the sources of information, may have intensified the powers of observation of the bone-setters, atoning in a measure for the absence of the revelations of science.

It was from Mr. Hutton himself that Dr. Hood received the secrets of the craft. The motive for, and the manner of, making this revelation are so interesting, that we must give them in Dr. Hood's own words :—

> "About six years ago my father, Dr. Peter Hood, in conjunction with Dr. Iles, of Watford, attended the late Mr. Hutton, the famous bone-setter, through a long and severe illness. On his recovery my father refused to take any fees from Mr. Hutton, out of consideration for the benefit which he had rendered to many poor people. Mr. Hutton expressed himself as being thereby placed under great obligation, and as being very desirous of doing something to show his gratitude. He offered as an acknowledgment of the kindness he had received to explain and show all the details of his practice as a bone-setter. Pressure of work prevented my father from availing himself of this offer, and Mr. Hutton then extended it to me. After some consideration I determined to accept it, and accordingly I went, when I could spare the time, to Mr. Hutton's London house, on the days of his attendance there. My decision was prompted, not only by the curiosity I felt to see how he treated the cases that came under his care, but also by the desire to make known to the profession, at some future time, any insight that I could gain into the apparent mystery of his frequent success."

By this means Dr. Hood in time found himself, as he tells us further on, able to take charge of all Mr. Hutton's poorer class of patients, "whom he was accustomed to attend gratuitously," and found that he could accomplish all that he had seen done. "And this practice," he elsewhere emphatically says, "gave me knowledge of a kind that is not conveyed in ordinary surgical teaching—that when guided by anatomy is of the highest practical value, as well in preventive as in curative treatment."

With this knowledge, thus acquired, the author's course was clear. In a series of letters in the *Lancet* he communicated to his professional brethren some of its most important facts, and now to the general reading public he submits the whole in the shape of a well-arranged and clearly-written volume.

ARCHIBALD MACLAREN

OUR BOOK SHELF

Worms; a Series of Lectures on Practical Helminthology, delivered at the Middlesex Hospital, by T. Spencer Cobbold, M.D., F.R.S. (London: Churchill, 1872.)

THESE lectures do not pretend to give any very minute anatomical details, or any full account of the life history of the remarkable group of animals of which they treat, and which Dr. Cobbold has so long and so carefully investigated. They were originally delivered to medical students, but are so simply and clearly written that they might advantageously be read by the public. They show the frequency with which parasites occur in man, and the necessity of careful supervision of the animal food exposed in our markets for sale, especially at night and to the poor. Dr. Cobbold remarks that the terms "measly mutton" and "measly beef" are terms which will sound strange to those who know of no other "measled meat" than pork; but he points out that his investigations have incontestably proved and verified the existence of larval tapeworms in the most esteemed kinds of animal food. The tapeworms derived from these three kinds of meat, beef, mutton, and pork, though agreeing in their general characteristics, yet differ in minor points, and especially in the shape of their heads. The head of the beef tapeworm is destitute of hooks, and has four large suckers, besides a supplementary fifth (so called); whilst the head of the pork tapeworm is a trifle smaller, and furnished with a slightly prominent proboscis, armed with a double row of hooks. The mutton tapeworm is also armed, at least the "measle" is provided with hooks. A fully-developed beef tapeworm numbers about eleven hundred joints, and attains its full development in about thirteen weeks or rather less. Dr. Cobbold appears to regard the well-made ethereal extract of the root of male fern as by far the best remedy for tapeworm, though kousso, kamala, turpentine, panna, pumpkin seeds, betel nuts, and the bark of the pomegranate, are occasionally successful in effecting their expulsion, and will sometimes accomplish this when the oil of male fern has failed. In regard to thread worms (*Oxyuris vermicularis*) Dr. Cobbold states that it is quite a mistake to suppose the lower bowel or rectum forms their special habitat. He recommends santorine, with active saline purgatives, and copious enemata, for their removal. The large, round worm, *Ascaris lumbricoides*, and the *Ascaris mystax*, are in his experience rare, the latter, indeed, very rare in England, but they are endemic in some regions, as in the Mauritius. The Trichina appears to have been only once recognised and treated in the human subject, namely, by Dr. Dickenson, of Worthington. In this instance Dr. Cobbold calculated that the patient played the host to forty million of the parasites. He observes that when once the Trichina has gained admission to our muscles, all hopes of dislodging it are at an end; but if a person suspects that he has eaten diseased or trichinised meat, he should lose no time in seeking assistance. Immediate advice, followed by a suitable antitrichinalic, might be the means of saving his life, whereas a few days' delay would perhaps prove fatal. Whilst the worms are in the intestinal canal we can get rid of them, but when once the trichinal brood migrates into the flesh no means are known by which their expulsion can be effected. The work terminates with some amusing cases of spurious worms.

H. P.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Recent Climatic Changes

IN two previous letters I have tried to show that the land is gaining on the sea at both poles; in other words that the periphery of the earth is being thrust out in the direction of its shorter axis. If this change has been so great as to make it probable that the Classical and Arabic accounts were correct when they made Scandinavia an archipelago in the earlier centuries of our era, we may be sure that it has not been without material influence upon other physical phenomena, and notably upon climate.

One obvious result of the conversion of a great space of water into an area of dry land in the high latitudes confined by the Arctic and Antarctic circles, would be to alter very materially the mean climate of both hemispheres. This has been noted by many inquirers into the subject, and not least by Mr. Hopkins in his very ingenious criticism of the different potential climatic revolutions which are consistent with the presence or absence of certain elements like the one we are dealing with. Ingenious and most interesting as Mr. Hopkins's well-known paper in the *Geological Transactions* is, it does not, I think, quite explain facts as observed. I am not writing in emulation of one who has every claim to be considered an authority, but in the humbler guise of a student whom you are always willing to assist, who desires some of your correspondents to throw light on a difficult and neglected question.

The increase of land at the poles at the expense of the water will tend to intensify the extremes of temperature in winter and summer, and in consequence, to make the climate much less constant and uniform and much more severe; and we ought to find evidence of this somewhere, if the premisses of my two previous letters are tenable. I wish to adduce a few facts in the way of such evidence, and to ask your correspondents for more either pro or con. Many such must exist.

Greenland is a name which seems ironical under present conditions of climate. It has always seemed to me that the land there has changed its appearance very considerably since that name was applied to it. The Esquimaux were apparently not known as inhabitants of Greenland to the Saga writers. The skzellings they met with were on the coasts of Labrador and farther South. They first appeared after the black plague had nearly destroyed the Norse settlements, and they completed the work the pestilence had commenced. They came from the north, probably from the area now occupied by the so-called Arctic Highlanders. The Indians who now live along the march, or frontier, bounding them and the Esquimaux in North America, have an apparently uniform tradition, that the Esquimaux were formerly not neighbours of theirs, and that they came south across the sea from the islands beyond. I believe that I have sufficient facts by me to justify the opinion that the Esquimaux of both shores of Behrings Straits have been constantly drifting westwards and southwards, and that they are but recent occupants of their present area there. This will appear in a future communication to the Anthropological Institute. These facts are quoted to show that the Esquimaux race has been uniformly leaving its more northern habitat and seeking a more southern one. It is remarkable that the recent Swedish Expeditions to the Eastern Coasts of Greenland found abundance of reindeer and musk-oxen there in areas formerly uninhabited by both animals. This emigration must have come from the north. I can see no adequate cause for a revolution affecting men as well as other animals in such a uniform manner, except the continuously increased severity of local climates which has driven the inhabitants farther south.

Iceland has notoriously become more harsh and untenable in its climate since the days of the Norsemen. I will quote from a capital authority, Henderson's Journal in Iceland, pages 6 and 7:—"It is evident from ancient Icelandic documents that on the arrival of the Norwegians, and for centuries afterwards, pretty extensive forests grew in different parts of the island, and furnished the inhabitants with wood both for domestic and nautical purposes. Owing, however, to the improvident treatment of them, *and the increased severity of the climate*, they have almost entirely disappeared, and what remains scarcely deserves any other name than that of underwood, consisting for the most part of birch, willow, and mountain ash. That grain was produced in former times in Iceland appears from the names of many places, such as akkrar, akkrances, akkraheron, &c., the word *akr* signifying a cornfield, and from certain laws in the ancient code, in which express mention is made of such fields, and a number of regulations are prescribed relative to their division and cultivation." Grain is no longer raised there. The Black Death, and other reasons, have been adduced for this cessation; but these are clearly inadequate causes, the real reason being no doubt the same which has caused grain culture to be discontinued elsewhere, namely, the increased severity of the climate.

What is true of Iceland is also true of Norway, in the most northern parts of which we find many names compounded with the Norse word for barley, proving, as the best authorities agree, that barley then grew where it grows no longer. In Scotland many places show signs of the plough, and of having been sown with cereals where arable farming is now unpractised. It is notorious that not only in Scotland, but even in England as far south as Lancashire, large districts that were once covered with forests are now entirely bare of trees, and not only so, but trees cannot be made to grow there. "The Romans planted vineyards and made wine in parts of England where the hop will now hardly grow."

In Northern Russia beyond the Dwina there is a vast area, formerly known as Biarmia, studded with the graves and other remains of a very prosperous people, whose wealth and civilisation are much descanted about by the Saga writers. Othere, the navigator, whose story was translated by Alfred, tells us that it was on arriving in their country after the dreary voyage round the North Cape, that he first again met with tilled fields and an agricultural race. This area is now deserted except by a few hunters and fishermen; the ancient inhabitants have moved westward and southward into Finland, &c. I have described the migration in a paper to be printed by the Anthropological Institute. The best authenticated case of this desolation is the increased severity of the climate, which makes agriculture almost unendurable there. The Norse traders used to frequent Cholmogorod, the port of Biarmia, in great numbers, both for traffic and for fishing. This navigation continued until the early part of the thirteenth century, when we are told it was gradually put an end to by the increased difficulties with the ice in the White Sea, which becomes practically choked with ice; and when the English found their way to Archangel in the sixteenth century, so forgotten was this old trade, that the journey was treated as one of discovery.

Farther east facts are less accessible. The following quotations from von Wrangel's voyage illustrates my position:—

"In 1810 Hedenstrom went across the tundra direct to Utsjansk. He says, 'On the tundra *equally remote from the present line of trees* among the steep sandy banks of the lakes and rivers, are found large birch trees complete with bark, branches, and roots. At first sight they appear well preserved, but on digging them up they are found to be in a thorough state of decay. On being lighted they glow, but never burst into flame. The inhabitants use them for fuel. They call them Adamoushtshina (*i.e.* of Adam's time). *The first living birch trees are not now found nearer than* 3° *to the south, and then only as shrubs.'* . . . . . 'Another cliff, 30 or 35 feet high, beyond the Malaya Kurspataschnaja river, consists of ice, clay, and black earth. On drawing out some interspersed roots we found them to be birch, and as fresh as if only just severed from the trees. *The nearest woods are* 100 *versts off.'*" These facts show how far to the south the limit of trees has been pushed quite in recent times in Siberia, that is, how much more severe the Siberian climate has become—a fact, perhaps, connected with the persistent south-westerly drifting of Ugrian tribes from this area which has taken place during the historic period. The flora of our own bogs must disclose evidences of some kind on this subject. I should be thankful to any of your correspondents for facts which illustrate the question drawn from this or any other source.

Will you allow me to correct two printer's errors in my last letter. Africa has been inserted instead of Arica, and Mine Journal instead of Same Journal, *i.e.*, Journal of the Geological Society. Both errors due to my execrable writing.

HENRY H. HOWORTH

A New Mode of taking Casts

IT has been suggested to me that an account of "A New Mode of taking Casts," reported in NATURE, May 2, 1872, might convey the impression that I claimed any share in the invention. Modelling wax has been employed for this purpose in France by the late M. Edouard Lartet, and I have to thank Prof. Busk, F.R.S., for having first brought the use of the material before my notice. My sole object was to formulate the process according to my own experience of its utility, that it might be employed by others for the multiplication of type specimens, without any idea of claiming its invention.

W. BOYD DAWKINS

Norman Road, Rusholme, Manchester, May 3

The Denudation of the Mendips

IN the address of the President of the Geological Society of London reported in NATURE No. 129, page 490, it is stated that "Denudation has removed from the crest of the Mendips a mass of strata possibly equal to two miles or more in height, and from that of the Ardennes as much as three or four miles."

Could you find space to inform me by what reasoning or on what data geologists arrive at such a conclusion, and whether it is considered that the level of the surrounding district was raised in a corresponding degree, or that the Mendips were isolated mountains, somewhat like the Peak of Teneriffe, rising abruptly from the plains below? INQUIRER

P.S.—Perhaps by inserting this letter and reply you may convey information to others as well as myself.

Segmentation of Annulosa

MR. RAY LANKESTER, in his letter on "The Segmentation of Annulosa" in NATURE of the 4th ult., appears to maintain that there is no fundamental difference between the segmentation of annelids and that of chitons. He says of the latter, "Why should there not be segmented molluscs?" I can no more answer this than I could answer the question why there should not be hexapod vertebrates. There is nothing impossible or absurd in the idea of the segmentation of chitons being essentially similar to that of annelids; the question is whether it is really so. He says, "The larva of a chiton is (in appearance) identical with that of an annelid, and its segmentation makes its appearance in the same way." This is an important argument, but not conclusive. If, as I believe, Mr. Spencer's theory of the origin of the annulosa is true, the segmentations of the two are fundamentally distinct phenomena, and it might appear that their development should be unlike. According to that theory, the segments of the ancestor of the annulosa had their origin by budding backwards from the head; while the segmentation of the chitons has from the first been "superinduced" like that of the spinal column of vertebrates. But it is admitted that the annulosa do not in their actual development show decided traces of this origin. I think, however, this last fact may be explained by a principle which Mr. Spencer lays down somewhere, with great probability, as a general law (though of course it is a law of tendency only, and may be subject to exceptions), namely, that there is a general tendency to the substitution of direct development for indirect. Thus, the segments of annulosa may have been originally formed one after the other by budding backwards, and yet may now be formed simultaneously, or nearly so, by the segmentation of a vermiform larva.

The opinion that the two cases are distinct is, I think, very strongly confirmed by the fact that annelids still propagate by budding off young annelids from their tails, while no mollusc whatever is known to do so. JOSEPH JOHN MURPHY

Brittany Dolmens and Tumuli

IN NATURE for May 2 Captain Oliver advocates the theory that dolmens are merely the skeletons of original chambered tumuli. This, I think, scarcely agrees with the fact to be observed in the principal dolmens and tumuli of Finistère. In most cases in that department the dolmens occupy situations in every respect similar to those in which the tumuli are found, so that meteorological, and, indeed, every other but human agencies, must have affected both in the same manner and degree. Notwithstanding this, the dolmens are invariably bare, and the kists are as constantly covered—there are no signs of even incipient degradation and denudation in the latter, and none of former covering in the first. It would be unwarrantable to suppose that had the dolmens been uncovered by human beings, no vestiges of the mounds would remain, or that, this perfect and unaccountable removal of material being allowed, the skeleton, *i.e.*, the part containing the most useful stones, should be left unscathed.

There is, however, a more important point of difference between dolmens and the barrow kists; namely, that in the chambered tumuli there is almost always present a floor-stone—a part of the structure which I have never seen at the base of any of the dolmens of the region in question. And there can, in their case, be no chance of removal, as the floor-stone would necessarily be the last to remain in its place. The dolmens, again, as a rule, were evidently erected with no attempt at nice adjustment of the sides or top, whereas tokens of some care and trouble are to be found in the way in which most of the entombed kists are built.

In these remarks I have limited my observations to those dolmens and covered chambers which are of the simplest grade, but they would, I think, be equally applicable to the more complicated structures similarly circumstanced. It seems, therefore, premature to apply the term "dolmen mound" to such barrows as Carnoët, for instance, where the small and accurately-made kist bears no resemblance to the frequent dolmens of the district.

Harbottle, Rothbury, May 4 G. A. Lebour

The University of Freiberg

May I be allowed to ask in your pages for information about the University of Freiberg in Saxony as a place of instruction in Geology, Mineralogy, and Chemistry? I should also be glad to know which German Universities are held to be the best for learning these three subjects, and when their terms end. Any information on these points will oblige,

Cambridge University, May 5 Undergraduate

Sources of Sandstone

Your geological readers will readily admit the importance of any hint that will assist them in determining the source from which a particular bed of sand or sandstone has been derived.

How to discriminate between sand produced by the breaking-up of quartz, and sand produced by the breaking-up of flint, does not appear to be generally understood. Chemical analysis gives no assistance; and when examined microscopically by ordinary light, no difference can be detected.

Polarised light, however, differentiates these two substances in such a decided way, that where they alone are concerned, no doubt can remain for an instant as to whether a grain of sand consists of one or the other. The quartz, as of course everybody knows, is resplendent with prismatic colours, while the flint shows a cold steel-grey surface covered with a peculiar marking, which I am obliged to call a species of reticulation for want of a term more exactly descriptive. This marking I consider to be indicative of something in the structure of the organism replaced by the flint. When once seen it is easily recognised. Some species of chert, that from the Portlandian beds for instance, show the same marking as flint.

I may also add that polarised light shows chalcedony to be, like quartz, a crystalline instead of a non-crystalline substance, as usually taught. It bears the same relation to quartz crystals and massive quartz as fibrous gypsum does to selenite and common gypsum. M. Hawkins Johnson

Polarised Light

We have all noticed that when the sun shines directly through a window hung with figured muslin curtains, the reflection of the pattern of the curtains in the window interferes with the prospect.

When this reflected image is viewed through a Nicol's prism it disappears when the prism is rotated, leaving the prospect unobstructed; the experiment is very interesting, and can be performed by any one who has a polariscope attached to a microscope, and it is only necessary to observe that the image is viewed at the proper angle. The effect will possibly be best when the sun's rays make an angle with the curtains and the glass nearly coinciding with the polarising angle. (In my case the angle was 36° 52'.)

Tyndall has mentioned a case in which the haze obstructing the view of a mountain-top was rendered transparent by the Nicol.

The readers of Nature have probably observed how completely the leaves of the ivy polarise light; viewed through the Nicol and a pink selenite, the plant appears covered with blossom.

R. S. Culley

CHOLERA AND SUN-SPOTS

On Friday evening, the 26th ult. Mr. B. G. Jenkins, of the Inner Temple, read before the Historical Society a remarkable paper on Cholera, founded on a communication to the Russian Imperial Academy of Sciences, and now under the consideration of the Medical Council of the Minister of the Interior. The author of the paper maintained that no true advance could be made in any science founded on experience, and looking to facts for its development, until the history of that science had been recorded and correctly interpreted; and that it was, because we have been looking at the facts of cholera, which have been accumulating for half a century, as facts without attempting to show, or rather without succeeding in showing, in what relation they stand to each other, that we are really no wiser than we were forty years ago.

He held that, instead of one "home" of the cholera in the delta of the Ganges, there are seven, all situated on or near the Tropic of Cancer, equally distant from each other, of which the most important is that at the mouth of the Ganges; the others are to the east of China, to the north of Mecca, on the west coast of Africa, to the north of the West India Islands, to the west of Lower California, and among the Sandwich Islands; that a reference to the map would show that the recorded appearances of cholera over the globe may be satisfactorily explained by supposing seven atmospheric streams, each 1,400 miles in breadth, to proceed from these foci in a north-westerly direction; and that at some periods, as 1833, 1850, and 1866, nearly all the streams were in activity.

Having pointed out the course of these streams on a map especially prepared, and shown how the disease moved within the limits of each, in both the north-west course and its south-east extension across the Equator, the author, in tracing in detail the course of the cholera in India during 1817 and 1818, called attention to a remarkable law which manifested itself, a law which he held was generally applicable wherever cholera appeared.

Although the course of cholera during 1817 was not very clear, still it is evident that it was north-west and south-west. The lull in virulence and advance which occurred in December 1817 continued to March 1818, when cholera broke out again just where it had ceased the previous December. He drew attention to a very recent similar instance: the cholera last year halted on the western border of Russia, and about a fortnight ago broke out in Poland, which augurs ill for the North of England this year. The remarkable law which the author pointed out was that in 1818 the cholera advanced simultaneously in two directions, north-west and south-west, in such a manner that all the places attacked at any given time by its north-west advance were situated at right angles to all the places attacked at the same time by its south-west advance. This double advance is made evident by cutting a piece of paper square, placing a corner upon the map at Calcutta, and moving it across India in a direct line to Surat. In 1819 the cholera crossed the Arabian Sea to Muscat, and passed simultaneously through Persia, and up to 1823 advanced as far as Asia Minor and the Caspian, and then died out. In 1823 a fresh outbreak occurred in India; this steadily proceeded to the north-west, and halted in the west provinces of Russia in 1830, and the next year broke out in full force in the same locality, thus presenting a parallel to 1871-2, and went as far as Britain. By referring to the map it will be seen that all places attacked by this stream of cholera in 1831 lie within the boundaries represented by two lines, one drawn from the southern point of India to the north of England, and the other from the Ganges through Orenburg to Archangel. The author having described with great minuteness the rise and progress of the other six streams, bringing the subject down to the present day, stated that Europe was liable to attacks from two great sources, India and Arabia: Russia and Northern and part of Central Europe coming under the influence of the Indian stream; Southern and Western and part of Central Europe under the influence of the Arabian; and that the Continent would certainly be attacked by both this year. The curious cases of ships at sea being suddenly attacked by cholera, and, again, the instances of ships sailing along the coast of India being struck by the disease when at the same place, he explained on the supposition that they had been sailing within the limits of the cholera streams; for when they got outside the limits the disease suddenly ceased. He called attention to a fact worthy of mention, that all the places recorded by Dr. Gavin Milroy as unaffected hitherto by cholera lie outside these streams, or in their possible, but not actual, extension.

Having stated that he was prepared to give, in another paper on the origin of the disease which he was preparing, an ample explanation of some well-known points about cholera; such as its partial connection with the east wind, its following the course of large rivers, its greater prevalence on tertiary strata, alluvial tracts, and the deltas of rivers, and its comparative rarity on secondary and primary strata, the author proceeded: "It was not my intention at the present time to enter into the question of the origin of the disease; but having read a few days ago that Dr. Buchanan in this very hall congratulated the meeting on being able to number among the things of the past the time when the propagation of cholera was supposed to be due to all manner of cosmic and atmospheric influences, and on having 'reached a solid basis of fact and knowledge upon which further observation might be built with security,' I am tempted to declare that I for one maintain that this despised theory, which Dr. Buchanan fancies is buried and put out of sight, is the correct one. I maintain that cosmic influence lies at the origin of cholera—that cholera is intimately connected with auroral displays and with solar disturbances. I believe that I am able to show that a remarkable connection exists between the maxima and the minima of cholera epidemics and of solar spots; and in directing your attention to this map, on which I have represented graphically the amount of cholera and the number of sun-spots for the last fifty years, I wish to show that there is here also 'a solid basis of fact and knowledge upon which further observation might be built with security.' You are all probably aware that the great astronomer Schwabe discovered that the sun-spots have what is called a ten-year period; that is, there is a minimum of spots every ten years. It was also discovered that the diurnal variation in the amount of declination of the magnetic needle has a ten-year period. The same was proved in regard to earth currents, and also auroræ. The maxima and minima of the four were found to be contemporaneous. This was a great result; but Professor Wolf, on tabulating all the sun-spots from the year 1611, discovered that the period was not ten years, but 11·11 years. This period is now the accepted one for the sun-spots, and it has been established for the magnetic declination, and by Wolf for the auroræ. Now, it is a curious fact that the last year of every century, as 1800, has a minimum of sun-spots, so that the minima are 1800, 1811·11, 1822·22, 1833·33, &c. The maxima do not lie midway between the minima, but anticipate it by falling on the year 4·77 after a minimum; for example, 1800 was a minimum year, then 1804·77 was a maximum year. Now, cholera epidemics have, I believe, a period equal to a period and a half of sun-spots. Reckoning then from 1800, we get as a period and a half the date 1816·66, which was shortly before the great Indian outbreak; another period and a half gives 1833·33, a year in which there was a maximum of cholera; another, 1849·99, that is, 1850, a year having a maximum of cholera; another, 1866·66, a year having a maximum of cholera; another, 1883·33, as the year in which there will be a cholera maximum. It follows from what has been already said that 1783·33 would be a year in which cholera was at a maximum. Now it is a fact that in April 1783 there was a great outbreak of the disease at Hurdwar.

"I would call attention to the parallelism of increase and decrease of these curves. I am not, however, prepared to say that sun-spots originate cholera; for they may both be the effects of some other cause, which may indeed be the action of the other planets upon the earth and upon the sun. If that be the case—and I see no reason why it should not—we may then have an explanation of the minor periods and of the large period of 56 years, which Wolf believes he has detected, and also of the minor periods observed in cholera-epidemics.

"My own opinion, derived from an investigation of the subject, is that each planet, in coming to and in going from perihelion—more especially about the time of the equinoxes—produces a violent action upon the sun, and has a violent sympathetic action produced within itself—internally manifested by earthquakes, and externally by auroral displays and volcanic eruptions, such as that of Vesuvius at the present moment; in fact, just such an action as develops the tail of a comet when it is coming to and going from perihelion; and when two or more planets happen to be coming to or going from perihelion at the same time, and are in, or nearly in, the same line with the sun—being of course nearly in the same plane—the combined violent action produces a maximum of sun-spots, and in connection with it a maximum of cholera on the earth. The number of deaths from cholera in any year—for example, the deaths in Calcutta during the six years 1865-70—increased as the earth passed from perihelion, especially after March 21, came to a minimum when it was in aphelion, and increased again when it passed to perihelion, and notably after equinoctial day; thus affording a fair test of my theory."

ON THE DEPTHS OF WATER IN WHICH WAVES BREAK

OBSERVATIONS MADE AT SCARBOROUGH IN 1870

As the force which different sea-works have to resist varies with the height of the waves that reach the coast line, any data which will enable the marine engineer to predict this height when designing such works must obviously be of importance. In the *Edinburgh Philosophical Journal* for July 1852, I stated, as the result of experiments made in 1850 on a small fresh-water loch, and afterwards on larger sheets of water, that the height of the waves increased most nearly in the "*ratio of the square root of their distances from the windward shore*," or, in other words, *the crest of the wave as it increases in height describes a parabolic curve.* So that, if h = height of wave, d = distance, and a is a coefficient varying with the strength of the wind, $h = a\sqrt{d}$. For most practical purposes of the engineer, the coefficient a may for heavy gales be taken at one and a half; so that the formula becomes—

$$h = 1{\cdot}5\sqrt{d},$$

where h represents the height of the wave in feet, and d = length of exposure in miles.[*] As elsewhere stated,[†] this formula becomes for short distances—say for under ten miles—

$$h = 1{\cdot}5\sqrt{d} + (2{\cdot}5 - \sqrt[4]{d}).$$

The height of the wave, however, which reaches any particular work, is not necessarily that which is due to the line of exposure; for the shallowing of the water near the shore may cause the heaviest waves to break, either partially or wholly, before they reach the work. Mr. Leslie found that at Arbroath Harbour the works were not so severely tried by the very heaviest class of waves as by others of lesser size, which were not *tripped up* by the outlying rocks. The same effect has also been observed at the river Aln, where the smaller waves occasion a greater "range" in the harbour than the larger ones which break in passing over the bar, and are thus reduced in height. The larger waves are not, then, always so destructive as the smaller. It becomes, therefore, a question of some moment to determine the maximum height of wave that is possible in a given depth of water.

Mr. Scott Russell, whose contributions to what may be called the marine branch of hydrodynamics are of such great value, has stated that "he has never noticed a wave so much as 10ft. high in 10ft. water, nor so much as 20ft. high in 20ft. water, nor 30ft. high in five fathoms water; but he has seen waves approach very nearly to those limits." Mr. Russell has not stated whether the depths of water referred to are those below the trough of the sea or below the still-water level. In my book on "Harbours" I gave three observations on short waves from 2ft. 6in. to 3ft. high, which corroborated Mr. Russell's statement, supposing him to refer to the depth below the hollow. But since that time I had an opportunity, during a N.E. swell in July 1870, of observing the depths in which waves of a larger class broke at the Promenade Pier at Scarborough, where the heights could be measured with very considerable accuracy on the iron piles and open sloping slip or grating at the seaward end of the pier; and the following are the results:—

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[*] Mr. Hawkesley, in the "Proceedings of the Institute of Civil Engineers," gives a formula which is satisfactory so far as it corroborates the law of increase which I had stated in the *Edinburgh Philosophical Journal* in 1852; but he employs a coefficient which gives much greater results than my experience warrants.

[†] "The Design and Construction of Harbours," by Thomas Stevenson, F.R.S.E. (Edinburgh, 1864; p. 22).

Heights of waves from hollow to crest.

′	″	
5	6	
5	0	
5	0	
5	6	
5	3	= mean height.

The mean depth of water below the trough was 10ft. 3in.

Heights of the highest waves from hollow to crest.

′	″	
6	0	
6	0	
8	0	
6	0	
6	0	
6	6	= mean height of highest waves.

The mean depth of water below the trough was 13ft. 8½in. So that in both cases those waves did not follow Mr. Russell's law, but *broke when the depths below their troughs were about twice their own height.*

It must not be supposed, as is generally believed, that the height of the crest above the mean level of the sea is equal to the depression of the trough below that level; for Prof. Rankine has lately shown that this is not the case. When L = length of wave, H = height from trough to crest.

$$\text{Crest } \textit{above} \text{ still water} = \frac{H}{2} + \cdot 7854 \frac{H^2}{L}.$$

$$\text{Trough } \textit{below} \text{ still water} = \frac{H}{2} - \cdot 7854 \frac{H^2}{L}.$$

These formulæ, he states, are exact only for water of considerable depth as compared with the wave's length.

Edinburgh THOMAS STEVENSON

CYCLONES IN THE INDIAN OCEAN

SEVERAL cyclones have passed Mauritius since the latter part of January. From the 24th to the 30th of that month the barometer at the Observatory fell from 29·888 to 29·708 inches, with the wind squally from E. At 10 A.M. on the 30th it was intimated to the newspapers that there were "indications of a hurricane approaching the island;" but at 2 P.M., the wind having in the interval veered to N. of E., it was announced that there was "little danger."

This storm was encountered by the schooner *Emily*, on her passage from Tamatave, from Jan. 29 to Feb. 1. At the commencement of the gale, she was in 19° 31′ S., and 53° 30′ E. The wind veered from N.E. to E., S.E., S., S.W., W., and N.W., with a "tremendous sea and torrents of rain," and the lowest reading of the barometer on board was 29·00 inches. The vessel escaped with the loss of only a few sails.

The storm then curved to the S. and E., and was experienced by the barques *Gladiateur* and *Abbotsford* on Feb. 2 and 3, in 31° to 29° S., and 54° to 55° E. With the former vessel the wind veered from E. to N.E. and N., blowing at one time with great violence. The barometer at 8 A.M. on the 2nd was at 28·80, and the wind from E.N.E. The *Abbotsford* had the wind from the same direction, and her barometer was at 28·40 at 5 A.M. on the 2nd. Both vessels had a "tremendous sea and torrents of rain," and they lost sails and bulwarks.

On Feb. 5 the barometer at Mauritius, after rising to 29·790, again began to fall, and on the 7th was at 29·606. The wind was squally from S.E., and it veered to S. by W., from which point there was a gentle breeze at 9.30 P.M. on the 7th, with fine clear weather.

At 10 A.M. on the 7th it was announced that "the weather of the last two or three days indicated the passage of another storm, which then broke between E.N.E. and E.;" and at 10 A.M. on the following day that "the storm had curved to the S. and S.E."

This storm was encountered by the barque *Elizabeth*, from Melbourne to Mauritius, on the 7th and 8th, in 20° 16′ S., and 68° E. The wind was strongest from E.N.E. to N.N.E., and the lowest barometer was 29·20. There were "torrents of rain." By standing back to the E. the *Elizabeth* avoided all danger.

On Sunday, Feb. 11, the barometer at the Observatory, after rising to 29·870, again began to fall, with the wind squally from S.E., and the weather fine. During the 12th it fell ·060 inch, and ·090 inch more during the 13th, with the wind still squally from the same quarter. At 10 A.M. on the 14th the following notice was sent to the newspapers:—"A hurricane since the 11th. It now bears about E.N.E. of us. There are some signs that it will pass to the E. and S. of the island, but there is danger." The barometer still falling, and the wind increasing to strong breezes from S. by E. to S.S.E, at 3 P.M. a telegram was sent to Port Louis (6 miles off), stating that "the centre of the hurricane was about 350 miles to the E.N.E., and approaching the island," and soon afterwards storm signals were hoisted at the railway stations. The barometer at 3 P.M. stood at 29·612, and the wind, which was then S. by E., was blowing with an estimated force of 2·5lbs. on the square foot.

At 9 A.M. on the 15th the barometer was at 29·478, with a strong gale from S.E., and it was estimated and announced that the "centre of the storm bore about N.N.E. 150 to 200 miles, and that it was still approaching the island." At 3 P.M. the wind being from E.S.E. to E. by S. in increasing gales, and the barometer at 29·382, it was telegraphed to Port Louis that "the centre was about 150 miles to N. by E., and that it would probably pass, with an increase of wind, to N.W. and W. of the island, without doing much damage."

During the night the wind increased considerably from E.S.E. to E. by S., and the barometer attained its lowest reading (29·328) at 1 A.M. on the 16th; but the mercury was oscillating, being at 2 A.M. at 29·356, and at 3 A.M. 29·330; and the time of the greatest depression of the mercury, as shown by the barograph (at the Magnetic Observatory, three miles off) was 2.40 A.M. At 9 A.M. the barometer was at 29·440, with the wind at E. to E. by N., and it was announced that "the centre bore N.N.W., and that there was no danger."

The barometer then continued to rise, until, at noon on the 18th, it was at 29·882, with a moderate breeze from N.E.

It is worthy of remark that the wind never went beyond N.N.E., but gradually backed to East.

This storm was more or less encountered at sea by the *Harpesia*, *Gryfe*, *Oleander*, *St. Germaine*, *Misser*, *S. S.*

Danube, *Staffordshire*, *William Fairbairn*, *Pendragon*, *Odalisk*, and *Paolo Revello*, some of which suffered severely.

At 5 A.M. on the 15th, the *Staffordshire*, in about 18° 30′ S. and 61° E., was thrown on her beam ends, and in great danger of foundering. The *William Fairbairn*, a fine iron vessel of 1,293 tons, lost all her masts and sails, and had her decks almost completely swept. On the 13th, in 19° 2′ S. and 64° 40′ E., she had a strong gale from S.E., which increased to a hurricane. About 7 P.M. her barometer was at 28·70, and early on the 14th the wind shifted from S.E. to N.W. The *Paolo Revello*, on the 14th, in 18° 8′ S. and 61° 54′ E., was completely gutted. The captain's papers and log-book, cabin furniture, &c., together with the chief officer and nine men, were washed overboard.

From the logs hitherto received it appears that the storm was formed between the S.E. trade-winds, and the N.W. monsoon from the 7th to the 9th. On the 10th the centre was in 13° 10′ S. and 78° 30′ E.; on the 12th in 15° 6′ S., and 71° 34′ E.; on the 14th in 17° 15′ S., and 63° 28′ E.; on the 16th in 20° 7′ S., and 55° 50′ E.; and on the 18th in 22° 15′ S. and 51° 50′ E. During the first six days it travelled on a W.S.W. course, and then curved a little towards the south. It passed about 165 miles north of Rodrigues at noon on the 14th, about 65 miles north of Mauritius early on the 16th, and N.N.W., &c., of Reunion from noon on the 16th to noon on the 17th. Its average rate of progression was nine miles an hour, and the area over which the wind blew from strong breezes to hurricane violence was about 800 miles.

The fact that in this, as in other storms, the wind at Mauritius did not veer more than twelve points, seems to be explained by the incurving of the air towards the centre.

On the evening of the 15th, or morning of the 16th, seventeen vessels put to sea from the roadsteads of Reunion, and their fate is not yet known. If they held to the N.W., with the wind from S.E., they probably got into the heart of the storm.

CHARLES MELDRUM

Mauritius, March 8

P.S.—The aurora seen here on the night of the 4th to 5th February, was also seen at sea by several vessels. Here are extracts from their logs:—

Olive Branch in 27° 47′ S. and 59° 48′ E.—"At 10 P.M. the sky became very red and fiery—southern lights."

Abbotsford in 30° 9′ S. and 56° 10′ E.—"Dull atmosphere. Aurora australis reflecting brightly in the south, giving light over all the ship. Clouds tinged with deep red."

Elizabeth in 20° 33′ S. and 78° 3′ E.—"At 10 P.M. Aurora australis unusually bright."

Gladiateur in 30° 32′ S. and 57° 28′ E.—"At 8 P.M. a red and yellow and strange looking sky. Midnight, sky the same."

Pendragon in 13° 43′ S. and 84° 13′ E.—"At midnight very suspicious-looking weather to the S., the sky being quite red."

William Fairbairn in 32° 57′ S. and 60° 2′ E.—"At 10 P.M. looking ugly, and meteorological signs of a hurricane. Midnight same, and up till 3 A.M. when it cleared off."

Caton in 31° 31′ S. and 108° 10′ E.—"Midnight, red sky, like fire to E.S.E."

Oleander in 38° 26′ S. and 31° 53′ E.—"From 7.30 to 11.30 P.M. the sky was illuminated with a very brilliant Aurora australis."

There is little doubt that the suspicious-looking weather to the S., seen by the *Pendragon* in about 14° S., was the aurora. Captain McKenzie of the *W. Fairbairn* reports that his standard compass was affected to the extent of $\frac{3}{4}$ of a point, and his other compass to the extent of two to three points. C. M.

PHYSICAL SCIENCE IN GLASGOW UNIVERSITY

THE Physical Laboratory of Glasgow University, which till quite lately was the only one in this country, dates from the year 1852. It was with difficulty that room could be found for a laboratory of any kind in the old building; but in the new building, of which this is the second year of habitation, considerable space has been set apart for Experimental Natural Philosophy.

At present six rooms belong to the department, exclusive of the Professor's private sitting-room and the store-rooms, and on the completion of the tower, which is not yet finished, additional rooms will be devoted to it. The whole suite of rooms is arranged so as to be in direct communication with those of the professors of mathematics, engineering, and astronomy.

The chief lecture-room is 42ft. long by 35ft. broad, its side windows look nearly north and south, and over the lecture table there is a glass-covered turret, or louvre, the top of which is 40ft. from the floor. The windows of the room are completely darkened with the greatest ease by means of double curtains of blue baize, an inner and an outer curtain for each window, and these can be unfurled and furled at a moment's notice; two baize screens, one below the other, are drawn across the base of the louvre. The room is ventilated, as are all the rooms in the new University building, on Mr. Phipson's plan. Pure air is drawn down a shaft in the tower by fanners, which are worked by a small steam engine. The air is passed through a dry chamber, containing hot water pipes, and is then driven mixed with any quantity of fresh cold air that may be required, into the class-room. It enters at the top of the room, and the used air is drawn off through passages below the floor.

Benches are arranged for about 150 students. They are not on a level, but rise at an angle of 25°, and beneath them there is a large convenient space, with shelves for 50 or more cells of Daniell's battery, which I shall describe immediately.

Of the other five rooms one is an additional lecture and experiment room, one is the general laboratory, one is the principal apparatus room and museum, and the remaining two are used for storing apparatus and for occasional experimenting. The laboratory is on the ground floor, and is below the lecture-room, which is on the second story. It is a room 52½ft. long by 34ft. broad. It has six windows, three looking north and three looking south, and these can be darkened like those in the lecture-room by means of drop curtains of baize. Three quarters of the floor is wood, the remainder concrete, covered with Portland cement; but in order to get perfectly steady tables, piers of masonry, built on the foundation, rise through the floor, and on them the feet of the tables rest. The flooring does not touch the piers at all, and thus, however much the floor may shake, the table remains comparatively steady. This arrangement gives far greater steadiness than a complete stone floor. Besides these piers there are two somewhat larger stone constructions, which are also unconnected with the flooring; one of these is intended for a large steady table; and on the other there is a massive stone erection (Fig. 1), on which is to hang a pendulum for a clock, or for experiments on the force of gravity. It is intended that the point of suspension of the pendulum shall be perfectly free from vibration.

Some of the tables are ordinary working tables. On others, instruments such as the electrometer and electrodynamometer are set. Below the table there are frames for supporting 500 cells of a constant Daniell's battery, which were in use in the old college, and a great part of which are now re-charged.

In one corner of the room there is a wooden enclosure, which is fitted up as a small chemical bench. The ordinary reagents and apparatus for chemical testing are thus at hand.

There are large gas holders, each holding eighteen cubic feet of gas, in the laboratory. The construction of the gas-holders is the same in principle as that used in ordinary gas works. Each is, in fact, a large copper bell, with its mouth dipped under water in a deep tank. All the stop-cocks connected with the gas-holders are kept constantly enclosed in vessels full of water, in order to prevent leakage. The holders are filled at the beginning of the session, one with oxygen and the other with hydrogen, and the lime light is thus always ready.

With this brief account of the general arrangement of the premises, I shall now describe more particularly some of the apparatus and conveniences of the lecture-room and laboratory, and some of the experimental investigations going on at present.

The lecture-table in the lecture-room is 26ft. long by 2¾ft. It is not straight, but made of three pieces, so as to be concave on the lecturer's side. A portion of the top of the table is removable; and when it is taken away a large trough suited for showing waves in water, and for other hydrodynamic experiments, is exhibited. The trough is as long as the table, and is 14in. broad and 12in. deep. It opens out at one end into a tank 26in. broad and 20in. deep. The lecture-table is, of course, furnished with the usual supply of coal, gas, and water; and besides pipes, which give hot water and steam, are led to it. These pipes come from the boilers that heat the air with which the University buildings are warmed and ventilated. Pipes are also led to the table from the oxygen and hydrogen gas holders in the lower laboratory, so that the oxyhydrogen light is always at command at a moment's notice; and it is found a great convenience to have it so. It is used very frequently, and enables us to show to a large class many experiments which we could not attempt without it—experiments, for instance, with the reflecting galvanometer and electrometer. The preparation for such experiments generally requires much time and trouble; but with the permanent gas-holders filled once for all at the beginning of the session and always ready, the oxyhydrogen light gives less trouble than an ordinary oil lamp.

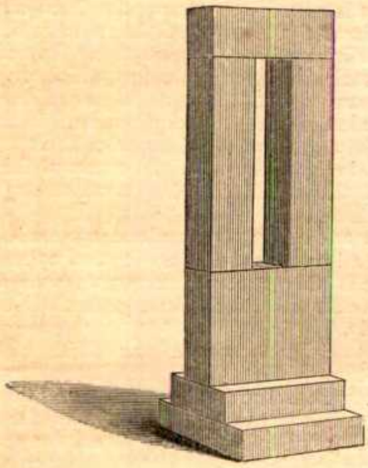

FIG. 1

A powerful battery, which I shall have to describe immediately, is always ready. Very thick electrodes, consisting of nine ply of No. 16 copper wire of high conductivity, plaited together, pass from binding screws on the lecture-table to the battery below the lecture-room seats, and thence down to the lower laboratory.

For instruments that require a very steady support there are two pillars, one at each end of the lecture-room table. These are unconnected with the flooring. They pass through it without touching the boards, and rest on the stone arches that cover a gateway beneath.

On each side of the lecture-room there is a clock. One of them is governed by electricity on Jones's principle; the other is an electric clock by Bain, and has a current from a single cell for its motive. The former is an ordinary eight-day clock, and is regulated in the following way:—The bob of the pendulum is a hollow coil of insulated wire. The plane of the coil is perpendicular to that of the motion of the pendulum. The ends of the coil are carried up the pendulum rod, and are connected with telegraph wires which proceed from the Observatory of Glasgow University. Fig. 2 gives a front view of the pendum, and shows the coil and a pair of permanent magnets pointing towards the coil. When the pendulum swings the hollow coil passes over the end of the magnet at each side. Fig. 3 shows the suspension of the pendulum, which consists of two flat springs, to which the wires coming up the pendulum rod are joined. The springs are attached to insulated brass pieces, to which are connected, by means of binding screws, the wires from the Observatory. There is a clock in the Observatory which is constantly kept right, and by means of a make-and-break arrangement connected with its pendulum, a galvanic current is sent once in each second through the wire that proceeds from the Observatory, and thus through the coil at the extremity of the pendulum of the clock that is to be controlled. The coil is thus once in each second converted into a temporary magnet, and is attracted and repelled by the permanent magnets between which it swings. It will be readily understood that with this arrangement a clock that is going slightly too slow may be accelerated a little, and a clock that is going slightly too fast may be retarded a little, at each passage of the current. To be able to set the controlled clock to agree with the Observatory clock, it is necessary to have some way of distinguishing one second from another. This is done by means of a galvanoscope or indicator, which is included in the circuit, and which beats seconds with the currents. The instrument is placed close to the clock in a place convenient for comparison of it and the seconds hand of the clock. It is arranged that there shall be no beat on the last second of each minute; and that in the last minute of each hour there shall be an interval of twenty seconds without any current. Thus the ends of the minutes and the ends of the hours are distinguished.

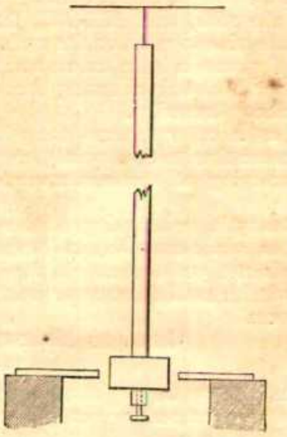

FIG. 2

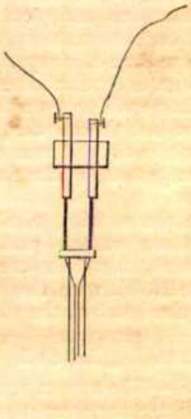

FIG. 3

Of mechanical apparatus almost daily employed for lecture illustrations may be mentioned various kinds of vibrators, weighted spiral springs, a Coulomb's torsion vibrator, a cycloidal and a common pendulum arranged for comparison, a friction brake, &c., &c. Over the lecture-table there is suspended from the top of the louvre already mentioned a pendulum, which is intended to show the rotation of the earth. The length of it is 38 ft.; and it thus executes one complete vibration in rather less than seven seconds. In showing the rotation of the earth by Foucault's method, everything depends on perfect symmetry in the suspension of the pendulum. The suspension used here is quite new, and, I think, will prove satisfactory. I hope to take an opportunity of describing it hereafter, when I can make a statement as to results obtained.

I believe that every one who has ever had to work in a laboratory with a large battery has felt how much a powerful and constant cleanly and easily managed battery is wanted. A dozen batteries have been invented, and some of them patented within the last two or three years. Most of them are only fit for such purposes as ringing an electric bell for an instant, and even at such work as that they do not last long, and not one of them is even tolerably constant, though constancy is pretended for almost all. What is wanted is a battery that will remain constant for six months or a year without more attention than that of occasionally wiping the outsides of the cells, and replenishing them with some salt or acid and with water; and which is powerful enough for an electric light or for any other class experiment. Freedom from acid fumes is also in most laboratories an essential. A great many experiments have been made here with the view of getting such a battery as I have described, and at last, as we hope, with some success.

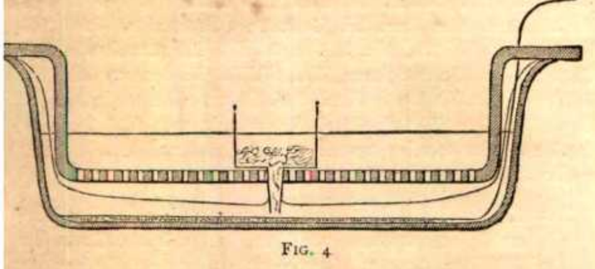

FIG. 4

Since the invention of Grove's battery, that and Bunsen's modification of it have been almost exclusively used for the electric light. These, as far as high electromotive force and smallness of resistance are concerned, are preferable to any other; but there are great objections to the use of them in the necessity for discharging them frequently, and in the emission of acid fumes, which cannot but be injurious to the person who discharges the battery, and which are very destructive to apparatus unless the battery be kept in a special chamber. The battery which we are employing in Glasgow, and which gives great promise of being really successful, is a modification of Daniell's battery.

Sir William Thomson described (Proc. R. S., Jan. 1871, quoted in NATURE of Feb. 2, 1871) a gravitation battery, in which advantage is taken of the fact that water saturated with both sulphate of zinc and sulphate of copper is denser than either saturated solution of sulphate of zinc alone or sulphate of copper alone. A horizontal copper plate being put at the bottom of the cell and a plate of zinc near the top, the cell is charged with saturated solution of sulphate of zinc, and crystals of sulphate of copper are placed in a funnel, whose delivering tube passes down to the bottom of the cell. The superior density of the solution containing sulphate of copper in addition to the sulphate of zinc, is that which keeps the sulphate of copper from surrounding the zinc plate and attacking it. This arrangement, if the sulphate of copper travelled towards the zinc solely by diffusion, would have great advantages over any in which the zinc and copper plates are placed vertically, and a porous separator is used.

It was thought that the form of cell described then would turn out admirably, and it is excellent in many ways; but it was found that a constant evolution of hydrogen takes place at the copper plate, bubbles rising perpetually from it. These cause so much stirring up of the solutions that sulphate of copper is carried rapidly up to the zinc plate, and both eat the zinc away, and it is itself wasted in depositing copper on the zinc plate. The reason of this bubbling appears to be that particles drop from the zinc on to the copper plate, and, forming small circuits there, send up hydrogen bubbles through the liquid. We have now done away with the difficulty in the following way, and we have got a cell which, so far as we have been able to test it, promises to be in all respects satisfactory. The under surface of the zinc is now covered with a sheet of parchment paper (known as manilla) the edges of which are brought up round the zinc, so that it is enclosed in a porous cell of this material. The paper, while it does not add sensibly to the resistance of the cell, acts most beneficially by hindering the particles which drop off the zinc from falling on to the copper plate; and if there are any bubbles rising from below, it prevents them from bringing sulphate of copper up to the zinc plate.

We are now employing two kinds of cells, and have forty of each kind in action. The first is very similar to that described by Sir W. Thomson in the paper already referred to. The cell is of glass (Fig. 4); and this is an advantage, as the condition of the solutions and metals which it contains may be seen at any time. It is a circular pan* with a flat bottom The diameter is 21 inches. A disc of thin sheet copper is laid on the bottom; and a thick copper wire covered with gutta percha is soldered to the copper disc and rises to the top of the cell as an electrode. In the upper part of the cell is a heavy mass of zinc, cast into the form of a circular gridiron, with three ears or projections which rest on the edges of the glass. The gridiron shape is adopted in order to permit the hydrogen, which we find constantly being liberated, to escape. The distance between the zinc and copper plates is about $2\frac{1}{2}$in. A large sheet of parchment paper covers the under side of the zinc, and the corners and edges of the paper are brought up round the vertical sides of the zinc so as to form a kind of bag round it. The parchment paper is thus a separator between the mass of liquid in the cell and that immediately surrounding the zinc. There is a circular hole in the middle of the zinc, and the tube of a glass or earthenware funnel passes through this and through a hole in the parchment paper, the edges of which are tied round the tube, down to the bottom of the cell. The cell is then filled up with saturated solution of sulphate of zinc till the level of the liquid is higher than that of the top of the zinc; and on the top of this a layer of pure water two or three inches deep is poured carefully, so as to avoid mixing. The pure water forms an atmosphere into which the sulphate of zinc formed during the action of the battery may diffuse, and thus crystallisation is avoided. To set the cell in action crystals of sulphate of copper are put into the funnel just described. The dense solution

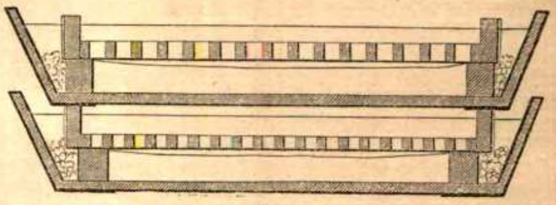

FIG. 5

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* The pans that we are using are, I believe, made by glass-blowers for milk pans. They are inexpensive, and answer excellently. To make the bottom horizontal we cover it with sand moistened with saturated solution of sulphate of zinc, and carefully level it by comparison with a little of the liquid lying on the top. It is essential to have it level if we wish to use up all the charge of sulphate of copper, as described in the text, to the best advantage.

flows down over the copper plate at the bottom, and in less than five minutes the cell is in full work.

We have at present forty cells such as I have just described. The average resistance of one cell is 0·19 of an Ohm. The electro-motive force of a Daniell's cell is 1·07 Volts.

The second form of cell is of the following construction. A shallow wooden tray, square, and with slightly slanting sides, is lined with sheet lead; and this, after being electrotyped with copper, forms both the containing vessel for the liquids and the copper plate of the cell. Copper trays were used at first, but they were soon eaten through by the solution. The lead is not attacked at all. The length of a side of the lead tray is 21in., and its depth is 3¾in. In each corner is set a small block of wood 1½in. high. The zinc plate, which is like a square grid-iron, rests at its corners on these blocks. The zinc has parchment paper tied round its lower surface and sides. The cell is filled up with saturated solution of sulphate of zinc, and crystals of sulphate of copper are dropped in when required round the edges outside the parchment paper. For connecting these cells together in series, the lead lining is carried over the wooden tray at the corners and down the outside to the under surface of the bottom of it. Here it is soldered to a small square of thick sheet tin. The cells are piled up one on the top of the other, the tin plates of the second cell resting on the zinc of the first, and so on. The tin connections—a suggestion of Mr. Varley—are most excellent. Two of these cells are shown in section Fig. 5. The resistance of each of these cells is on an average 0·19 of an Ohm. They are now used at all the telegraph stations where Sir William Thomson's siphon recorder is employed.

In using these batteries in a laboratory where they are not perpetually at work, the best way of managing them may possibly be not to charge them with sulphate of copper except when they are about to be used, and only to put in as much as will do the work required. To calculate the quantity is easy; and any small excess might be worked off through a low resistance. We have been keeping them at work almost night and day. They require no attention except to be occasionally supplied with sulphate of copper crystals, and to have the sulphate of zinc that creeps up over their edges wiped away with a cloth.

At present our battery is tested very frequently, generally once in four or five days. The electromotive force and the internal resistance of each cell is determined. We have now had the greater number of the eighty cells in action for three months, and some of them for five or six months. During all that time they have been most satisfactory, the electromotive force of them having remained perfectly constant.

We test them by means of the reflecting electrometer, or the tangent galvanometer.

J. T. BOTTOMLEY

(To be continued.)

NOTES

AFTER we went to press last week, a most cheering telegram was received in this country respecting the fate of Dr. Livingstone, as follows:—' Aden, May 1, 1872.—The *Abydos* has returned from Zanzibar. She brings news that Dr. Livingstone is safe with Stanley. The news is brought by natives." It may fairly be hoped that still more authentic intelligence will shortly be received respecting the fate of the great traveller, with respect to whom such anxiety has been manifested in this country. Another despatch speaks also of the destruction of a large portion of the town of Zanzibar by a terrible hurricane on April 15.

THE eruption of Vesuvius, respecting which we gave such details as were accessible last week, appears to be over. Whether any scientific results have been obtained by any observers besides Palmieri it is too early yet to know. We hope it may be so, and shall return to the subject as soon as the authentic accounts have been collated.

THE Annual Visitation of the Board of Visitors to Greenwich Observatory will take place on Saturday, June 1st.

THE President, Vice-President, and Council of the Pharmaceutical Society of Great Britain will hold a *conversazione* at the South Kensington Museum on Wednesday evening next, May 15.

AT a meeting of Convocation at Oxford last week, it was carried that in all the schools, except that of theology, examiners might be appointed who were not members of the University. The liberal change which had already been granted for the Natural Science School will do much to widen the general course of reading at that University, and to prevent the studies pursued there partaking too much of any narrow or special character.

MR. CHARLES TOMLINSON, F.R.S., lectures this evening at the London Institution, Finsbury Circus, on Solution and Supersaturation.

AT the Annual Meeting of the Literary and Philosophical Society of Manchester, held on the 30th ult., the following officers were elected for the ensuing year:—President, Mr. E. W. Binney, F.R.S., F.G.S.; Vice-Presidents, Dr. Jas. P. Joule, F.R.S., Dr. E. Schunck, Dr. Robert Angus Smith, F.R.S., and the Rev. W. Gaskell; Secretaries, Prof. H. E. Roscoe, F.R.S., Mr. J. Baxendell. As members of the Council, Mr. Peter Spence, Mr. W. L. Dickenson, Mr. H. Wilde, Mr. R. D. Derbyshire, Prof. Osborne Reynolds, Mr. W. Boyd Dawkins, F.R.S., Prof. Balfour Stewart, F.R.S. Few local societies can boast such a distinguished list of names as the above.

TWO Scholarships of the annual value of 30*l.* and 20*l.* respectively, tenable for two years, have been founded by the Governors of the Middlesex Hospital, for the encouragement of the study of medicine and surgery, in memory of the late Francis Broderip, a munificent benefactor to the hospital. These scholarships will be open to competition, at the end of each winter session, amongst the general students of the hospital who shall have completed their third year of study at the Medical College. The successful candidates will be required to attend and work at the hospital for a fourth year, during which period they will be eligible for the various resident appointments.

TWO Scholarships, of the annual value of 25*l.* and 20*l.* respectively, will be offered for competition at Middlesex Hospital at the commencement of the Winter Session 1872-73. Each scholarship is tenable for two years, provided the scholar conducts himself satisfactorily. These scholarships are open to all gentlemen who commence their medical studies in October 1872. Successful candidates will be required to become general students of the College. The examination will take place on September 27 and following days, and the result will be declared on October 5. The following are the subjects for examination:—Latin, Greek, French or German, Mathematics, Natural Philosophy, Chemistry, Botany, Zoology. Candidates will be examined in any three of the above subjects they may select; but only one Modern Language and two out of the last three subjects are permitted. An equal number of marks will be given to each subject, and candidates will be expected to attain a certain standard of proficiency in the subjects they select. Candidates must send in their names in writing, addressed to the Dean, at the Middlesex Hospital, stating the subjects which they elect for their examination, on or before September 24.

We referred recently to Tilghman's ingenious process for cutting hard substances by means of a jet of sand. The Committee on Science and the Arts, constituted by the Franklin Institute of the State of Pennsylvania, for the promotion of the mechanic arts, to whom the process was referred for examination, report that they have seen the operation, and that the invention seems capable of extensive use in the arts. Some of the products of the invention appear to present new and valuable features. Glass ornamented by this process can only be compared with that etched by powerful acids, yet the entire absence of all undercutting, no matter how deeply the glass is cut, renders it superior. The great merit of the invention consists in its extended utility. By means of this sand-blast effects have been produced which would be hard to imitate by any other known mechanical process, and with an ease and precision truly remarkable. They consider the invention original and of the highest utility, and deem it worthy of any mark of approbation it may be thought fit to bestow upon its originator. They therefore recommend the bestowal on the inventor of the Elliott Cresson medal of the Franklin Institute, in accordance with the rules governing such award.

Prizes to the amount of 25*l.* are offered by the proprietors of the *Gardeners' Magazine* for dinner-table decorations in fruit and flowers, or flowers only, arranged for effect in artificial light. The competition is to take place in connection with the Royal Horticultural Society's Exhibition in Birmingham, in June.

Prof. Hull, head of the Geological Survey of Ireland, has in the press a work designed to render practical service to builders and architects by information about the varieties of stone and their respective advantages for building purposes. "In arranging the matter treated in this work," says Prof. Hull, "I have not followed any very definite order, but rather that which the subject seemed to indicate." Regretting the incorrect nomenclature which is employed in the architectural classification of natural building materials, and yet feeling that the adoption of the mineral basis of rocks as the principle of arrangement would render his book less useful for reference by architects, the author has followed the plan drawn out on his fourth page. "Commencing with the noblest of all rocks, granite, I have been naturally led onwards to the allied rocks, such as syenite, porphyry, and from those to other plutonic or volcanic rocks. After these the metamorphic serpentines and marbles form a transition series through the simpler and rarer ornamental stones into those adapted for building, and of aqueous formation." The work will be published by Messrs. Macmillan and Co.

Our readers will be glad to learn that Dr. Bastian's "Beginnings of Life" will be published shortly after Whitsuntide. This book will be a complete exposition of those views of its author upon vital phenomena, and the conditions of their appearances, which have already excited so much interest and discussion.

The Rugby Council for Promoting the Education of Women has just published a very useful Calendar of women holding University certificates and engaged in teaching. It contains also all the needful information for those intending to try for the examinations for women and girls conducted by the Universities of Oxford, Cambridge, London, Dublin, Edinburgh, Durham, and the Queen's University for Ireland, and will thus be of essential aid, not only to these, but also to those who are seeking the services of women who have shown themselves capable of obtaining the highest educational honours as yet open to them. It may be obtained of Mrs. F. E. Kitchener, Rugby.

The Report of the Rugby School Natural History Society for 1871 lies before us. The preface refers with modest pride to several subjects of congratulation by the Society:—nearly all the published papers, some of them of considerable merit, are written by actual members of the school; a meteorological and astronomical section was begun in 1871, in consequence of the establishment of the Temple Observatory, and has been heartily carried out by those members of the school who were competent to serve; a large entomological section is springing up; the members of the Society have doubled during the year; and during the present spring each section has begun fortnightly meetings, from which all but real workers are to be excluded. Every year shows more and more the important place which the Natural History Societies of our public schools are taking in the scientific education of the country.

The Report of the Marlborough College Natural History Society, now eight years old, for the half-year ending Christmas, 1871, contains no paper bearing directly on the natural history of the district. That good local work is being done by members of the Society is, however, evidenced by the excellent list of entomological notices appended. We cannot unite with the sentence in the Preface that "it is an almost utter impossibility to get any original matter from beginners, and it would be unreasonable to expect any." It ought to be the special aim of school Natural History Societies to cultivate original work rather than mere collecting; the opportunities for it, even for beginners, are endless; and there is no over-estimating the value to the observer of the least morsel of such original work. The papers printed in this Report are excellent in their way; Mr. Babington's on "The Malay Archipelago and its Inhabitants," is a model of what such a paper should be. The Report is illustrated by an admirable plate of *Clavaria fusiformis*, drawn by Mr. F. C. Hulme. We would venture to suggest to the Committee the propriety of excluding in future from a published report personal matters, which, though of great importance to the Society itself, do not interest the outside public.

The Royal Cornwall Polytechnic Society has published its thirty-ninth Annual Report for 1871. Among the more important lectures and papers contained in it may be mentioned "On the Comparative Health and Longevity of Cornish Miners," containing a large number of very valuable statistics, by Mr. R. Blee; and the "Meteorology of West Cornwall and the Scilly Islands for 1871," by Mr. W. P. Dymond, besides descriptions of a number of mechanical inventions and applications especially valuable to those engaged in mining operations.

The first excursion of the Geologists' Association to Watford took place on April 13, under the guidance of Messrs. W. Whitaker and J. Hopkinson. The first section visited was a fine one of the Glacial Drift now exposed by the side of the railway near Watford to a depth of 30 feet. The drift is the "Middle Glacial," and consists of sand and sub-angular gravels with pebbles of various rocks, some of which are evidently from very distant localities. After passing through Cassiobury Park, the party proceeded to Bushey Heath, at which place the "Basement Bed" of the London Clay is exposed, with the upper portion of the Woolwich and Reading series. This section was described by Mr. Whitaker, as was also one at Bushey Heath Kiln, where the Woolwich and Reading series is again seen The formation is, however, represented by beds of sand at the former place, and by pebble beds at the latter. The next excursion was to Hampstead on April 27, directed by Messrs. J. R. Pattison and C. Evans. The party assembled at the Swiss Cottage station, and crossed the fields to Hampstead; but before the village was reached the structure of the hill was described, and the indications of the junction of the London Clay and the Bagshot Sands were observed. The summit of the hill consists of an outlier of the Bagshot Sands overlying the uppermost sandy beds of the London Clay, containing *Pectunculus decussatus* and *Voluta nodosa*. The Conduit spring was visited, and the line of junction of the two formations, indicated by springs and pools, was followed as far as the Vale of Health pond. The highest point of the Heath was then soon gained, and here the physiography of the district was pointed out. After passing through the West Heath valley, in which a thin bed of water-bearing sand above the clay has produced a swamp, the party visited the residence of Mr. C. Evans, and inspected that gentleman's fine collection of Tertiary fossils.

THE new School of Science and the new Museum and Art Gallery at the rear of the Hartley Hall, Southampton, are fast approaching completion. There are two class rooms of about 20ft. by 16ft. 6in and 16ft. high, and a large drawing-class-room, 32ft. by 16ft. 6in. and 16ft. high. In the Art Gallery and Museum are two large rooms, 42ft. by 20ft., and about 18ft. high, lighted from the roof and connected with the Science School and the Art School.

A FREE Museum has lately been opened at Nottingham in Wheeler-Gate. The greater portion of the objects of interest which are in the museum, now the property of the town, originally belonged to the Nottingham Naturalists' Society, and a considerable number of valuable objects were added from time to time until the museum assumed its present dimensions.

ACCORDING to the Stockholm *Aftonbladet* an important discovery has been made in Sweden. An extensive coal-bed of remarkable depth and excellent quality has been struck near Raus, in Schonen. An enterprising company formed some time ago was encouraged by promising geological indications to institute borings, but the first results were hardly satisfactory. At a depth of 566ft. eleven strata of coal had indeed been pierced, but none of these exceeded in depth 1¼ft. Five feet farther down, however, a bed was struck with a thickness of 8¾ft. The borings have been continued, and are believed to prove satisfactorily the existence of an extensive coal-bed.

A MOST violent cyclone occurred at Madras on May 1. Many vessels were driven on shore, and completely wrecked. The pier has been again breached, and great damage done to the city and suburbs. On Friday, the 3rd, the storm was slightly abating.

AT a recent sitting of the French Academy a letter was read from the French Consular agent at Mostar on the earthquakes felt throughout the Herzegovina during the months of February and March last. The first was felt on the 6th of February, two days after the great aurora borealis. Other oscillations followed on the 7th and 8th, apparently in a N.W. and S.E. direction On the 13th a longer shock was experienced, followed by a loud rumbling sound like distant cannon. On the 25th and 27th stronger shocks, accompanied by noise, were felt, making about forty since the 6th. On March 2 and 3 the manifestations increased in intensity; but neither Ragusa nor Serajevo, so subject to earthquakes, appear to have been at all affected all the time.

SOME time in the summer of 1871 it was stated that Mr. Octave Pavé, a young Louisiana Frenchman, had started toward the North Pole by way of Siberia and Wrangell's Land, and that, in the absence of news from him, the assistance of the Siberian Government had been invoked, in consequence of grave fears for his safety. It now appears that he has not yet started on his mission, but is to sail from San Francisco in May for Kamschatka, where he will take in supplies, and proceed to Cape Yakan, on the north-east coast of Siberia. Here the vessel is to be abandoned, and a further exploration attempted on an India-rubber raft, composed of four rubber cylinders fastened together on the decks by wooden slats, to which the masts and rigging are attached. It is intended to head, after leaving Cape Yakan, for Wrangell's Land, a large island discovered by Captain Long in 1867. This being reached, the island is to be crossed on sledges; and if an open sea occur beyond, he is to take the raft again, and endeavour to sail to Greenland or Spitzbergen. The entire enterprise is conducted at the expense of the traveller; and however hazardous or chimerical the plan may be, we cannot but wish him success in his movements.

HISTORY OF THE NAMES CAMBRIAN AND SILURIAN IN GEOLOGY[*]

(*Continued from page* 17)

WHAT then was the value and the significance of the Silurian sections of Murchison, when examined in the light of the results of the Government surveyors? The Llandeilo rocks, having throughout the characteristic *Orthis* so much insisted upon by Murchison, were shown to be the base of a great conformable series, and to the eastward, in Shropshire, to rest on the upturned edges of the Longmynd rocks; while westward, near Bala, they overlie unconformably the Lingula-flags, and in the island of Anglesea repose directly upon the ancient crystalline schists. According to the author of the "Silurian System," there existed beneath the base of the Llandeilo formation a great conformable series of slaty rocks into which this formation passed, and from which it could not be distinguished either zoologically, stratigraphically, or lithologically. The sequence, determined from what were considered typical sections in the valley of the Towey in Caermarthenshire, as given by Murchison, for several years both before and after the publication of his work, was as follows:—1. Cambrian; 2. Llandeilo flags; 3. Caradoc sandstone; 4. Wenlock and Ludlow beds; 5. Old Red sandstone; the order being from north-west to south-east. What then were these fossiliferous Cambrian beds underlying the Llandeilo and indistinguishable from it? Sedgwick, with the aid of the Government surveyors, has answered the question in a manner which is well illustrated in his ideal section across the valley of the Towey. The whole of the Bala or Caradoc group rises in undulations to the north-west, while the Llandeilo flags at its base appear on an anticlinal in the valley, and are succeeded to the south-east by a portion of the Bala. The great mass of this group on the south-east side of the anticlinal is however concealed by the overlapping May Hill sandstone—the base of the unconformable upper series which includes the Wenlock and Ludlow beds. (Philos. Mag. IV. viii. 488) The section to the south-east, commencing from the Llandeilo flags on the anticlinal, was made by Murchison the Silurian system, while the great mass of strata on the north-west side of the Llandeilo (which is the complete representative of the Caradoc or Bala beds, partially concealed on the south-west side) was supposed by him to lie beneath the Llandeilo, and was called Cambrian (the Upper Cambrian of Sedgwick). These rocks, with the Llandeilo at their base, were in fact identical with the Bala group studied by the latter in North Wales, and are now clearly traced through all the intermediate distance. This is admitted by Murchison, who says:—"The first rectification of this erroneous view was made in 1842 by Prof. Ramsay, who observed that instead of being succeeded by lower rocks to the north and west, the Llandeilo flags folded over in those directions, and passed under superior strata, charged with fossils which Mr. Salter recognised as well-known types of the Caradoc or Bala beds." ("Siluria," 4th ed., p. 57, foot-note.)

The true order of succession in South Wales was in fact:—1. Llandeilo; 2. Cambrian (=Caradoc or Bala); 3. Wenlock and Ludlow; 4. Old Red sandstone; the Caradoc or Bala beds being repeated on the two sides of the anticlinal, but in great part concealed on the south east side by the overlapping May Hill or Upper Llandovery rocks. These latter, as has been shown, form the true base of the upper series which, in the Silurian sections, was represented by the Wenlock and Ludlow. Murchison had, by a strange oversight, completely inverted the order of his lower series, and turned the inferior members upside down. In fact, the Llandeilo flags, instead of being, as he had maintained, superior to the Cambrian (Caradoc or Bala) beds, were really inferior to them, and were only made Silurian by a great mistake. The Caradoc, under different names, was thus made to do duty at two horizons in the Silurian system, both below and above the Llandeilo flags. Nor was this all; for by another error, as we have seen, the Caradoc in the latter position was made to include the Pentamerus beds of the unconformably overlying series. Thus it clearly appears that, with the exception of the relations of the Wenlock and Ludlow beds to each other and to the overlying Old Red sandstone, which were correctly determined, the Silurian system of Murchison was altogether incorrect, and was moreover based upon a series of stratigraphical mistakes, which are scarcely paralleled in the history of geological investigation.

It was thus that the Lower Silurian was imposed on the scientific world; and we may as well ask with Sedgwick, whether geologists "would have accepted the Lower Silurian classification and nomenclature, had they known that the physical or sectional evidence upon which it was based had been from the first positively misunderstood." Feeling that his own sections were, as has since been fully established, free from error, Sedgwick naturally thought his name of Upper Cambrian should prevail for the great Bala group. Hence the long and embittered discussion that followed, in which Murchison in many respects occupied a position of vantage as against the Cambridge professor, and finally saw his name of Lower Silurian supplant almost entirely that of Upper Cambrian given by Sedgwick, who had first rightly defined and interpreted the geological relations of the group.

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[*] Reprinted from advance sheets of the *Canadian Naturalist.*

In a paper read before the Geological Society in June, 1843 (Proc. Geol. Soc. iv. 212-223), when the perplexity in which the relations of the Upper Cambrian and Silurian rocks were involved had not been cleared up by the discovery of Murchison's errors in stratigraphy, Sedgwick proposed a compromise, according to which the strata from the Bala limestone to the base of the Wenlock were to take the name of Cambro-Silurian; while that of Silurian should be reserved for the Wenlock and Ludlow beds, and for those below the Bala the name of Cambrian should be retained. The Festiniog group (including what were subsequently named the Lingula flags and the Tremadoc slates) would thus be Upper instead of Middle Cambrian, the original Upper Cambrian being henceforth Cambro-Silurian; it being understood that, wherever the dividing line might be drawn, all the groups above it should be called Cambro-Silurian, and all those below it Cambrian. This compromise was rejected by Murchison, who in the map accompanying the first edition of his "Siluria," in 1854, extended the Lower Silurian colour so as to include all but the lowest division of the Cambrian, viz., the Bangor group. When, however, the relations of Upper Cambrian and Silurian were made known by the discoveries of Sedgwick and the Government surveyors, this compromise was seen to be uncalled for, and was withdrawn in 1854 by Sedgwick, who re-claimed the name of Upper Cambrian for his Bala group.

In June 1843, Sedgwick proposed that the whole of the fossiliferous rocks below the horizon of the Wenlock should be designated Protozoic, and on Nov. 29, 1843, presented to the Geological Society an elaborate paper on the Older Palæozoic (Protozoic) Rocks of North Wales, with a coloured geological map. This paper, which embodied the results of the researches of Sedgwick and Salter, was not, however, published at length, but an abstract of it was prepared by Mr. Warburton, then president of the society, with a reduced copy of the map (Proc. Geol. Soc. iv. 212 and 251-268; also Geol. Jour. i. 5-22). In this map of Sedgwick's three divisions were established, viz, the hypozoic crystalline schists of Caernarvonshire, the Protozoic, and the Silurian On the legend of the reduced map, as published by the Geological Society, these latter names were altered so as to read "Lower Silurian (Protozoic)" and "Upper Silurian." These changes, in conformity with the nomenclature of Murchison, were, it is unnecessary to say, made without the knowledge of Sedgwick, who did not inspect the reduced and altered map until it was appealed to as an evidence that he had abandoned his former ground, and had recognised the equivalency of the whole of his Cambrian with the Lower Silurian of Murchison The reader will sympathise with the indignation with which Sedgwick declares that his map was "most unwarrantably tampered with," and will, moreover, learn with surprise that an inspection of the proof sheets of Warburton's abstract of Sedgwick's paper was refused him, notwithstanding his repeated solicitations. The story of all this, and finally of the refusal to print in the pages of the *Geological Journal* the reclamations of the venerable and aggrieved author, make altogether a painful chapter, which will be found in the *Philos. Magazine* for 1854 (IV. viii. pp. 301-317, 359-370, and 483-506), and more fully in the "Synopsis of British Palæozoic Rocks," which forms the introduction to McCoy's "British Palæozoic Fossils"

In connection with this history it may be mentioned that in March 1845 Sedgwick presented to the Geological Society a paper on the Comparative Classification of the Fossiliferous Rocks of North Wales and those of Cumberland, Westmoreland, and Lancashire, which appears also in abstract in the same volume of the *Geological Journal* that contains the abstract of the essay and the map just referred to (i. 442). That this abstract also is made by another than the author is evident from such an expression as "the author's opinion seems to be grounded on the following facts," &c. (p. 448), and from the manner in which the terms Lower and Upper Silurian are applied to certain fossiliferous rocks in Cumberland. Yet the words of this abstract are quoted with emphasis in "Siluria" (1st ed, 147), as if they were Sedgwick's own language, recognising Murchison's Silurian nomenclature.

II.—*Middle and Lower Cambrian*

Investigations in continental Europe were, meanwhile, preparing the way for a new chapter in the history of the lower palæozoic rocks. A series of sedimentary beds in Sweden and Norway had long been known to abound in singular petrifications, some of which had been examined by Linnæus, who gave to them the name of *Entomolithi*. They were also studied and described by Wahlenberg and by Brongniart, the latter of whom, from two varieties of the *Entomolithus paradoxus*, Linn, established in 1822 two genera, *Paradoxides* and *Agnostus*. In 1826 appeared a memoir by Dalman on the Palæadæ, or so-called Trilobites; which was followed, in 1828, by his classic work on the same subject ("Ueber die Palæaden oder so-genannten Trilobiten," 4to, with six plates, Leipsic). In these works were described and figured, among many others, two genera—*Olenus*, which included *Paradoxides* Brongn. and *Battus*, including *Agnostus* of the same author. Meanwhile, Hisinger was carefully studying the strata in which these Trilobites were found in Gothland, and in the same year (1828) published in his *Anteckningar*, or Notes on the Physical and Geognostical Structure of Norway and Sweden, a coloured geological map and section of these rocks as they occur in the county of Skaraborg, where three small circumscribed areas of nearly horizontal fossiliferous strata are shown to rest upon a floor of old crystalline rocks, in some parts granitic and in other gneissic in character. The section and map, as given by Hisinger, show the succession in the principal area to be as follows, in ascending order: (1) granite or gneiss, (2) sandstone, (3) alum-slates, (5) orthoceratite-limestones, (4) clay, slates. By a curious oversight the colours on the legend are wrongly arranged and wrongly numbered, as above; for in the map and section it is made clear that the succession is that just given, and that the clay-slates (4) instead of being below, are above the orthoceratite-limestones (5).

In 1837 Hisinger published his great work on the organic remains of Sweden, entitled *Lethæa Suecica* (4to, with forty-two plates) In this he gives a tabular view, in descending order, of the rock-formations, and of the various genera and species described. The rocks of the areas just noticed appear in his fourth or lowest division, under the head of *Formationes Transitionis*, and are divided as follows:—

a. Strata calcarea recentiora Gottlandiæ.
b. Strata schisti argillacei.
c. Strata schisti aluminaris.
d. Strata calcarea antiquiora.
e. Strata saxi arenacei.

The succession thus given was however erroneous, and probably, like the mistake in the legend of the same author's map just mentioned, the result of inadvertence, the true position of the alum-slates (*c*) being between the older limestone (*d*) and the basal sandstone (*e*). This is shown both by Hisinger's map of 1828, and by the testimony of subsequent observers. In Murchison's work on the Geology of Russia in Europe, published in 1845, there is given (p. 15 *et seq*) an account of his visit to this region in company with Prof Loven, of Christiania; which, with figures of the sections, is reproduced in the different editions of "Siluria." The hill of Kinnekulle, on Lake Wener, is one of the three areas of transition rocks delineated on the mad of Hisinger above referred to. Resting upon a flat region of nearly vertical gneissic strata, we have according to Murchison, (1) a fucoidal sandstone, (2) alum-slates, (3) red orthoceratite limestone, (4) black graptolitic slates, the whole series being little over 1,000 ft. in thickness, and capped by erupted greenstone. Above these higher slates there are found in some parts of Gothland, other limestones with orthoceratites, trilobites, and corals, the newer limestone strata (*a*) of Hisinger; the whole overlain by thin sandstone beds. These higher limestones and sandstones contain the fauna of the Wenlock and Ludlow of England; while the lower limestones and graptolitic slates afford *Calymene Blumenbachii*, *Orthis calligramma*, and many other species common to the Bala group of North Wales. The alum-slates below these however contained, according to Hisinger, none of the species then known in British rocks, but in their stead five species of *Olenus* and two of *Battus* (*Agnostus*).

In 1854 Angelin published his *Palæontolologica Scandinavica*, part 1, *Crustacea formationis transitionis* (4to, forty-one plates), in which he divided the series of transition rocks above described by Hisinger into eight parts designated by Roman numerals, counting from the base. Of these I. was named *Regio Fucoidarum*, no organic remains other than fucoids being known therein; while the remaining seven were named from their characteristic genera of trilobites, which were as follows, in ascending order; certain letters being also used to designate the parts:—II. (A) Olenus: III. (B) Conocoryphe; IV. (BC) Ceratopyge; V. (C) Asaphus; VI. (D) Trinucleus; VII. (DE) Harpes; VIII. (E) Cryptonymus. In the *Regio Olenorum* (II.) was found also the allied genus *Paradoxides*. With regard to the characteristic genus of Regio III., the name of *Conocoryphe* was proposed for it by Corda in 1847, as synonymous with Zenker's name of *Conocephalus* (*Conocephalites*), already appropriated to a genus of insects.

Meanwhile the similar crustaceans which abound in the transition rocks of Bohemia had been studied and described by Hawle, Corda, and Beyrich, when Barrande began his admirable investigations of this ancient fauna and of its stratigraphical relations. He soon found that beneath the horizon characterised by fossils of the Bala group (Llandeilo and Caradoc) there existed in Bohemia a series of strata distinguished by a remarkable fauna, entirely distinct from anything known in Great Britain, but closely allied to that of the alum-slates of Scandinavia, corresponding to Regiones II. and III. of Angelin. To this he gave the name of the first or primordial fauna, and to the rocks yielding it that of the Primordial Zone. Resting upon the old gneisses of Bohemia appears a series of crystalline schists designated by Barrande as *Etage A*, overlain by a series of sandstones and conglomerates, *Etage B*, upon which repose the fossiliferous argillites of the Primordial Zone or *Etage C*. The rocks of the Etages A and B were by Barrande regarded as azoic, but in 1861, Fritsch of Prague, after a careful search, discovered in certain thin-bedded sandstones of B the traces of filled-up vertical double tubes; which, according to Salter (Mem. Geol. Sur. iii. 243), are probably the marks of annelides, and are identical with those found in the rocks of the Bangor or Longmynd group in Great Britain; which will be shown to belong to the Primordial Zone. It is, therefore, probable that the Etage B, which apparently corresponds to the Regio Fucoidarum or basal sandstone of Scandinavia, should itself be included in the Primordial Zone. It may here be noticed that it is in the crystalline schists of A that Gumbel has found *Eozoon bavaricum*. To the Etage C in Bohemia, Barrande assigns a thickness of about 1,200 feet, and to this his first fauna is confined, while in the succeeding divisions he distinguished a second and a third. The second fauna, which characterises Etage D, corresponds to that of the Bala group; while the third fauna, belonging to the Etages E, F, G, and H, is that of the May Hill, Wenlock, and Ludlow formations of Great Britain.

This classification of the ancient Bohemian faunas was first set forth by Barrande in 1846, in his *Notice Préliminaire*, in which he declared that the first fauna was below the base of the Llandeilo of Murchison, unknown in Great Britain, and, moreover, "new and independent in relation to the two Silurian faunas (his second and third) already established in England." This opinion he reiterated in 1859. These three divisions form in Bohemia an apparently continuous series, and being connected with each other by some common species, Barrande was led to look upon the whole as forming a single stratigraphical system; and finally to assert that these three independent faunas "form by their union an indivisible triad which is the Silurian system." (Bul. Soc. Geol. de Fr. II. xvi. 529-545.) Already, in 1852, in his magnificent work on the Silurian System of Bohemia, Barrande had given to the strata characterised by his first fauna the name of Primordial Silurian. It is difficult to assign any just reason for thus annexing to the Silurian—already augmented by the whole Upper Cambrian or Bala group of Sedgwick, (Llandeilo and Caradoc)—a great series of fossiliferous rocks lying below the base of the Llandeilo, and unsuspected by the author of the Silurian system; who persistently claimed the Llandeilo beds, with their characteristic second fauna, as marking the dawn of organic life.

Up to this time the primordial palæozoic fauna of Bohemia and of Scandinavia was, as we have said, unknown in Great Britain. The few organic remains mentioned by Sedgwick in 1835 as occurring in the region occupied by his Lower and Middle Cambrian, on Snowdon, were found to belong to Bala beds, which there rest upon the older rocks; nor was it until 1845 that Mr. Davis found in the Middle Cambrian remains of *Lingula*. In 1846, Sedgwick, in company with Mr. Davis, re-examined these rocks, and in December of the same year described the Lingula beds as overlaid by the Tremadoc slates and occupying a well-defined horizon in Caernarvon and Merionethshire, beneath the great mass of the Upper Cambrian rocks. (Geol. Jour. ii. 75, iii. 139.) Sedgwick, at the same time, noticed about this horizon certain Graptolites and an *Asaphus*, which were supposed to belong to the Tremadoc slates, but have since been declared by Salter to pertain to the Arenig or Lower Llandeilo beds, the base of the Upper Cambrian. (Mem. Geol. Sur. iii. 257, and Decade II.)

This discovery of the Lingula flags, as they were then named, and the fixing by Sedgwick of their geological horizon, was at once followed by a careful examination of them by the Government surveyors; and in 1847, Selwyn detected in the Lingula flags, near Dolgelly, in Merionethshire, the remains of two crustacean forms, the one a phyllopod, which has received the name of *Hymenocaris vermicauda* Salter, and the other a trilobite, which was described by Salter in 1849 as *Olenus micrurus*. (Geol. Survey, Decade II.) A species of *Paradoxides*, apparently identical with *P. Forchammeri* of Sweden, was also about this time recognised among specimens supposed to be from the same horizon. It has since been described as *P. Hicksii*, and found to belong to the basal beds of the Lingula flags—the Menevian group.

Upon the flanks of the Malvern Hills there are found resting upon the ancient crystalline rocks of the region, and overlain by the Pentamerus beds of the May Hill sandstone (originally called Caradoc by Murchison) a series of fossiliferous beds. These consist in their lowest part of about 600 feet of greenish sandstone, which have since yielded an *Obolella* and *Serpulites*, and are overlain by 500 feet of black schists. In these, in 1842, Prof. John Phillips found the remains of trilobites, which he subsequently described, in 1848, as three species of *Olenus* (Mem. Geol. Survey ii. part 1, 55). These black shales, which had not at that time furnished any organic remains, were by Murchison in his "Silurian System" (p. 416) in 1839 compared to the supposed passage beds in Caermarthenshire between the Llandeilo and the Cambrian (Bala) rocks; which, as we have seen, were newer and not older strata than the Llandeilo flags. From their lithological characters, and their relations to the Pentamerus beds, these lower fossiliferous strata of Malvern were subsequently referred by the Government geologists to the horizon of the Caradoc proper or Bala group; nor was it until 1851 that their true geological age and significance were made known. In that year, Barrande, fresh from the study of the older rocks of the Continent, came to England for the purpose of comparing the British fossils with those of the Primordial Zone which he had established in Bohemia and Scandinavia, and which he at once recognised in the Lingula flags of Sedgwick and in the black schists at Malvern; both of which were characterised by the presence of the genus *Olenus*, and were referred to the horizon of his Etage C. This important conclusion was announced by Salter to the British Association at Belfast in 1852 (Rep. Brit. Assoc., abstracts, p. 56, and Bull. Soc. Geol. de Fr. II. xvi. 537). Since that time the progress of investigation in the Middle and Lower Cambrian rocks of Wales has shown a fauna the importance and richness of which has increased from year to year.

The palæontological studies of Salter, while they confirmed the primordial character of the whole of the great mass of strata which make up the Middle Cambrian or Festiniog group of Sedgwick (consisting of the Lingula flags and the Tremadoc slates), led him to propose several sub-divisions. Thus he distinguished on palæontological grounds between the upper and lower Tremadoc slates, and for like reasons divided the Lingula flags into a lower and an upper portion. For the discussion of these distinctions the reader is referred to the memoirs of the Geol. Survey (iii. 240-257). Subsequent researches led to the division of the original Lingula flags into three parts, an upper and a middle, to which the names of Dolgelly and Maentwrog were given by Mr. Belt, and a third consisting of the basal beds, which were separated in 1865 by Salter and Hicks, with the designation of Menevian, derived from the ancient Roman name of St. David's in Pembrokeshire. It was here that in 1862, Salter found *Paradoxides* with *Agnostus* and *Lingula* in fine black shales at the base of the Lingula flags, resting conformably on the green and purple grits of the Lower Cambrian or Harlech beds. The locality was afterwards carefully studied by Hicks, and it was soon made apparent that the genus *Paradoxides*, both here and in North Wales, was confined to a horizon below the great mass of the Lingula flags, which, on the contrary, are characterised by numerous species of *Olenus*. These lower or Menevian beds are hence regarded by Salter as equivalent to the lowest portion of the Etage C of Barrande.

Beneath these Menevian beds there lies, in apparent conformity, the great Lower Cambrian series, frequently called the bottom or basement rocks by the Government surveyors; represented in North Wales by the Harlech grits, and in South Wales, near St. David's, by a similar series of green and purple sandstones, considered by Murchison and by others as the equivalent of the Harlech rocks. They were still supposed to be unfossiliferous until, in June 1867, Salter and Hicks announced the discovery in the red beds of this lower series, at St. David's, of a *Lingulella*, very like *L. ferruginea* of the Menevian (Geol. Jour. xxiii. 339; Siluria, 4th ed. 550). This led to a further examination of these Lower Cambrian beds, which has resulted in the discovery in them of a fauna distinctly primordial in type, and linked by the presence of several identical fossils to the Menevian; but in many respects distinct, and marking a lower fossiliferous horizon than anything known in Bohemia or in Scandinavia.

The first announcement of these important results was made to the British Association at Norwich in 1868. Further details were, however, laid before the Geological Society in May 1871 by Messrs. Harkness and Hicks, whose paper on the Ancient Rocks of St. David's Promontory appears in the Geological Journal for November 1871 (xxviii. 384). The Cambrian sediments here rest upon an older series of crystalline stratified rocks, described by the geological surveyors as syenite and greenstone, and having a north-west strike. Lying unconformably upon these, and with a north-east strike, we have the following series, in ascending order:—1, quartzose conglomerate, 60ft.; 2, greenish flaggy sandstone, 460ft.; 3, red flags or slaty beds, 50ft., containing *Lingulella ferruginea*, besides a larger species, *Discina*, and *Leperditia cambrensis*; 4, purple and greenish sandstones, 1,000ft.; 5, yellowish gray sandstones, flags and shales, 150ft., with *Plutonia*, *Conocoryphe*, *Microdiscus*, *Agnostus*, *Theca*, and *Protospongia*; 6, gray, purple and red flaggy sandstones, with most of the above genera, 1,500ft.; 7, gray flaggy beds, 150ft., with *Paradoxides*; 8, true Menevian beds, richly fossiliferous, 500ft. The latter are the probable equivalent of the base of Barrande's Etage C, and at St. David's are conformably overlain by the Lingula flags, beneath which we have, including the Menevian, a conformable series of 3,370ft. of uncrystalline sediments, fossiliferous nearly to the base, and holding a well-marked fauna distinct from anything hitherto known in Great Britain or elsewhere.

The Menevian beds are connected with the underlying strata by the presence of *Lingulella ferruginea*, *Discina pileolus*, and *Obolella sagittatus*, which extend through the whole series; and also by the genus *Paradoxides*, four species of which occur in the lower strata, from which the genus *Olenus*, which characterises the Lingula flags, seems to be absent. To a large tuberculated trilobite of a new genus found in these lowest rocks the name of *Plutonia Sedgwickii* has been given. Hicks has proposed to unite the Menevian with the Harlech beds, and to make the summit of the former the dividing line between the Lower and Middle Cambrian, a suggestion which has been adopted by Lyell. (Proc. Brit. Assoc. for 1868, p. 68, and Lyell, Student's Manual of Geology, 466—469.)

Both Phillips and Lyell give the name of Upper Cambrian to the Lingula flags and the Tremadoc slates, which together constitute the Middle Cambrian of Sedgwick, and concede the title of Lower Silurian to the Bala group or Upper Cambrian of Sedgwick. The same view is adopted by Linnarsson in Sweden, who places the line between Cambrian and Silurian at the base of the Llandeilo or the second fauna. It was by following these authorities that I, inadvertently, in my address to the American Association for the Advancement of Science in August 1871, gave this horizon as the original division between Cambrian and Silurian. The reader of the first part of this paper will see with how much justice Sedgwick claims for the Cambrian the whole of the fossiliferous rocks of Wales beneath the base of the May Hill sandstone, including both the first and the second fauna. I cannot but agree with the late Henry Darwin Rogers, who, in 1856, reserved the designation of "the true European Silurian" for the rocks *above* this horizon. (Keith Johnston's Physical Atlas, 2nd ed.)

T. STERRY HUNT

(*To be continued*)

ACOUSTICAL EXPERIMENTS*

SHOWING THAT THE TRANSLATION OF A VIBRATING BODY CAUSES IT TO GIVE A WAVE-LENGTH DIFFERING FROM THAT PRODUCED BY THE SAME VIBRATING BODY WHEN STATIONARY

The Apparatus

FOUR tuning-forks mounted on resonant cases and giving the note UT^3, = 256 complete vibrations per second, were obtained. I will designate them as Nos. 1, 2, 3, and 4.

Nos. 1 and 2 were brought into perfect unison by a process to be described.

No. 1 was placed before a lantern, and just touching one of its prongs was a small ball (5 or 6^{mm} diam.) of good cork, suspended by a silk fibre. The images of the fork and of the cork ball were projected on a screen.

No. 3 had one prong weighted with wax, so that it gave two beats a second with No. 1 or 2.

No. 4 had the ends of its prongs filed off, until it also gave two beats per second with 1 or 2; thus No. 4 gave two vibrations a second more than No. 1, while fork No. 3 gave two vibrations a second less than No. 1.

The Experiments

In the experiments one to seven inclusive, fork No. 1 remains before the lantern, with the suspended cork ball just touching one of its prongs.

EXP. 1. Fork No. 2, screwed on its case, was held in the hand, at a distance of 30 to 60 ft. from No. 1, and sounded; the ball was projected from the prong of fork 1, which vibrated in unison with 2.

EXP. 2. I stationed myself 30 ft. distant from fork No. 1, and fork No. 2 was screwed off its case and vibrated in one hand, while the case was held in the other. I now walked rapidly toward fork 1, and after I was in regular motion I placed the fork on its case, and just before I ceased walking I took it off; although, when I did so, I was only about a foot from fork 1, yet the cork ball remained at rest against its prong.

EXP. 3. Again I walked toward 1, as in Exp. 2, but I did not remove the fork from its case after it was placed on it. The ball remained at rest until the moment I suddenly stopped walking; at that instant the ball flew from the fork, while an assistant, whose ear was close to the case of fork 1, while his eye was directed to the screen, found that at the instant I stopped walking, the fork 1 sounded, while the ball jumped from its prong.

EXPS. 4 and 5. These experiments were exactly like Exps. 2 and 3, except that I walked away from fork 1 instead of approaching it. The results were the same as in Exps. 2 and 3.

EXP. 6. Fork No. 3, giving 254 vibrations per second, was sounded as in Exp. 1. It had no effect in moving the ball. I now screwed the fork off its case, and, standing about 30 ft. from fork 1, with my arm I swung the case toward fork 1, and while it was approaching it I placed fork No. 3 on the case; the proper velocity (from eight to nine feet per second) having been obtained, the ball was suddenly projected from fork 1. On greatly increasing or decreasing the above velocity of the moving case, the vibrations of fork 3 produced no effect on fork 1.

EXP. 7. Fork No. 4, which gives two vibrations per second more than No. 1, was substituted in Exp. 6, but was placed on its swinging case when this was receding from fork 1. The effect of this motion and of varying velocities was the same as in Exp. 6.

EXP. 8. I placed fork 3 before the lantern, and swung fork 1 as in Exp. 7. The effects were the same as described in Exp. 7.

EXP. 9. I now placed fork 4 before the lantern, and moved fork 1 as in Exp. 6. The effect on the ball was the same as in Exp. 6.

By these simple experiments I have shown the change of wave-length produced by the translation of the vibrating body, and have given an experimental proof of the important theorem which Döppler established in 1841. By analogy they clearly unfold that exquisite modern method of determining the motions of a heavenly body by variations in the refrangibility of the rays which it emits—motions often impossible even to detect by any other means. I therefore deem it proper that I should proceed to state the delicate conditions on which depend the perfection of experiments which so satisfactorily elucidate the nature of those grand and refined problems offered to spectral observation.

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* By Alfred M. Mayer, Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey (reprinted from the *American Journal of Science and Arts*, vol. iii., April, 1872).

It is, first of all, essential that forks 1 and 2 should really be in unison. Two forks, sounded together, may give no perceptible beats, for they may constrain each other into a common forced oscillation, and thus both will give the same number of vibrations, yet may be removed from equality when separately vibrated. The process I have adopted is as follows: Three forks are taken which are supposed to give the same number of vibrations in a given time. They are supported on india-rubber tubing, and are thus insulated. One of the forks is now loaded so that it gives two or three beats in a second, with one of the other two that are to be brought into exact unison. The interval of time occupied by twenty or thirty of these beats is accurately determined by means of a chronograph (one of Casella's registering stop-watches does very well). The interval occupied by the same number of beats given with the second fork is now ascertained, and if it differs from that given by the first, the quicker vibrating fork is made to give the same number of beats as the slower by loading it with wax. When the forks have thus been carefully adjusted, I have had no difficulty in projecting the ball, in Exp 1, at a distance of sixty feet, and I believe that it could have been accomplished at a distance of 100 feet. The ball of cork should be *spherical*, so that it will always just touch the fork, no matter how much it may rotate around its suspending thread, which latter should consist of only one or two fibres of unspun silk. The cork is rendered as smooth as possible and is then *varnished;* this is important, for the varnish gives a firm coating to the ball, without sensibly increasing its weight, and is especially useful in covering the minute asperities or elastic projections on its surface, which otherwise would act as "buffers" to the impacts of the fork and deaden its projectile effects.

The above-stated conditions having been obtained, no physicist will have any difficulty in repeating these experiments.

A machine has been devised by which a uniform motion of translation can be given to the forks, and with this I propose making a quantitative investigation of the pheuomena, using an apparatus essentially the same in its action as the one here described.

We may substitute for the suspended cork-ball a light plane mirror, held between two stretched vertical fibres, while one of its edges touches the fork. The motions of a beam of light reflected from the mirror to a screen, indicate most beautifully the vibrations of the fork. This ingenious and most delicate device for detecting vibrations is due to Prof. O. N. Rood, of Columbia College, N.Y., who first used it in a public lecture, delivered in New York on the 28th of last December. We have, however, in our special work, found the image of the projected ball more convenient, and sufficiently delicate, for our experiments.

Quantitative relations in the experiments and analogical facts in the phenomena of light.

The UT_3, No. 1 fork, makes 256 complete vibrations in one second, while fork No. 3 makes 254, giving for the respective wave-lengths of these vibrations 4·367 and 4·401 feet, which we will designate in order as λ and λ'. We will take 1,118 feet per second as the velocity of sound at 60° F.

Now 256 vibrations in 1,118 ft. make $\lambda = 4{\cdot}367$ ft.
and 254 „ „ $1{,}118 - 2\lambda\ (= 1{,}109{\cdot}266)$ give $\lambda = 4{,}367$ ft.

As the velocity of propagation of the vibrations and λ are the same in both cases, it follows that $\left(n = \frac{V}{\lambda}\right)$, the number of vibrations in a second, reaching a distant point, is the same, and, therefore, 256 vibrations from a body at rest will produce the same effect on a distant surface, as 254 vibrations emanating from a body which moves toward that surface, with a velocity of 2λ, or of 8·734 feet per second; and this is the velocity we gave the fork in Exps. 6 to 9.

We will now examine the analogical phenomena in the case of light. Let fork No. 1, giving 256 vibrations a second, stand for 595 millions of millions vibrations a second, which we will take as the number of vibrations made by the ray D_1 of the spectrum. Then fork No. 3 will represent 590 millions of millions vibrations per second, which gave a wave-length ·0000042 millimetre longer than that of D_1, and nearly corresponds with an iron line situate ·42 div. below D_1 on Angström's chart. We saw that fork No. 3, giving 254 vibrations a second, had to move toward the ear with a velocity of 8·734ft., to give the note produced by 256 vibrations per second, emanating from a fixed point; so a star sending forth the ray which vibrates 590 millions of millions times a second, will have to move toward the eye with a velocity of 28,470 miles per second to give the colour produced when ray D_1 emanates from a stationary flame.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, October 1871. Naumann has made a long series of experiments on the dissociation-tensions of ammonic carbonate, he finds that when it is volatilised it is entirely decomposed into ammonia and carbonic anhydride, and that for lower temperatures the dissociation-tensions of this body increase by increase of temperature precisely as the tensions of other substances. Leist has obtained three compounds of bismuth oxide with sulphuric acid, all of which are basic salts, he has not been able to form the normal salt except in combination with potassium. Faust has made a series of experiments on the derivatives of phthalic acid, he has obtained nitrophthalic, bromophthalic, and dechlorophthalic acids. Faust and Saame have made a careful examination of the chloro-compounds, both addition and substitution of naphthaline: this work has already been performed many years ago by Laurent; the authors have thought fit to commence a revision of the subject, but it is as yet far from complete. A very long paper by Schutzenberger follows "on the acetyl derivations of carbo-hydrates, mannite and its isomerides, and on certain vegetable products," this contains some interesting though complicated results. A translation of Dr. Mills' paper on the nitration of chloroform, and two other papers of less interest complete this number.

Annales de Chimie et de Physique, March 1872.—The greater portion of this number is occupied by the second part of MM. Pierre and Puchot's researches on some of the bodies produced in fermentation. They give the results of a very detailed study of propylic alcohol, its haloid ethers, the formiate, acetate, propionate, butyrate, and valerate, and propylic aldehyde; butylic alcohol and the same series of ethers as above, and amylic alcohol with its butyrate and valerate. Besides these we have the detailed description of several other ethers, methyl valerate, and ethyl propionate and valerate, forming altogether a very complete and exhaustive monograph on these subjects. The author has also made some interesting observations on the "simultaneous distillation of water with certain alcohols insoluble therein." Thus a mixture of water and amylic alcohol, when submitted to distillation, boils at 96°, and a definite proportion of the two bodies is found in the distillate, at this temperature 2 parts of water and 3 of amylic alcohol invariably condense; should the water be in excess the whole of the amylic alcohol will pass over, the thermometer remaining at 96°. Butylic alcohol and water distil over at 90·5° when a constant mixture of 5 parts of alcohol and 1 part of water condenses.—M. Bourgoin has electrolysed a solution of potassic phthalate, and finds that it splits up into water, carbonic oxide, and carbonic anhydride, an aqueous solution of phthalic acid does not appear to be decomposed by the electric current.

THE *Scottish Naturalist* for April contains a number of short articles on various branches of Scottish Natural History. Among the more interesting may be mentioned especially a note by Dr. Buchanan White on the discovery in Braemar of a colony of *Zygæna exulans*, a common moth in the Alpine districts of Southern Europe and in Scandinavia, but hitherto unknown in Britain. Dr. White considers it, like some of the characteristic plants of the district, a relic of the glacial epoch which once overspread Scotland; its characters are intermediate between the northern and southern forms.—Mr. George Sim contributes an important paper, comprising a list of the stalk-eyed Crustacea of the north-east coast of Scotland, with descriptions of new genera and species, and a plate.—The instalment of the catalogues of Insecta Scotica includes a continuation of the Lepidoptera by Dr. Buchanan White, and the commencement of the Coleoptera by Dr. D. Sharp.

SOCIETIES AND ACADEMIES

London

Royal Society, May 2.—"On some Elementary Principles in Animal Mechanics.—No. V. On the most perfect form of a Plane Quadrilateral Muscle connecting two Bones.—No. VI. Theory of Skew Muscles, and investigation of the conditions necessary for Maximum Work." By Rev. Prof. Haughton, F.R.S.

"On the Rings produced by Crystals when submitted to Circularly Polarised Light." By William Spottiswoode, Treas. R.S.

Geological Society, April 24.—Prof. Ramsay, F.R.S. V.P. in the chair. 1. "An Extract from a Despatch from H M. Minister in Teheran." This letter described the effects of some severe earthquake shocks experienced at Khabooshan in North-Western Khorassan. On December 23, 1871, an earthquake occurred which destroyed half the town of Khabooshan, and buried about 2,000 of its inhabitants in the ruins. On January 6, 1872, another severe shock destroyed the remainder of the town, and killed about 4,000 people. Four forts near the town were so completely buried that not a trace of them can be seen. It was estimated that 30,000 lives were lost in Khabooshan, Bojnoord, and the surrounding villages by the effects of these earthquakes. 2. "Notes on the Geology of the Colony of Queensland," by R. Daintree. The author stated that Alluvial deposits are very scanty in Queensland, except on the northern shores of Carpentaria and near the mouths of the larger rivers. The fossil remains of extinct Mammalia (*Diprotodon, Macropus, Thylacoleo, Nototherium*, &c.) are found in old brecciated alluvia, representing beds of old watercourses, through which modern creeks have cut their channels. With these mammalia are found shells of existing species. Of Cainozoic deposits the most important is called the "Desert Sandstone" by the author; it consists of horizontal beds of coarse grit and conglomerate, nowhere exceeding 400 feet in thickness, forming a sandy barren soil by their disintegration. The only fossils found in it are rolled fragments of coniferous wood; and its stratigraphical position is determined solely by its resting unconformably upon beds containing apparently Cretaceous fossils. The author considered that this deposit formerly covered nearly the whole of Australia. Beds containing Mesozoic forms of fossils, and referred by the author to the Cretaceous series, occur upon the Upper Flinders. At Marathon these deposits consist of a fine-grained yellow sandstone, and below this a series of sandstones and argillaceous limestones, containing four species of *Inoceramus*, with a species of *Ichthyosaurus* and two of *Plesiosaurus*. At Hughenden station, near Mount Walker, there s a series of calcareo-argillaceous beds, probably inferior to hose of Marathon, and containing two species of Ammonites, with *Avicula gryphæoides*, a *Pecten*, &c. At Hughenden Cattle Station, twenty miles farther up the river, numerous Belemnites are found loose upon the surface. The Mesozoic rocks also extend down the Thompson River and its tributaries. The author referred to the fossils described by Mr. Charles Moore as probably Oolitic, and stated that it is more than probable that Oolitic and Cretaceous rocks extend throughout the whole of Central Queensland, and thence to Western Australia. On the eastern side of the dividing range a small patch of ferruginous grit containing *Panopæa plicata* occurs near Pelican Creek; and from Gordon Downs species of *Panopæa, Pholadomya*, and *Cucullæa* have been obtained. These beds probably represent a ower horizon than those on the Flinders River; and a large ortion of the colony east of the dividing range is covered by eshwater deposits, containing plant-remains (including *Tæniopteris*), and in their upper part a fauna-apparently intermediate etween the Gordon Downs and Flinders River series. In these eposits, on the Cnodamine, Brisbane, and Mary rivers, numerous Coal-seams exist. The author supposes that, contemporaneously ith the deposition of a series of marine beds to the west of the ividing range, during the Oolitic and part of the Cretaceous eriod, a vast lacustrine deposit was accumulated over a large rea to the eastward of the range, to which the sea subsequently otained access. Among the Palæozoic deposits, the author istinguished Carboniferous and Devonian rocks. The *Carbonerous* series was said to be represented in Northern Queensland y an extensive Coal-field. The upper portion of the series rits, sandstones, and shales) contains chiefly fossil plants, the ost abundant being a *Glossopteris*. The lower strata (generally *gillaceous* limestone) contain *Producti, Spiriferæ*, &c. of true arboniferous type, intermixed with scanty and imperfect remains the above-mentioned plants. A set of fossils from the head of the Don River were said to agree with those found in the Hunter River series of New South Wales. *Devonian* rocks extend from 18° S. lat. to the southern boundary of Queensland and for 200 miles inland. They consist of slates, sandstones, and Coral-limestones. The upper portion of this series contains an abundance of fossil plants, the deposits containing which, at Mount Wyatt, are interstratified with beds containing *Spiriferæ*, and other fossils of Devonian type occur in beds reached by shafts sunk through these strata. In the limestone of the lower portion of the series corals are very numerous. On the Broken River this formation may be best studied. Gold is found in many parts of the Devonian district, and the author entered in considerable detail into its mode of occurrence there. Metamorphic rocks were described by the author as occurring in various localities. At the Cloncurry, Cape River, Gilbert, Peak Downs, Black Snake, Kilkwan, and Goaroomjain Diggings there are mica- and hornblende-schists, whilst at the Ravenswood Diggings the rock is a granite with triclinic felspar. The latter, which contains more or less hornblende, the author regarded as of metamorphic origin. The author noticed the connection between the presence of certain trappean rocks in these metamorphic areas and in the Devonian area, and the production of auriferous and cupriferous lodes. True *Granites* crop out along the eastern coast of Queensland, and these vary much, passing into porphyry and quartz-porphyry, but monoclinic felspar always predominates in them. The intrusive Trappean rocks, which are regarded as influencing the production of auriferous vein-stones in the Devonian and Metamorphic rocks, are noticed at considerable length by the author, and consist of pyritous porphyrites and porphyries, pyritous diorites and diabases, chrome-iron serpentines and pyritous felsites; the author considers that this order probably indicates the succession of these rocks in time. The veinstones he thinks were probably deposits of mineral matter from the hydrothermal action which preceded, accompanied, and continued long after the cooling of the traps themselves. The volcanic rocks, in the author's opinion, have played a most important part in determining the elevation and present physical outline of North-eastern Queensland; they follow the line of greatest elevation on the main watershed at altitudes of from 1,500 to 2,000 feet above the sea-level. The general arrangement of the other rocks referred to is epitomised by the author as follows:—"With the exception of the McKinlay ranges, a line drawn parallel with the eastern coast at a distance of 250 miles would include all the Palæozoic, Metamorphic, Granitic, Trappean, and Volcanic rocks represented in the colony, both coal-groups lying within the same area. The Mesozoic and Cainozoic systems occupy the surface area to the westward. The descent going eastward is first locally a thin capping of 'Desert Sandstone,' next Carboniferous, then Devonian, and possibly Silurian, with patches of metamorphic and granitic rocks interspersed. The chief granitic mass extends from Broad Sound to Cape York, with an occasional capping of 'Desert Sandstone.'" The paper contained numerous analyses of the various rocks, and the fossils have been worked out by Messrs. Etheridge and Carruthers, whose lists and descriptions of them are appended to the paper.

Linnean Society, May 2.—Mr. G. Bentham, president, in the chair.—Dr. Joseph Leidy, of Philadelphia, and Prof. Notaris, of Genoa, were elected to the two vacant places in the list of foreign members.—On *Alibertia edulis*, by Senor Correa de Mello. —Mr. Miers exhibited a substance which he had received from the Brazilian Government, which it was thought might, to a certain extent, become a substitute for cotton. It is a product of the liber of a climbing plant of unknown relationship, and can be procured in any quantity, furnishing a fibre of very strong and silky texture.

Anthropological Institute, May 6.—Sir John Lubbock, Bart., president, in the chair. The following papers were read:—"Note on the Peculiarities of the Australian Cranium," by Mr. S. M. Bradley, F.R.C.S.; "Notes on a Scaphoid Skull," by Dr. Barnard Davis, F.R.S.; "On Certain Points concerning the Origin and Relations of the Basque Race," by Rev. W. Webster and Mr. Stuart Menteath; "Mann: its names and their origins," by Mr. J. M. Jeffcott; "Vocabulary of Original Dialects of Queensland," by Mrs. Barlow; "On the Mode of Preparing the Dead among the Natives of the Upper Mary River, Queensland," by Mr. A. McDonald.

Dublin

Natural History Society, February 7.—Prof. E. Perceval Wright, M.D., in the chair.—The following gentlemen were elected as officers and council of the society for the present session:—President—Prof. E. Perceval Wright, M.D.; Vice-Presidents—Mr. William Archer, Dr. Alexander Carte, Dr. Robert M'Donnell, Lord Ventry; Honorary Treasurer—Mr. A. Andrews; Honorary Secretaries—Mr. William Andrews and Dr. A. W. Foot; Council—Mr. R. Ball, Mr. H. Barton, Rev. S. Haughton, M.D., M. A. Jacob, M.D., Mr. T. Kift, Mr. A. Macalister, M.B., Mr. D. Moore, Mr. M. Barrington, Mr. Edward Crowe, Dr. Fraser, Rev. T. O'Mahony, M.A., Rev. Eugene O'Meara, M A, and Mr. George Porte. Dr. E. P. Wright returned thanks to the members for the honour they had conferred upon him, and stated that, at the suggestion of the Hon. Secretary, he would defer the introductory address to the next meeting of the Society.

KILKENNY

Royal Historical and Archæological Association of Ireland, April 3.—Rev. P. Moore in the chair.—The secretary exhibited an ancient ecclesiastical seal of the Primatial See of Armagh, and read a report on the state of the Round Tower of Kilmacduagh, County G lway. The following papers were read: "On the old Church of Donaghmore, County Limerick, with a photograph," by the Rev. M. Malone; "On the old Kilkenny Canal," by A. Walters; "On the Corrack or Ancient Wicker Boat covered with Skin," by W. F. Wakeman; "On an Ancient Bell found near the old Church of Drumrath, County Tyrone, with a photograph, and on a Silver Ring-brooch found in the Cran nog of Aghalougher, County Antrim," by J. Nolan.

VIENNA

Imperial Academy of Sciences, March 21.—Prof. Hlasiwetz presented a memoir by M. A. Exner, on the synthesis of hyponitric acid, N^2O^4.—Prof. Suess made a preliminary communication on the structure of the Italian Peninsula, in which he showed that the mountain chain which forms the Calabrian peninsula is a fragment of the tectonic axis of the peninsula, but that the continuation of this axis lies concealed under the Tyrrhenian sea. The southern half of the western part of the Alps is also sunk beneath the plain of Lombardy. The Appenines form the north-eastern, and Sicily a fragment of the south-western, subsidiary zone of the Tyrrhenian mountain chain; and the volcanoes stand for the most part either in series on the margins of fracture, or in groups in the middle of the regions of depression. The relation of the Hungarian trachytes to the Carpathians is the same as that of the Italian volcanoes to the Appenines.—Prof. E. Weiss reported upon the difference of longitude between the observatory of Vienna and that at the Military Academy of Wiener-Neustadt.—Dr. H. W. Reichardt reported upon the Botanical Results of the Polar Expedition of 1871. The number of species brought by Lieutenant Payer was about thirty; they were collected in the southern part of Spitzbergen and some adjacent islands, and in Hope Island.

BOOKS RECEIVED

ENGLISH.—Extracts from the 13th vol of the Astronomical Observations made at the Royal Observatory, Edinburgh: C. P. Smyth (Neil and Co., Edinburgh).—Botany for Beginners, Dr. M. T. Masters (Bradbury and Evans).—Beeton's Science, Art, and Literature, a Dictionary of Universal Information, Vol. i (Ward, Lock, and Tyler).—The Martyrdom of Man: Winwood Reade (Trubner and Co.).—Spiritualism Answered by Science, 2nd edition. E. W. Cox (Longmans)

PAMPHLETS RECEIVED

ENGLISH.—Journal of the Quekett Microscopical Club, April.—Concerning Sewage and its Economical Disposal: F. H. Danchell—The Ukara Lake: R. F Burton.—The Scottish Naturalist, April.—Quarterly Journal of Science, April.—Journal of the Statistical Society, March.—A Series of Chemical Labels for Use in Laboratories: Mottershead—University of Cambridge. Report of the Museums and Lecture Rooms Syndicate.—On the Curability of Cancer: Dr. G. von Schmidt.—Proceedings of the Cleveland Institute of Engineers, March—Currents and Surface Temperature of the North Atlantic Ocean—Proceedings of the Bristol Naturalists' Society, 1871—An Appeal to Reason to Reform Itself.—39th Annual Report of the Royal Cornwall Polytechnic Society, 1871.—The Miners' Association of Devon and Cornwall, Report of Annual Meeting—Report of the Rugby School Natural History Society, 1872.—Quarterly Journal of Microscopical Science, April.—Proceedings of the Royal Physical Society of Edinburgh, 1870–71—On Teaching Geology and Botany as part of a Liberal Education: J. M. Wilson—Annual Report of the Maidstone and Mid-Kent Natural History and Philosophical Society, 1871.

AMERICAN AND COLONIAL.—Third Annual Report of the State Board of Health, Massachusetts.—On two new Ornithosaurians from Kansas: E. D. Cope—Some Phases of Modern Philosophy: E. K. Price.—The Use and Origin of the Arrangements of Leaves in Plants: Dr. Chauncey Wright.—A Continuation to a Catalogue of Maps of the British Possessions of India.—The Development of *Limulus polyphemus*: A. S. Packard.—On the Families of Fishes: E. D Cope.—Historical Notes on the Systems of Weather Telegraphy in the United States: C. Abbé.—The Cincinnati Medical News, Vol. i, No. 3—The Indiana Journal of Medicine, Vol ii., No 2—Medical Education in America. J. H Bigelow.—Preliminary Report of the United States Geological Survey of Montana: Prof. Hayden—Canadian Naturalist, Vol vi, No 3.—Report of the Chief Commissioner of Mines for the Province of Nova Scotia, 1871.—Acoustical Experiments: Alfred M. Mayer.

FOREIGN—Öfversigt af konig Vetenskaps Akad. Forhandlinger.—Anwendung der Darwinschen Lehre auf Bienen: H. Muller—Memorie della Societa degli Spettroscopisti Italiani, 3 nos.—Bulletin de la Société d'Anthropologie de Paris, July and August, 1871.—Contributions to the Biology and History of the Development of the *Ustilagineæ*: Dr. A Fischer von Waldheim.—La Belgique Horticole, March and April.—k. k Akademie der Wissenschaften zu Wien, No 7. 1872—Académie Royale de Belgique, 1872.—Berichte der k sächsischen Gesellschaft der Wissenschaften, July, 1871.—Notes sur des singes fossiles trouvés en Italie: C. J. Forsyth Major.

DIARY

THURSDAY, MAY 9.

SOCIETY OF ANTIQUARIES, at 8.30—Inventories of Westminster, Waltham, and St. Albans: Rev. M. E. C. Walcott, F.S A.

LONDON INSTITUTION, at 7.30.—On Solution and Supersaturation: C. Tomlinson, F.R.S.

MATHEMATICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—On Heat and Light: Prof. Tyndall, F.R.S

FRIDAY, MAY 10.

ASTRONOMICAL SOCIETY, at 8.

QUEKETT MICROSCOPICAL CLUB, at 8.

ROYAL INSTITUTION, at 9.—On Meteoric Stones: Nevil Story-Maskelyne.

SATURDAY, MAY 11.

ROYAL INSTITUTION, at 3—The Star-Depths: R. A. Proctor.

GOVERNMENT SCHOOL OF MINES, at 8.—On Geology: Dr. Cobbold, F.R.S.

MONDAY, MAY 13.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

TUESDAY, MAY 14.

ROYAL INSTITUTION, at 3—On the Development of Belief and Custom amongst the Lower Races of Mankind: E. B. Tylor, F.R.S.

PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, MAY 15.

SOCIETY OF ARTS, at 8.—On a New Mode of Utilising Sewage Precipitates: Major-General H. Y. D. Scott, C.B.

PHARMACEUTICAL SOCIETY, at 11 A.M.—Anniversary Meeting.

THURSDAY, MAY 16.

ROYAL SOCIETY, at 8 30.

SOCIETY OF ANTIQUARIES, at 8.30.

CHEMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—On Heat and Light: Prof. Tyndall, F.R S.

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NOTICE

We beg leave to state that we decline to return rejected communica- tions, and to this rule we can make no exception. Communica- tions respecting Subscriptions or Advertisements must be address to the Publishers, NOT *to the Editor.*

THURSDAY, MAY 16, 1872

EXAMINATIONS IN TECHNOLOGY

NO subject has been more talked about of late years than Technical Education. No term has been more vaguely or indefinitely used than this, even in education, that region of loose definition; yet it cannot be doubted that at the present time no subject is of more vital importance to this country, to enable it to maintain its manufacturing position, than a general diffusion of sound technical knowledge—a knowledge, that is, which rests on a thorough apprehension of the scientific principles which lie at the root of the various arts and manufacturing processes.

Bacon, in the first of his "General Aphorisms for Interpreting Nature and Extending the Empire of Man over Creation," says:—"Man, who is the servant and interpreter of Nature, can act and understand no further than he has, either in operation or in contemplation, observed of the method and order of Nature." And he proceeds, "neither the hand without instruments, nor the unassisted understanding, can do much; they both require helps, the understanding no less than the hand, to fit them for business. . . . . The knowledge and power of man are coincident; for whilst ignorant of causes he can produce no effects, nor is Nature to be conquered but by submission. And that which in speculation stands for the cause, is what in practice stands for the rule." The men of science of our day are not open to Bacon's rebuke to the mathematician, the physician, and the chemist of his day, that they were concerned in the works of Nature, but all of them superficially and to little purpose. Day by day sees them conquering Nature by submission. Are the thousands engaged in our manufacturing industries capable of taking advantage of their conquests? How painfully true with respect to them still are his words, written some 250 years ago, "The works hitherto discovered are owing rather to accident and trial than to the sciences."

Playfair remarks on this, "One of the considerations which appear to have impressed Bacon's mind most forcibly was the vagueness and uncertainty of all the physical speculations existing in his time, and the entire want of connection between the Sciences and the Arts." The vagueness and uncertainty of physical speculation is rapidly clearing away. Is the connection between the Sciences and the Arts as rapidly being cemented? We fear not. The world has become imbued with the truth of Bacon's saying, that "in works men can do no more than put natural bodies together and take them asunder; all the rest is performed by the internal operations of Nature." But how little is the order of Nature which should regulate this putting together and taking asunder understood in our workshops! How much is trusted to trial and error! How little is the store of knowledge accumulated by our men of science drawn upon! How great is the waste of our resources!

We therefore hail with satisfaction a scheme of technological examinations proposed by Captain Donnelly, R.E., which the Society of Arts has adopted on the recommendation of an able committee of scientific and professional men to whom it was referred, and which it appears, from a paper lately issued, the Society intends to carry out if the requisite support be forthcoming.

The proposal of Captain Donnelly is briefly that every year the Society of Arts should arrange for the examination in the science and technology of certain arts and manufactures. A committee qualified to advise on the subject is to prepare a syllabus of the examination in each branch of industry. It is an instruction to them "that it is essential that the candidate should possess, on the one hand, such an elementary knowledge, at least of science, as will prove that he understands the scientific principles of which his art is an application; and, on the other hand, such a knowledge of the application of those principles in his trade, as will show that he is practically conversant with the various processes and manipulations of the factory or workshop. The theoretical knowledge must not be a mere 'cram' of empirical dicta, nor the practical knowledge a mere committal to memory of descriptions of manufactures picked up from text-books." This instruction shows that the technology which it is proposed to cultivate by means of these examinations is thoroughly sound.

The requirements from a candidate fall naturally under three heads. We have first those branches of abstract science which are involved in the special industry under consideration; secondly, the special applications of those abstract or general sciences to that industry; and lastly, a practical knowledge of the machinery, processes, and manipulation.

The examinations of the Science and Art Department, which are now held pretty generally in all parts of the kingdom, and which can be extended to any place which desires to avail itself of them, by the simple process of forming a local committee of superintendence, provide the ready means of testing the candidates' knowledge of any branches of general science. It is only necessary then to determine what branches of science are the foundation of the technology of any industry, and to specify the examination which the candidate shall pass for each grade of certificate. The Society of Arts, working in concert with the Science and Art Department, proposes to avail itself therefore of these examinations to determine the candidate's knowledge of pure science.

As respects the technology or special applications of general science, the committee will prepare a syllabus for each industry. And the examination in these matters will also be conducted by means of the local committees after the general science examinations, the papers of questions being prepared by special examiners, to whom the answers will be submitted. Finally, the candidate's practical knowledge will be ascertained by a return of his employment in the factory or workshop, giving his rate of wages, &c., certified by his employer, somewhat in the form of the return required from candidates for Whitworth Scholarships. No more reliable criterion of a candidate's practical knowledge could be afforded than this. It is in the workshop, and in the workshop alone, that a true practical education can be obtained. It is a great advantage that this scheme wholly avoids running counter to the just susceptibilities of our manufacturers on this cardinal doctrine, and proposes simply to strengthen and ennoble this practical education by combining it with sound scientific instruction. The whole machinery of the examination is simple, effective, and, by means of local co-operation—a machinery which already exists—readily applicable, at small cost, to all parts of the kingdom.

The examinations are to be adapted to three grades of certificates: an elementary, or "Workman's" certificate; an advanced, or "Foreman's" certificate; and an honours, or "Manager's" certificate.

No syllabus has yet been issued, but we have seen the syllabus for paper manufacture, which, though not finally adopted by the committee, is in a forward state. How many candidates will come up to the mark? We fear but few. There is no use shutting our eyes to the fact. Among manufacturers how many are there who could pass a fair examination in the Science and Technology of their trade? And yet the committee have not pitched their standard too high. One great benefit—if no other—will be conferred by these committees. They will show at least what ought to be known.

As a commencement, Captain Donnelly proposes that those industries should be taken for examination which form the subjects of the Annual International Exhibitions, and that the Royal Commissioners for the Exhibition of 1851 should be asked to provide the requisite funds. The committee endorse Captain Donnelly's view, and think that the Council of the Society of Arts "may find it advantageous to include those Arts and Manufactures." We trust that it may be found practicable to include annually many more, and that this important movement will not be left to depend on the Annual Exhibitions alone, however ready the Commissioners may be to support it. It is for the manufacturers of this country, for the City Companies, and the large towns, whose very existence depends on their manufacturing supremacy, to come forward and aid this important work, and do for their several industries what Sir Joseph Whitworth has done for mechanical engineering by his noble endowment.

By this means a stimulus will be given to the extension of scientific instruction; an aim and organisation afforded of which it stands in much need; and a decided step taken to re-establish our manufacturing supremacy, which, in consequence of the superior educational position of our continental rivals, is now trembling in the balance.

WATTS'S DICTIONARY OF CHEMISTRY

A Dictionary of Chemistry. By Henry Watts, F.R.S., B.A., &c. *Supplement.* (Longmans, Green, and Co., London, 1872.)

ENGLISH chemists will hail with gladness the appearance of the supplemental volume to "Watts's Dictionary." It was evident almost before the completion of the last volume of the original work, that a supplementary volume would be required very shortly. In these days of progress chemical books are quickly left behind, and it needs energetic measures for our literature to keep pace with fresh chemical discoveries. Chemistry has much to be thankful for at the hands of Mr. Watts. The present volume brings up our knowledge to the end of 1869, and also includes several additions, corrections, &c., which have appeared in 1870 and 1871. The scope of this volume is, as in former volumes, sufficiently wide; the contents are not entirely confined to chemistry, but include articles on electricity, heat, light, &c. The connection between these subjects and chemistry is so close that no book would be perfect which did not enter into and explain some of the effects caused by these forces. The plan of "Watts's Dictionary" is too well known to require any comment. The present volume is strictly a continuation of the former ones; and, as time rolls on, other supplemental volumes will be required to make this record of chemical history complete. As it is, we now possess in "Watts's Dictionary" a complete account of chemical discovery up to the end of 1869; and in the abstracts of foreign papers published by the Chemical Society we have a contemporaneous record of all new facts, beginning, however, with the year 1871. It is, perhaps, unfortunate either that Mr. Watts did not bring out his Supplement one year later, or that the Chemical Society did not commence their extremely valuable work one year earlier. At the present time, therefore, we have one year to a certain extent unrepresented. We have, however, gained a great step; instead of having to wait two or three years for the appearance of the "Jahresbericht," we have now the abstracts of foreign papers a month or so after their publication. It is worthy of remark that Mr. Watts's dictionary has outstripped the "Jahresbericht," the third volume of which, for 1869, has not yet appeared. The author has fortunately succeeded in obtaining the assistance of some of the former contributors to his work, thus Prof. C. G. Foster contributes two very clear articles on recent discoveries in electricity and heat, whilst Prof. Roscoe has written the articles on "Light and on Spectrum Analysis," which give a very excellent *résumé* of the work done in these branches of science, and which, perhaps, might have been lengthened with advantage. The article on "Proteids" is written by Prof. M. Foster, whose name is a sufficient guarantee for its excellence. Dr. Paul and Mr. Wanklyn have also contributed to the Supplement, the latter having written on acetic ether (in part), on butyl alcohols, &c. The only possible objection to this outside help is that, in some instances, undue prominence may be given to certain of the author's theories or remarks, to the comparative overlooking or slighting of the work of other chemists. We must not, however, omit to speak in the highest terms of many of the articles contributed by Mr. Watts himself, such as the extremely clear and succinct account of the aromatic series as explained by Kékulé's hypothesis, the article "On Atomicity," and many others too numerous to mention. We cannot give the volume greater praise than by saying it is quite equal to the former productions of the author. Since the publication of the last volume, the chemistry of the aromatic series seems to have usurped the principal attention of chemists, as we find by long articles on benzine, on its derivatives and homologues, no less than forty-five pages being thus occupied; then again the substitution derivatives of benzoic acid and of phenol occupy a considerable space. Another subject which seems to have attracted a considerable amount of attention, and to have yielded very interesting results, is that of the alcohol cyanides and nitriles. An article on inorganic analysis, by Bunsen's flame reactions, will be found of great interest, and will repay a considerable study. In fact, the whole volume is most complete, and must be looked on with great satisfaction.

A. P.

OUR BOOK SHELF

Index der Petrographie und Stratigraphie der Schweiz und ihrer Umgebungen. Von B. Studer, Professor der Geologie. Pp. 272. (Bern: K. Schmid. London: Williams and Norgate)

TWENTY years having elapsed since the publication of the "Geologie der Schweiz," Prof. Studer thinks that some new account of the geology of his country cannot be deemed superfluous. Since the date of that work numerous separate volumes, papers, maps, &c., relating to the geology of Switzerland have appeared. Many of these, however, are difficult of access, and not a few have been to all intents and purposes lost sight of. As a consequence of this, it is exceedingly difficult or even impossible for the student of Swiss geology to find out what has been written. This is easily understood when we remember that Switzerland has been a favourite field of study with geologists of all nations, and that descriptions of her rock-masses and formations are to be met with in the publications of almost every scientific society in Europe. Prof. Studer complains, and not without reason, that many of the names of rock-divisions and formations are derived from little obscure outlying places, for which we look in vain on the best maps, or from fossils which are familiar to only a few adepts, and that the same rock or formation, as the case may be, is known by different names in different regions, thus giving rise in the student's mind to confusion worse confounded. This index (the preparation of which must have cost its author a world of labour) will smooth the way to learners, and will, we are persuaded, be of scarcely less value to professors themselves. Petrological and stratigraphical synonyms are clearly explained, and the equivalents of the Swiss rocks met with in adjoining countries are given. The index is arranged alphabetically, and the list of "articles" leaves nothing to be desired. The descriptions are short, clear, concise, and at the same time comprehensive, those which relate specially to Swiss geology being of course the fullest. The author modestly says that his index makes no pretensions to be a text-book, and refers his readers for greater details to the works of Naumann, Zirkel, Senft, Cotta, &c.; yet we think that the very absence of minute details will be one of its chief recommendations to the geologist, who can always turn to the text-books and other sources when he feels inclined, for the index literally bristles with references. A long list of localities is added, by consulting which we are referred to the various articles in which they are mentioned. Thus, with a good map and Prof. Studer's index before him, one may gather a very clear conception of Swiss geology. The book is not bulky, and will be an invaluable companion to any geologist who thinks of trying his hammer in the "playground of Europe."

J. G.

On the Early Stages of an Ascidian (Cynthia pyriformis). By Edward S. Morse, Ph.D. (Boston: 1871.)

IN this communication, reprinted from the Proceedings of the Boston Society of Natural History, Dr. Morse gives an account of his examination of the tadpole-like larva of a sessile Tunicate at Eastport, Maine, in July 1870. He confirms the statements of Kowalewsky and Kupffer, and describes "a remarkable structure in the caudal fin, which vividly recalled the fine diverging rays seen in the embryo fish. These rays were exceedingly delicate, though plainly marked. They ran off nearly parallel to the longitudinal axis of the tail, and were confined to the last five segments." This observation, if confirmed, will be of importance; it points rather to general piscine affinities in the Tunicata than to their special connection with *Amphioxus.* We are glad to see that Dr. Morse is alive to the danger of mistaking the effects of preserving fluids for natural appearances in microscopic specimens. Some neat figures illustrate the paper, which we hope is only the beginning of more complete investigation of this deeply interesting subject by the writer.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Magnetism in Copper Slags

ON examining the magnetic properties of some ores and minerals, I observed that a specimen of ore furnace-slag from copper smelting was strongly polar magnetic.

Being surprised at this phenomenon, I mentioned it to Dr. Percy, Professor of Metallurgy at the Royal School of Mines, who kindly gave me permission to examine some of his slags, and also some of those exhibited in the Geological Museum.

I examined several specimens of ore furnace slags, and found they were all more or less magnetic and strongly polar; this even extended to some very small pieces the size of a pea. Most of these were of the ordinary kind, and of a porphyritic appearance, from the pieces of white quartz imbedded in their mass.

One specimen was of a vitreous character. This was not so strongly magnetic as the ordinary kind.

Metal slags from the second fusion.—Those examined were polar magnetic. They were Museum specimens, beautifully crystallised; the magnetic properties were distinct throughout the mass, though more feeble than in the ore slags.

Roaster slags from the third fusion.—I examined two specimens of this class; both were polar magnetic, but the magnetism was confined to a few points, and was not developed in the whole mass, resembling the consequent points in magnetism.

Refinery slags.—I examined one specimen; it was very feebly magnetic, though not polar, and the magnetism was confined to a few points in the mass.

In the analysis of copper slags, the iron present is always estimated as protoxide in combination with silica, forming a silicate of protoxide of iron. Unless this silicate is magnetic, it is difficult to understand how the whole of the iron is thus combined. Further analysis must decide this point.

EDMUND F. MONDY

The Volcanoes of Central France

GEOLOGISTS state that the volcanoes of Auvergne have not been in action in historic times (see Lyell, last ed., p. 479; also Jukes and Geikie, p. 354). I find, however, that the Rogation Days were appointed by Mamercus, Bishop of Vienne, in Gaul, about A.D. 460, for the purpose of chanting litanies to stay the volcanic eruptions which were then devastating his diocese (see Robertson's "Hist. Ch. Church," 4th ed, vol. i. p. 589; also "Proctor on Book of Common Prayer," note, page 251.)

Youghal, Co. Cork, May 13 W. J. GREEN

The Eruption of Vesuvius in 1855

IT has occurred to me that, at the present moment, the subjoined extract from the travelling notes of my husband, the late Dr. Marshall Hall, might be thought worthy of insertion in your valuable periodical. We happened to be at Naples when the eruption of Vesuvius in May 1855 occurred, of which the following gives some description:—

"During five years Vesuvius had remained in a state of inactivity, when, on May 1, 1855, indications of an eruption manifested themselves.

"Early on the morning of the 1st smoke and fire appeared, occupying a point on the north-west side of the mountain, and red-hot lava began to flow down the Atrio del Cavallo.

"On May 2 we all left our hotel at a quarter-past 5 P.M. for Vesuvius, driving up to the Hermiage, and then walking about a mile to the incandescent stream.

"The distant view presented a zig-zag line of fire. As we approached we saw the movement of this stream of lava; in some points solid masses were turned over and moved downwards; in others, bright points occurred where some tree or other combustible was inflamed. Nothing could be more splendid. It was a river of fire from fifty to one hundred yards in breadth.

"We had seen the Falls of Niagara and the glaciers of the Alps, all of a stupendous beauty; but this was sublime and fearful.

"The lava on which we stood, near this stream was warm and on raising a portion the substratum was red-hot. Was it quite safe to be there?

"The view was magnificent, and our position possessed quite sufficient of the fearful to make it sublime: a scene of moving molten masses of liquid fire.

"The stream issued, not from the summit of the mountain, as heretofore, but from its north-western side, on which seven apertures existed. From the highest of these a burst of fire took place upwards from time to time.

"The stream seems to issue in a viscid half-liquid state, whilst its surface, and especially its edges, cold by contact with the atmosphere, become solid, forming a channel and floating or rolling masses. The lava remains long incandescent, even when it has become solid, being a bad conductor of heat. On the same principle the masses of consolidated lava are formed in minor masses, giving to the general mass of surface the most irregular forms, frequently with sharp and prominent edges and projections. Even when these masses are the smallest, it is difficult and even dangerous to walk upon them.

"On Saturday, May 5, I ascended to the summit with Dr. Bishop and my son. From the great crater at the summit there issued much smoke, consisting chiefly of sulphurous acid gas. This proved extremely irritating to the nostrils and bronchia, inducing sneezing and coughing. Three openings existed near together along the edge of the crater at the very summit, which emitted a similar vapour. At points considerably lower, three larger openings were formed on the north side of the cone, from which immense quantities of smoke issued, mingled with fire, differing somewhat in colour, and depositing sulphur of a light green, orange, and bright yellow colour. At a distance the green sulphur, spread over ancient lava, was mistaken, even by our guide, for vegetation, but proved to be sulphur on a nearer examination.

"Below the third of these larger openings there was a cascade of red-hot lava from the edge of a precipice, of immense dimensions, presenting a sublime fall of liquid fire.

"Along this part of the cone masses of stone continually rolled down the slope, dislodged by the movement of the cone (for none were ejected), which some of our party felt distinctly.

"Below the fall the lava proceeded in a continuous stream, consisting partly of flowing, partly of rolling masses, pursuing an irregular course downwards in the Atrio del Cavallo, in one place dividing into two, in others taking a zig-zag turn. From its surface a dense smoke arose generally, but in some places the existence and combustion of a tree gave a bright blaze of light.

"The surface of some parts of the lava stream had already cooled and consolidated sufficiently to admit of our walking over it. This surface was crisp and wave-like, and in some parts sounded hollow when struck with our staffs.

"We picked up one specimen of porous lava, in a crevice of which a fly of considerable size was imprisoned, a miniature picture of Herculaneum or Pompeii. Our boots were torn to tatters."

CHARLOTTE HALL

Brighton, May 10

Earthquakes and Permanent Magnets

IN a notice by Dr. A. B. Meyer in No. 116, vol. v. of NATURE, respecting earthquakes in the Island of Celebes, he states that the often-repeated story of the keepers of permanent magnets detaching themselves, and falling at the moment of an earthquake shock, was never verified in his experience. If I remember rightly, this peculiar action was first mentioned by a Frenchman living on the west coast of South America, and much doubted by many at the time. It is not likely that there is any difference between the effect of an American and an Asiatic earthquake on the magnet, if any such exists; but I must state, in corroboration of Dr. Meyer's note, that I have had a permanent magnet and keeper suspended in my study for many years, expressly for the purpose of testing this matter, and on no occasion of an earthquake has the keeper fallen from the magnet, not even in the terrific earthquake of 1863, which was so destructive to the city of Manila and the neighbouring provinces.

The terrestrial disturbances still continue in the Philippines, and almost every post brings us intelligence of earthquakes in the provinces. More has occurred lately: an eruption of the Mayou, a magnificient volcano in the province of Albay, unaccompanied, however, by any serious disasters. As I have already mentioned in my notice of the new volcano which suddenly broke out in the island of Camiguin,* the past year has been remarkable for the great number of earthquakes throughout the Archipelago, especially in the great island of Mindanao, where the new military colony on the great river suffered greatly.

Manila W. W. WOOD

The Australian Eclipse Expedition

IN your number of NATURE for December 7, 1871, it is stated, in regard to the Eclipse Expedition, that "notwithstanding the supineness displayed by the other Australian colonies, it was still hoped that the Government of Victoria would render such pecuniary assistance as would make it possible for the Expedition to set out with some chance of success in obtaining results of scientific value."

Now, while giving all honour to the Royal Society of Victoria for originating and carrying out the expedition, it is only bare justice to the colonies of New South Wales and Queensland to state that, but for the liberal way in which they responded to the cause of science, there would have been no Eclipse Expedition here. No steamer could be obtained in Victoria within the means of the Royal Society, and, at the instance of the Government of New South Wales, who were moved by their astronomer, the Government of Queensland lent a steamer and contributed 115*l.* in money; while the Government of New South Wales contributed 330*l.* to the funds of the Royal Society of Victoria for the Eclipse Expedition, and upwards of 130*l.* towards the expense of steamer and their own observing party, besides sending Lieut. Gowlland, without whose aid in navigating the ship in those dangerous waters, the ship would not have been lent, and there would have been no Expedition here.

A SYDNEY MEMBER OF THE ECLIPSE EXPEDITION

Sydney, March 25

⁂ Our information was derived from correspondents in Australia. We gladly notify the correction, and give the Governments of Queensland and New South Wales their due.—ED. NATURE.

Waterspouts in the Fen-land

THE letter of Mr. Gray on a waterspout observed by him on the river Ebwy, which appeared in your number for April 25, vividly recalled a similar case which has come under my notice in the Fen-land.

During the summer of 1870, while in Deeping Fen on a day when the wind was blowing in gusts, carrying the dry powdery peat-dust in clouds before it, I observed a whirling column of dust advancing towards me. It was like those smaller pillars so frequently seen in the streets of a town on such a day, but was considerably larger, being from 15 to 20 feet in height. When it was first seen it was advancing from the far side of a *ground*, as the unenclosed fields are called, towards me at the rate of about six miles an hour, and was distant some five hundred yards. It moved with an unsteady staggering motion. accompanied with a rushing noise. I stayed to watch it cross a dyke about 15 feet wide, which ran directly across its path. The smaller dykes it seemed to cross without affecting them, but on reaching the one in question it whisked the water up into a waterspout some 10 feet high with a gurgling hissing sound, and steering directly across the dyke burst on reaching the opposite shore, projecting a considerable quantity of water upon the land. This effort seemed to spend its force, for the dust-column resumed on the opposite land was but small in proportion, and after swaying about for a few yards, died away.

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* In some accounts of this phenomenon, the island of Camiguin on the coast of Mindanao, has been confounded with another island of the same name in the Bataanes, to the north of Luzon.

I have been informed of similar cases in this district, but the above is the only instance in which I observed a waterspout myself.

SYDNEY B. J. SKERTCHLY

H.M. Geological Survey, Wisbech, May 2.

The Geologists' Association at Watford

IN the notice in your last number of the excursion of the Geologists' Association to Watford there is a slight error which perhaps I may be permitted to correct.

The sections of the Woolwich and Reading Series which were examined are at Watford Heath Kiln and at Bushey Kiln (*not* Bushey Heath), and the formation, although varying very much in the short distance (scarcely a mile) that these places are apart, is at each place represented by beds of clay, sand, and pebbles. The pebble beds are, however, better seen at the latter than at the former place.

JOHN HOPKINSON

Temperature of Hill and Valley

IN NATURE of May 2, page 19, there is an interesting report of a paper by Mr. Dines "On the Temperature of Hill and Valley," showing that (to use Mr. Gaster's words in the discussion) "the air is colder on the top of the hill than in the valley, by day, when the sun is shining, and warmer at night when it is not shining." This appears anomalous at first sight, but I believe it has long been known, and it is easily explained. The temperature of the hill top is not produced where it is observed. As Mr. Glaisher stated in the same discussion, the higher we go in a balloon the less is the range of temperature; and the temperature at the top of a hill which is at all isolated, or even steep on one side, approximates to that observed in a balloon at the same absolute height.

The reason why temperature has a less range in the higher atmospheric strata, is that the earth has very much more power of both absorbing and radiating heat than the molecules of air, and consequently heats more rapidly in the daytime and cools more rapidly at night. The temperature of air at the surface of the earth is determined much more by the absorption and radiation of the earth than by its own.

The truth of this view is shown by the fact that the law is quite different for table-lands from that which we have seen to be true for isolated hills. On table-lands the diurnal range of temperature is greater than on low plains, though on isolated hills it is less. These facts appear to show that while, other conditions being given, the mean temperature of a station chiefly depends on its height above the sea level (or, in other words, the thickness of the atmosphere above it) the diurnal range at the station (whether hill top or balloon) chiefly depends on the proximity of the atmospheric stratum surrounding it to the surface of the earth.

The fact stated in the same paper that "in cold weather the air on the top of the hill is never so cold as that in the valley," is no doubt partly due to the cause above stated, but partly also, I think, to the fact that cold air is heavier than warm, and seeks the lowest position. I believe there is no doubt this is the explanation of the *glacières* or freezing caverns of the Jura described in the Rev. Harold Browne's book.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, May 8

Meteors

I OBSERVED a meteor at about half-past eleven on the night of the 8th inst., in the constellation Scorpio, which passed very close to the star Antares, travelling from right to left.

It appears to me worth remarking from the fact of its course lying very near, and roughly parallel to, that part of the ecliptic which corresponded to the earth's position in her orbit.

It traversed some eight or ten degrees of arc, and was visible for three or four seconds, gradually increasing in brightness till it was nearly on a par with Antares, which star it also resembled in colour. Its slow apparent motion immediately suggested the idea that it was moving in the same plane and direction as the earth; in fact, that it was overtaking us in an orbit just outside our own.

The course of another meteor, seen about half an hour earlier from a westerly window, and described to me as not very inferior to Jupiter, appears also to have lain in the direction of the ecliptic, but from left to right, in the neighbourhood of the constellations Gemini, Cancer, or Leo.

It is rash to generalise from such insufficient data, but I conceive these meteors may both have belonged to a system whose orbit lies nearly in the plane of the earth's; the apparent retrograde motion of the last-named being caused by the direction of its path crossing our orbit at a point behind the earth's then place instead of in advance of it.

Cardiff, May 10

GEO. C. THOMPSON

The Ivory Crayfish

I THINK it may be interesting to some of the readers of NATURE to know that a specimen of the "Ivory crayfish," or *Astacus* (*Cambarus*) *pellucidus*, mentioned in the very interesting communication of Dr. Packard on the Mammoth Cave of Kentucky in NATURE, April 18, has been living in the aquarium-house in the Regent's Park for a considerable time. The specimen was brought over at the close of the last year by Mr. Boyd from the Mammoth Cave; it is now placed in the first fresh-water tank on the right hand of the visitor as he enters the aquarium. Its dark-loving habits do not permit it to be seen much, as it generally burrows partially under a large stone, from beneath which only the tips of its white claws can be seen.

Culverlea, Winchester, May 8

W. A. FORBES

THE AMERICAN ACADEMY OF SCIENCES

THE Annual Meeting of the National Academy of Sciences was convened, on the 16th of April last, at the Smithsonian Institution in Washington. The existence of this body was authorised by an Act of Congress passed in 1863; and it was originally limited to fifty members, designed to represent the most eminent men of science in the country, who were to be organised for the purpose of serving as advisers to the United States Government in questions of a scientific nature. The Academy has rendered excellent service in this capacity, and has had referred to it very many important questions, the satisfactory solutions of which have saved much money to the Government, and increased the efficiency of many of its bureaus. One condition of membership is that all such service to the Government is to be performed without charge.

As this Society, by its national character, takes the lead of State Institutions for a similar object, and the number of members was at first limited, considerable dissatisfaction was felt by many persons who believed themselves worthy of membership, but were excluded by this restriction. The Academy, therefore, after mature deliberation, decided to ask Congress to remove the limitation as to number, which being done, the principal business of the Academy at its late meeting consisted in the increase of its force. Twenty-five new members were selected, representing the various branches of theoretical and applied science, and the number is to be increased by five each succeeding year. Very few papers or communications were presented to the Academy, as all the time of the meeting was required in carrying out the changes involved by the alteration of the charter, including the formation of a new constitution and rules for its government.

The following is the list of members elected:—Prof. C. A. Young, Dartmouth; Prof. E. D. Cope, Philadelphia; Prof. J. Lawrence Smith, Louisville; W. S. Sullivant, Columbus; Prof. W. B. Rogers, Boston; J. H. Trumbull, Hartford; Prof. J. P. Cooke, Cambridge; Dr. A. S. Packard, jun., Salem; Prof. W. P. Trowbridge, New Haven; J. E. Oliver, Massachusetts; Prof. E. W. Hilgard, Oxford, Maine; Prof. R. Pumpelly, State Geologist, Missouri; Prof. J. H. Lane, Coast Survey; Prof. A. E. Verrill, New Haven; Dr. J. W. Crafts; Theodore Lyman, Boston; Prof. A. M. Mayer, Stevens Institute, Hoboken; Prof. H. J. Clarke, Amherst; J. Ericsson; Prof Hadley, New Haven; Dr. F. A. Genth, Philadelphia; Charles A. Schott, Coast Survey; Prof. A. H. Worthen, State Geologist, Illinois; Captain J. B. Eads, St. Louis; General H. L. Abbott, U.S.A.

NATURAL SCIENCE SCHOLARSHIPS AT OXFORD

FOR WHICH ELECTIONS WILL BE MADE DURING THE YEAR 1872

BALLIOL COLLEGE.—One Scholarship, value 70*l.* per annum, and tenable for three years, is generally given. There are two such Natural Science Scholarships on the foundation of Miss Brakenbury, and a third is usually added by the College. Papers are set in Physics, Chemistry, and Biology; the Examination in Chemistry and Biology will be partly practical if necessary. Candidates, if Members of the University, must not have exceeded eight Terms from their Matriculation. The Examination takes place in November.

CHRIST CRURCH.—Not less than one Studentship, of the annual value of 75*l.*, together with rooms rent free, and tenable for five years from the day of Election. The subjects of Examination are Physics, Chemistry, and Biology; but no Candidate is expected to offer himself for examination in more than two of the three. Candidates must not have exceeded the age of twenty on the 1st of January preceding the election. The Examination is held in the middle of February.

JESUS COLLEGE.—One open Scholarship, value 80*l.* per annum, is occasionally given. It is tenable to the close of the twentieth Term from the Scholar's Matriculation. Papers are set in Chemistry, Physics, and Biology; but answers are not expected in more than two subjects. Candidates will have to satisfy the Electors of their ability to pass the ordinary Classical Examinations required by the University. Candidates must not on the day of Election be full twenty-four years old. The Examination takes place in October, and full notice is given early in June. Two Scholarships have been given.

MAGDALEN COLLEGE.—One or more Demyships, value 95*l.* per annum, inclusive of all allowances, and tenable for five years, provided that the holder does not accept any appointment which will, in the judgment of the Electors, interfere with the completion of his University Studies. No person will be eligible who shall have attained the age of twenty years. In conducting the Examination, Questions will be put relating to General Physics, to Chemistry, and to Biology, including Human and Comparative Anatomy and Physiology, with the principles of the classification and distribution of Plants and Animals; but a clear and exact knowledge of the principles of any one of the above-mentioned Sciences will be preferred to a more general and less accurate acquaintance with more than one. The Examination in Biology and Chemistry will be partly practical, if necessary. Candidates have also to satisfy the Electors of their ability to pass the ordinary Classical Examinations required by the University, and for this purpose will have:—*a.* To translate a passage of English Prose into Latin. *b.* To bring up for Examination one Greek author, or a portion, such as five books of Homer, or two Greek Plays, or any equivalent; one Latin author, or a portion, such as the Georgics, or five books of the Æneid of Virgil, or three books of the Odes and the De Arte Poetica of Horace, or any other equivalent. *c.* To answer Questions in Greek and Latin Grammar. Very superior excellence, however, in Natural Science will be allowed to compensate for any deficiency which Candates may have shown in the Classical part of the Examination. Candidates will be required to bring with them a Certificate of Birth, with testimonials of good conduct and character, extending over a period of at least three years, from the Head Master of their School, or from the Private Tutor with whom they may have been reading. The Demyships are open without any restriction as to place of Birth or Education to all Candidates, whether already Members of the University or not, who are found to satisfy the above-named conditions. The Examination will be held in common with Merton College, at the same time and with the same Papers. Each Candidate will be considered as standing, in the first instance, at the College at which he has put down his name, and, unless he has then given notice to the contrary, will be regarded as standing at the other College also. The Examination usually commences on the first Tuesday in October. No Entrance Fees or Caution Money are required by the College. The University Fees payable on Matriculation amount to 2*l.* 10*s.*

MERTON COLLEGE—One Postmastership, value 80*l.* per annum, tenable for five years, or so long as the holder does not accept any appointment incompatible with the full pursuance of the University studies. Papers will be set in Chemistry, Physics, and Biology; and an opportunity will be given of showing a knowledge of practical work in Chemistry and Biology. The Postmastership will be given for special excellence in one subject, or for excellence in two of the three subjects; but no Candidate will be examined in more than two subjects. There is no limit of age for the Candidates, but a limit of six Terms of University standing. The Examination will be held in common with Magdalen College at the same time, and with the same Papers. Each Candidate will be considered as standing, in the first instance, at the College at which he has put down his name, and, unless he has given notice to the contrary, will be regarded as standing at the other College also.

NEW COLLEGE.—Candidates for Exhibitions may offer to be examined in Natural Science, in addition to the Classical Examination, or in lieu thereof. There is no restriction of age, but no Candidate must have already entered on residence in another College or Hall. The Examination usually takes place in March.

UNIVERSITY SCHOLARSHIP.—*Burdett-Coutts Scholarship.*—One Scholar is elected every year in Lent Term. Candidates must have passed all the Examinations necessary for the Degree of B.A., and not have exceeded the twenty-seventh Term from their Matriculation. The Examination is in Geology, and in Chemistry and Biology as bearing on Geology.—*Radcliffe's Travelling Fellowship.*—One Fellowship, value 200*l.* per annum, and tenable for three years, is filled up each year in Lent Term. For conditions of Examination and Election, see "The Oxford University Calendar."

A LECTURE ON THOMSON'S GALVANOMETER

DELIVERED TO A SINGLE PUPIL IN AN ALCOVE WITH DRAWN CURTAINS

The lamp-light falls on blackened walls,
And streams through narrow perforations;
The long beam trails o'er pasteboard scales,
With slow-decaying oscillations.
Flow, current! flow! set the quick light-spot flying!
Flow, current! answer, light-spot! flashing, quivering, dying.

O look! how queer! how thin and clear,
And thinner, clearer, sharper growing,
This gliding fire, with central wire
The fine degrees distinctly showing.
Swing, magnet! swing! advancing and receding;
Swing, magnet! answer, dearest, what's your final reading?

O love! you fail to read the scale
Correct to tenths of a division;
To mirror heaven those eyes were given,
And not for methods of precision.
Break, contact! break! set the free light-spot flying!
Break, contact! rest thee, magnet! swinging, creeping, dying.

$\frac{dp}{dt}$

ON MEASURING TEMPERATURES BY ELECTRICITY*

THE truth revealed to us by one of the younger branches of physical science, which has been cultivated and expounded nowhere more effectually than within these walls, has divested heat and electricity of their mysterious character, and has taught us to regard them simply as "modes of motion."

Light also has been shown to be identical in its nature with heat, and the only remaining physical agency, "chemical affinity," has been recognised as a force differing only in "quality of action" from the others. According to these views, force, in whichever type of action it presents itself, is as indestructible as matter itself, and is therefore capable of being stored up and measured with the same certainty of result. We have a unit of force, or the foot lb., and a unit of heat, or the heat necessary to raise the temperature of 1 lb. of water through 1° Fah., and it has been already proved that 772 units of force are the equivalent value of one unit of heat. Again, the chemical force residing in 1 lb. of pure coal is equal to about 14,000 heat units, or 14,000 × 772 = 10,808,000 ft. lbs. = 4,825 tons lifted one foot high.

Questions regarding the quantitative effects of heat present themselves, however, much less frequently for our consideration than questions regarding its intensity, upon which depends the nature of the phenomena surrounding us at every step, both in science and in ordinary life. The instrument at our command for determining moderate intensities or temperatures, the mercury thermometer, leaves little to be desired for ordinary use; but when we ascend in the scale of intensity, we soon approach a point when mercury boils, and from that point upward we are left without a reliable guide. The result is that we find in scientific books on chemical processes statements to the effect that such or such reaction takes place at a dull red heat, such another at a bright red, a cherry red, a blood red, or a white heat—expressions which remind one rather of the days of alchemy than of chemical science of the present day.

There are pyrometers, it is true, but these are either of a complex nature, or little reliance can be placed on them.

It is my purpose this evening to place before you an instrument by which I hope to fill up to some extent the existing gap. It is the result of occasional experimental research, spread over several years, and it aims at the accomplishment of a double purpose, that of measuring high temperatures, and of measuring with accuracy the temperatures of inaccessible or distant places.

But before entering upon the details of my subject, I propose to place before you an instrument which fulfils, in principle, all the conditions essentially necessary in thermometry, and is at the same time the very first instrument that was ever proposed for measuring temperatures. I speak of the air thermometer by Galileo. It can be shown on theoretical grounds that the expansion of a permanent gas at constant pressure is the most perfect index of temperature. It is, in fact, the degree of energy of the atomical motion in an elastic fluid which determines its volume, and which constitutes at the same time its temperature.

The air thermometer consists simply of a bulb of glass with a long tubular stem, open to the atmosphere at its extremity. If I heat the bulb (by dipping it, for instance, into boiling water) and put it into a holder, with the hollow stem reaching downward into a cup of mercury, the air within the bulb will no longer communicate directly with the atmosphere, because the mercury is interposed. If now I cool the air within the bulb, by the external application of iced water, its heat motion will diminish, and its volume would be reduced proportionally, if the external atmosphere could enter freely to fill up the vacancy thus created. But inasmuch as the external air cannot enter, a reduction of pressure will take place, which, according to the law of elasticity by Boyle, must be proportionate to the reduction of volume at constant pressure. The difference of pressure thus created between the bulb and the external atmosphere will be balanced by the column of mercury rising up into the tube, and the elevation to which the mercury attains is a true index of the temperature to which the air in the bulb had been previously heated. This is true with regard to all temperatures, from the lowest to the highest, and the instrument may be termed a universal thermometer. If the bulb could be cooled down to 273° Centigrade below the zero point, it would follow by the law of Charles that the elastic pressure of air would be reduced to nothing, that is to say, the motion of the particles of air, which we call heat, would have ceased, and we should have reached the point of an absolute zero, a point which has been theoretically established also by other means.

Practically, such an instrument would be most inconvenient; its indications would have to be corrected by calculation for barometrical variations; the capacity of the descending tube, which contains air not subjected to variation of temperature, would have to be taken into account, and no reliable observations could be arrived at, without taking special precautions, such as are only within reach of the experimental physicist.

[The other known methods of measuring ordinary and furnace temperatures were here passed in review, and the limits of their application pointed out. They were classified into—

- Thermometers, by expansion of liquids.
- Thermometers, by the expansion of solids.
- Pyrometers, by chemical decomposition of solids, comprising Wedgwood's and Deville's pyrometers.
- Pyrometers, by observing the melting-point of metals.
- Pyrometers, by thermo-electricity.
- Pyrometers, by exposing a copper or platinum ball of known heat capacity to the heat to be ascertained, and of quenching it in a measured quantity of water.]

The instrument which forms the subject matter of my discourse presents many points of analogy with the air thermometer, if we substitute "electrical resistance in conductors" for "expansion of gases." Both these effects are functions of temperature, increasing with the temperature according to progressive laws, which in the case of the gases we call the law of Charles, and in the case of conductors, the law of "increase of electrical resistance with temperature." The latter law, which is of recent origin, had already been partially developed by Arndsen, Swanberg, Lenz, and Werner Siemens, when my attention was directed in 1860 towards an application of the same to the measurement of temperatures at places inaccessible to the ordinary thermometer. By means of the contrivance which I shall describe presently, I was enabled to tell, in the testing cabin of a cable ship, the increasing temperature of the interior of the mass cable in the hold, and to prove the necessity of trans-shipment of the same into a vessel fitted with water-tight tanks, which have been resorted to ever since, to avoid the danger of softening the gutta-percha covering.

I have arranged an apparatus for proving to you in the first instance that the conductivity of a wire of platinum or other metal is greatly influenced by its temperature; for this purpose I direct the current of a galvanic battery at will through two branches of equal resistance, each branch comprising a free spiral wire of platinum and one of the coils of a differential galvanometer. By throwing the powerful light of an electric lamp upon the face of the differential galvanometer, and by throwing the image by means of a mirror and lens upon the screen, the audience will see any movement of the needle to the right or the left that may take place when I complete the battery connection. The resistance of both branch circuits being the same, no deflection of the needle is observable on depressing the key, but when I pass the flame of a spirit-lamp under the one platinum coil, the needle is thrown immediately over to the right, because the electrical resistance of the heated wire is increased, and consequently a larger proportion of the current is passing through the cooler circuit, exercising a preponderating influence upon the galvanic needle. When I withdraw the spirit flame from the wire the needle rapidly returns to its zero position, but in passing it under the other spiral wire the needle immediately deflects in the opposite direction.

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* Lecture delivered at the Royal Institution of Great Britain, March 1st, 1872, by C. William Siemens, F.R.S.

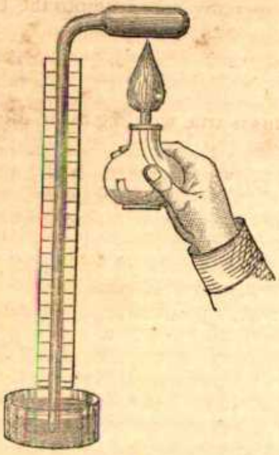

FIG. 1

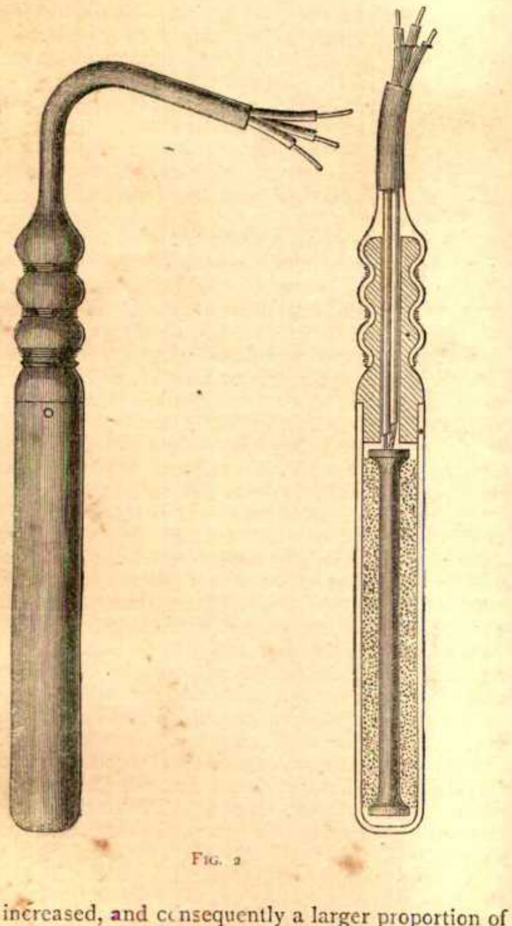

FIG. 2

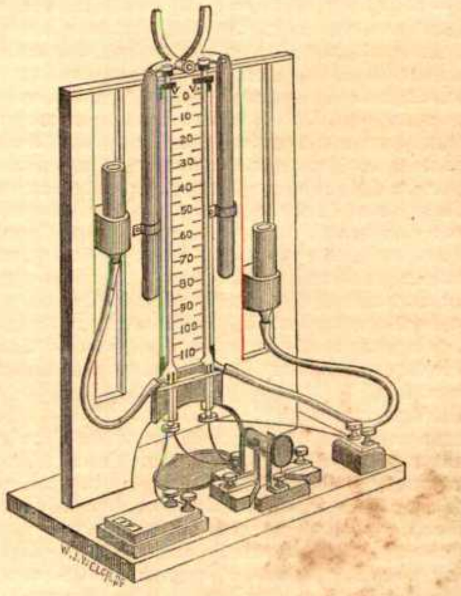

FIG. 3

If instead of using the open spirals I were to wind thin insulated wire of any pure metal upon two small cylindrical pieces of wood, and were to enclose the tiny spirals in small silver casings, as shown in view and in section by Fig. 2, taking care that the extremities of the spiral wires were soldered to thicker insulated wires leading respectively to the battery and differential galvanometer before mentioned, it follows that no deflection of the needle ensues when both the protected and equal spirals are dropped into a jar containing iced water. But if I take one of the spirals from the water, and place it, for instance, by his kind permission, into the hands of our President, without disconnecting the same from its leading wires, the balance of resistance will no longer take place, and a deflection of the needle to the right actually takes place. I will now endeavour, however, to re-establish the equilibrium by adding warm water to the iced water surrounding the comparison coil near me until no deflection of the needle is observable. This result being obtained, it follows that the temperature of the water surrounding the one coil must be identical with the temperature of our President's hand, and the delicate mercury thermometer which I have placed in my solution must give me the temperature of the distant place which I intended to measure. The temperature here observed is 89·5° Fahrenheit, which is at this moment that of Sir Henry Holland's hands. This result is independent of the ratio in which the electrical resistance increases with temperature in the similar coils, and considering that the silver casings containing the coils are not larger than small pencil-cases, this method might be advantageously employed in physiological research. The one coil would only have to be placed within the cavity to be measured, to enable the observer to read the temperature from time to time, without disturbing the patient, with the accuracy of which the mercury or spirit of wine thermometer employed is capable. But the same method is applicable for measuring the temperatures of distant or inaccessible places, such as the interior of stores or cargoes of materials liable to spontaneous combustion, of points elevated above the surface of the ground, or of great depths below for meteorological purposes, or for measuring the temperature of the sea continuously in attaching such a coil to the mariner's sounding lead. An error would in such cases arise, however, through the uncertainty of the resistance of long leading wires, if a complete remedy of error from such a source had not suggested itself. This consists in uniting three separate insulated leading wires into a cable by which the distant coil is connected with the measuring instrument. One galvanic circuit passes from the battery through one of the leading wires, through the distant spiral, and back again through the second leading wire to the differential galvanometer and the battery, and the second passes from the same battery through the near coil, and through the third leading wire up to the distant coil without traversing the same, and back again through the second leading wire to the galvanometer and battery. Thus both galvanic circuits comprise the leading wires up to the distant coil, and all variations of resistance by temperature to which the leading wires may be subjected affect both sides of the balance equally. In constructing coils for measuring deep-sea temperatures a large quantity of insulated copper or iron wire is wound upon a metallic tube open at both ends to admit the sea-water freely in order to impart its temperature to the innermost layers of the insulated wire. The coil of wire is protected externally by drawing a tube of vulcanised india-rubber over it, which in its turn is bound round by a close spiral layer of copper wire, whereby the sea-water is effectually excluded from the sensitive coil. By these arrangements the temperature of distant or otherwise inaccessible places can be accurately ascertained; but the method is limited to the range of temperature which can be obtained and measured in the comparison bath. In order to realise a pyrometer by electrical resistance, it is necessary to rely upon the absolute measurement of the electrical resistance of a coil of wire which must be made to resist intense heats without deteriorating through fusion or oxidation. Platinum is the only suitable metal for such an application, but even platinum wire deteriorates if exposed to the direct action of the flame of a furnace, and requires an external protection. The platinum wire used has, moreover, to be insulated and supported by a material which is not fused or rendered conductive at intense heats, and the disturbing influence of leading wires had in this case also to be neutralised. These various conditions are very fully realised by the arrangement represented on the preceding diagram, Fig. 4.

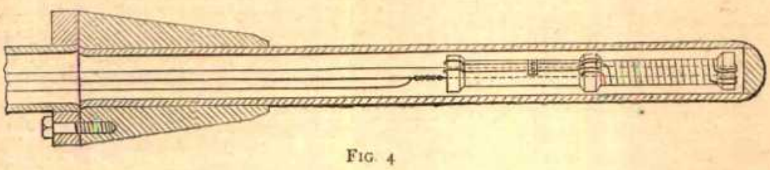

FIG. 4

Thin platinum wire is coiled upon a cylinder of hard-baked porcelain, upon the surface of which a double-threaded helical groove is formed for its reception, so as to prevent contact between the coils of wire. The porcelain cylinder is pierced twice longitudinally for the passage of two thick platinum leading wires, which are connected to the thin spiral wire at the end. In the upper portion of the porcelain cylinder the two spiral wires are formed into a longitudinal loop, and are connected crossways by means of a platinum binding screw, which admits of being moved up or down for the purpose of adjustment of the electrical resistance at the zero of Centigrade scale. The porcelain cylinder is provided with projecting rims, which separate the spiral wire from the surrounding protecting tube of platinum, which is joined to a longer tube of wrought iron, serving the purpose of a handle for moving the instrument. If the temperatures to be measured do not exceed a moderate white heat, or say 1,300° Centigrade = 2,372° Fah., it suffices to make the lower protecting tube also of wrought iron, to save expense. This lower portion only, up to the conical enlargement or boss of iron, is exposed to the heat to be measured. Three leading wires of insulated copper united into a light cable connect the pyrometer with the measuring instrument, which may be at a distance of some hundred yards from the same. They are connected by means of binding screws at the end of the tube to three thick platinum wires passing down the tube to the spiral of thin platinum wire. Here two of the leading wires are united, whereas the third traverses the spiral, and joins itself likewise to one of the two former, which forms the return wire for two electrical circuits, the

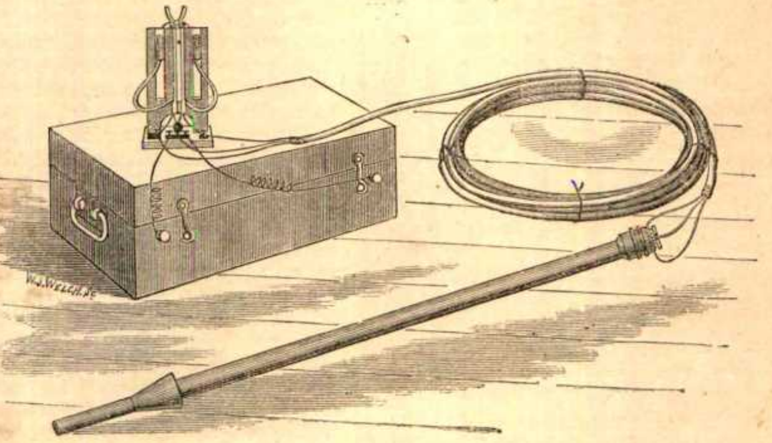

FIG. 5

one comprising the spiral of thin wire, and the other returning immediately in front of the same, but traversing in its stead a comparison coil of constant resistance. The measuring instrument may consist of a differential galvanometer as before, if to the constant resistance a variable resistance is added. If the pyrometer coil were to be put into a vessel containing snow and water, the balance of resistance between the two battery circuits would be obtained without adding variable resistance to the coil of constant resistance, and the needle of the differential galvanometer would remain at zero when the current is established. But on exposing the pyrometer to an elevated temperature, the resistance of its platinum coil would be increased, and resistance to the same amount would have to be added to the constant resistance of the measuring instrument, in order to re-establish the electrical balance. This additional resistance would be the measure of the increase of temperature, if only the ratio in which platinum wire increases in electrical resistance with temperature is once for all established. This is a question which I shall revert to after having completed the description of the pyrometric instrument.

Although I have explained that by means of a differential galvanometer and a variable resistance (constituting in effect a Wheatstone bridge arrangement) the increasing resistance of the platinum spiral may be measured, it was found that the use of a delicate galvanometer is attended with considerable practical difficulty in iron-works and other rough places where it is important to measure elevated temperatures, or on board ship for measuring deep-sea temperatures. I was therefore induced to seek the same result by the conception of an instrument which is independent in its action from tremulous motion, or from magnetic disturbance caused by moving masses of iron, and which requires no careful adjustment or special skill on the part of the operator. This instrument is represented by Fig. 3 on page 48, and may be termed a chemical resistance measurer or "differential voltameter." The immortal Faraday has proved that the decomposition of water in a voltameter expressed by the volumes of gases V, is proportionate in the unit of time to the intensity I of the decomposing current, or that

$$I = \frac{V}{T}.$$

According to Ohm's general law, the intensity I is governed by the electro-motive force E, and inversely by the resistance R, or it is

$$I = \frac{E}{R}.$$

It is therefore

$$\frac{V}{T} = \frac{E}{R} \quad \text{or } V = \frac{ET}{R}\ ;$$

or the volume V would give a correct measure of the electrical resistance R, if only the electro-motive force E and time T were known and constant quantities. But the electromotive force of a battery is very variable ; it is influenced by polarisation of the electrodes, by temperature, and by the strength and purity of the acid employed. The volume of gases obtained is influenced, moreover, by the atmospheric pressure, and it is extremely difficult to make time observations correctly. It occurred to me, however, that these uncertain elements might be entirely eliminated in combining two similar voltameters in such a manner that the current of the same battery was divided between the two, the one branch comprising the unknown resistance to be measured, and the other a known and constant resistance. The volume of gas V^1 produced in this second voltameter, having a resistance R^1 in circuit, would be expressed by

$$V^1 = \frac{E\,T}{R^1},$$

and we should have the proportion of

$$V : V^1 = \frac{E\,T}{R} : \frac{E\,T}{R^1};$$

or E and T, being the same in both cases, may be struck out, and the expression will assume the simple form

$$V : V^1 = R^1 : R.$$

The constant resistance R of the one circuit being known, it follows that the unknown resistance R^1 is expressed by $\frac{R^{v}}{V^1}$; that is to say, by a constant multiplied by the proportion of gas produced in the two voltameters irrespective of time, or strength of battery, or temperature, or the state of the barometer.

The resistance R and R^1 are composed each of two resistances, namely, that of the principal coils, which we may term R or R^1, and of the voltameter and leading wires, which is the same in both cases, and may be expressed by y. The expression should therefore be written as follows :—

$$V : V^1 = R^1 + y^1 : R + y,$$

R^1 being the unknown quantity.

The mechanical arrangement of the instrument will be understood from the diagram, Fig. 3 ; and the whole arrangement of the pyrometer, with its leading wire and resistance measurer, from the general view given in Fig. 5. The voltametric resistance measurer consists of two calibrated vertical tubes of glass of about 3 millimetres diameter, which are fixed upon a scale showing arbitrary but equal divisions. The upper ends of the tubes are closed by small cushions of india-rubber pressed down upon the openings by means of weighted levers, whereas the lower portions of the tubes are widened out and closed by plugs of wood, through which the electrodes in the form of pointed platinum wires penetrate to the depth of about 25 millimetres into the widened portions of the tubes. By a side branch the widened portion of each vertical tube communicates by means of an india-rubber connecting pipe to a little glass reservoir containing acidulated water, and supported in a vertical slide. In raising the weighted cushions closing the upper ends of the vertical tubes, and in adjusting the position of the small reservoirs, the acidulated water will rise in both tubes to the zero line of the scale. In turning a button in front of the tubes the battery current is passed through both pairs of electrodes, the one circuit comprising the permanent resistance R and the leading wires up to the pyrometer, and the other the leading wires and the pyrometer coil. If the resistance of the pyrometer coil should be equal to the permanent resistance R, then $R^1 + y$ will be equal to $R + y$, and therefore $V = V^1$, but as the resistances differ, so will the volumes. Necessary conditions are, that both reservoirs are filled with the same standard solution of pure water with about 10 per cent. of sulphuric acid, that all the electrodes are of the same form and size, and that their polarity is reversed frequently during the progress of each observation, in order to avoid unequal polarisation. With these precautions, which involve no particular skill or knowledge of electrical observation on the part of the operator, very accurate results are obtained ; but in order not to incur considerable error of observation, it is advisable not to continue the current, reversing the same, say twice, until at least forty divisions of gases are produced in the least activated tube, which operation will occupy from two to three minutes, if a battery of from four to six Daniel elements is employed. The volumes V and V^1 being noted, after having allowed half a minute for the gases to collect after the current has ceased, the weighted cushions upon the tubes are raised in order to allow the gases to escape, when the water levels will immediately return to their zero position, to make ready for another observation. By inserting the observed values for V and V^1 into the expression above given, the unknown resistance R^1 can be easily calculated; but in order to facilitate the use of the instrument, I have prepared a table which gives at a glance the resistance due to any two observed volumes, the volumes V governing the vertical, V^1 the horizontal columns, and the resistance being read off at the point of intersection. At each point of intersection the resistance is marked in black, and the corresponding temperature in red ink.

It now remains only to be shown what is the relation between the resistance and temperature in heating a platinum wire. The researches of Dr. Matthiesen, who has made the latest investigations on the effect of temperature upon electrical resistance, are restricted to the narrow range of temperatures between 0° and 100° Centigrade, nor do they comprise platinum. He adopted the following general expression for the pure metals:—

$$R_t = \frac{R_0}{1 + xt + yt^2}$$

which, in determining the specific values of x and y for each metal, gives a close agreement with observation between the narrow limits indicated, but is wholly inapplicable for temperatures exceeding 200° Centigrade, when the value t^2 commences to predominate and to produce absurd values for R_t.

It was necessary for my purpose to undertake a series of elaborate experiments with a view of finding a ratio of general application. Coils of thin wire, of platinum, iron, copper, and some other metals, were gradually heated and cooled in metallic chambers containing the bulbs of mercury thermometers, and for higher temperatures of air thermometers, and the electrical resistances were carefully noted. The progressive increase of electrical resistance was thus compared directly with the increasing volume of a permanent gas (carefully dried) between the limits of zero and 470° Centigrade, and a ratio established which is represented by the formula—

$$R_t = \alpha T^{\frac{1}{2}} + \beta T + \gamma,$$

in which T signifies total temperature counting from the absolute zero, and α, β, and γ specific coefficients for each metal. According to this formula the electrical resistance is a constant at the absolute zero, and progresses in a ratio represented graphically by a tipped-up parabola, approaching more and more toward a uniform ratio at elevated temperatures. Although the comparison with the air thermometer could only be carried up to 470° C., the general correctness of the ratio of increase just stated has been verified by indirect means in measuring progressive heats, and by comparison with the platinum ball pyrometer.

It is important to mention here that great care must be exercised in the selection of the platinum wire for the measuring spiral, platinum wire having been met with conducting only 4·7 times better than mercury at zero C., and others conducting 8·2 times better than mercury, although both samples had been supplied by the same eminent makers, Messrs. Johnson and Mathey. The abnormal electrical resistance of some platinum wire is due chiefly to the admixture of iridium or other metals of the same group, and it appears that the platinum prepared by the old welding process is purer and therefore better suited for electrical purposes than the metal consolidated by fusion in a Deville furnace.

In conclusion, I shall show some working results of the pyrometer in measuring by means of the same protected coil a mixture of ice and water, boiling water, molten lead, and the fire itself by which the lead is melted, the readings produced being 2° C., 98° C., 330° C., and 860° C. respectively. The latter temperature signified a cherry-red heat, as may be judged by the appearance of the tube when withdrawn from the fire. The instrument which I have had the honour to bring before you this evening has already received several useful applications. Through its first application an important telegraph cable was saved from destruction through spontaneous generation of heat. Prof Bolzani, of Kasan, has made some interesting applications of it for recording the temperature at elevated points and at points below the earth's surface. Mr. Lowthian Bell has used it in his well-known researches on blast-furnace economy; and at several iron-works pyrometer tubes are introduced into the heating stoves, and permanently connected with the office, where the heat of each stove can at all times be read off and recorded. These and other applications are sufficiently self-evident, if the soundness of the principles upon which I rely is conceded; but I feel that the shortness of time at my command has hardly enabled me to do more than to pass these in review, while endeavouring to demonstrate the results obtained of recording the temperatures of distant or inaccessible places, including furnace temperatures.

NOTES

The "Faraday lecture" of the Chemical Society will take place on Thursday, May 30, at 8 o'clock, in the theatre of the Royal Institution, Albemarle Street. This lectureship, founded in honour of the late Prof Faraday, was inaugurated two years since by a masterly address given by M. Dumas of Paris. Prof. Cannizzaro of Palermo has consented to deliver the lecture this year, which will be anticipated with much interest; he has chosen as his subject "Considérations sur quelques points de l'Enseignement théorique de la chimie."

At the meeting of the Royal Geographical Society held on Monday evening last, Sir Henry Rawlinson said that the opinion of the Council of the Society was favourable to the authenticity of the intelligence received by telegram respecting Dr. Livingstone. They had every reason to expect that Dr. Livingstone and Mr. Stanley would meet about the beginning of the year. But if there had been any discovery and relief, it was Dr. Livingstone that had discovered and relieved Mr. Stanley, and not Mr. Stanley who had discovered and relieved Dr. Livingstone; because Dr. Livingstone was in clover and Mr. Stanley was absolutely destitute. They knew by the last account that Mr. Stanley was without supplies, and he must have undergone much difficulty in getting to Ujiji; whereas this place was the head-quarters of Dr. Livingstone's supplies. He expected that they would have full letters in the course of a fortnight from Zanzibar, which would inform them on what was known about Dr Livingstone and Mr. Stanley, and in the meantime he could only say that the telegram was credible.

We exceedingly regret to have to announce the death, after a short illness (but too probably the effect of overwork), of Mr. George Robert Gray, F.R.S., Senior Assistant Keeper of the Zoological Collections in the British Museum, and an ornithologist of world-wide reputation through his numerous works. Of these we need only particularise two—but two such as have never been executed by any other man—the "Genera of Birds," in three folio volumes (1844-1849), illustrated by the late Mr. Mitchell, some time secretary of the Zoological Society, and by Mr. Wolf, showing an amount of labour at that time unparalleled; and the "Hand-list of Birds," only completed last year, and compiled with a like amount of assiduity. Both these are works found to be indispensable by advanced students of Ornithology in every country; a fact which is alone sufficient proof of their value. The magnificent bird gallery in the British Museum, probably the most popular portion of that building, owes its chief importance to Mr. Gray's labours; and we hope that, in selecting a successor to fill his post, the trustees will not hold themselves bound by any rule of routine, but will take care that the officer in whose charge the splendid collection will be placed is one whose appointment will command the confidence of ornithologists, not in this country only, but throughout the world.

JUST before going to press we have received the new scheme for the Natural Sciences, prepared by the Board of Studies at Oxford. We can only at present note the publication of this important document, of which we will give the main features next week.

AT the meeting of the French Academy on May 6, MM. Mascart and Janssen were selected to be recommended to the Minister of Public Instruction as candidates for the Chair of General and Experimental Physics at the College of France, vacant by the resignation of M. Regnault.

A MEETING of gentlemen who have within the last fifty years attended the Natural Philosophy class in the University of Edinburgh was recently held, to consider the best means of recognising the long services of Mr. James Lindsay, Experimental Assistant to the Professor of Natural Philosophy. Mr. Lindsay has been connected with the University for the last 58 years, and has acted as Experimental Assistant to the Professor of Natural Philosophy for 53 sessions, serving successively Prof. Sir John Leslie, Principal James David Forbes, and Prof. P. G. Tait. Mr. Lindsay has assisted these gentlemen in their original investigations, and has thus been intimately associated with the progress of physical science in Scotland within the last half-century. All have borne unqualified testimony to his accuracy as an experimentalist. Mr. J. C. Young, of 4, Brighton Place, Portobello, and Mr. James Dewar, of 15, Gilmour Place, Edinburgh, are the secretaries to the committee, and will gladly receive subscriptions in aid of the fund, which, we doubt not, will speedily be collected for so worthy an object.

AT the meeting of the Zoological Society on June 4, Prof. Owen will read a paper upon a new large wingless bird recently discovered in the post-tertiary deposits of Queensland, Australia. Prof. Owen refers these remains to a new genus of *Struthiones*, allied to the Emeu (*Dromæus*), which he proposes to call *Dromornis*.

THE Conversazione of the Society of Arts is fixed to take place at the South Kensington Museum on Wednesday, the 19th of June.

THE President of the Institution of Civil Engineers, Mr. Hawksley, has issued cards of invitation for a conversazione on Tuesday, the 28th inst., in the Western Galleries of the International Exhibition Buildings at Kensington.

DR. HOOKER has just issued his Report of the Royal Gardens at Kew for the year 1871. The number of visitors has not been quite equal to either of the two preceding years; but the director attributes this entirely to the diminished number of those classes whose presence is in every way undesirable: the number of visitors who take an intelligent interest in the gardens and their productions being, he believes, steadily on the increase. The number of Sunday visitors is more than two-thirds of the total number on all the other days of the week; Monday, the "artisans' day," showing considerably the largest numbers of any of the week days, and Dr. Hooker speaks of the almost uniformly orderly conduct of the visitors on this day, contrasting in some instances favourably even with that displayed by some of the fashionable Saturday visitors. In the Botanic Gardens no change of importance has been introduced, except the making of a few more shrubberies, and bringing together various scattered young trees by threes or in clumps, so as to give more extent of lawn in certain parts, and broader masses of foliage in others. The works in the Pleasure Grounds and Arboretum have been almost uninterruptedly continued, and a very large space has been planted, partly with young trees brought from the plantations in the Queen's garden, &c., and partly with smaller things to act as a shelter to these. The interchange of living plants and seeds has been continually kept up with similar establishments abroad and in the colonies; and a gardener has been sent out to Jamaica to re-establish the Botanic Garden there, at the request of the Governor, Sir J. P. Grant.

THE following works, bearing more or less on Science, are announced as in the press:—By Messrs. Longmans—"An Exposition of Fallacies in the Hypothesis of Mr. Darwin," by Mr. C. R. Bree, M.D., F.Z.S., 8vo, with plates; "As Regards Protoplasm," by Mr. J. H. Stirling. New and improved edition, completed by the addition of Part II. in reference to Mr. Huxley's second issue, and a new preface in reply to Mr. Huxley in "Yeast;" and by Messrs. Strahan—"Town Geology," by the Rev. Charles Kingsley.

THE first part has just appeared of the long-expected Flora of British India by Dr. J. D. Hooker, published under the authority of the Secretary of State for India in Council. The territory included in the Flora is that comprised within the British territories in India (including the Malay Peninsula and the Andaman Isles), together with Kashmir and Western Tibet, but excluding Affghanistan and Beluchistan, the plants of which countries are included in Boissier's "Flora Orientalis," and belong to quite another botanical region, that of Western Asia. Of the 12,000 to 14,000 species of flowering plants and ferns belonging to British Indian botany, not a twelfth part has hitherto been brought together in any one general work on Indian plants; the description of the remainder being scattered through innumerable British and foreign journals, or contained in local floras or works on general botany; and a very large number being either very badly described, or not at all. The work is, therefore, one of considerable labour as well as importance, Dr. Hooker being assisted in it by various other botanists. There are a large number of new species described in this part; and the natural orders included in it are Ranunculaceæ, Dilleniaceæ, Magnoliaceæ, Anonaceæ, Menispermaceæ, Berberideæ, Nymphœaceæ, Papaveraceæ, Capparideæ, Resedaceæ, Bixineæ, Violaceæ, and Pittosporeæ, by Dr. Hooker and Dr. Thomson; Cruciferæ by Dr. Hooker and Dr. Anderson; Fumariaceæ by Dr. Hooker; and a part of Polygaleæ by Mr. A. W. Bennett.

THE Annual Report of the Maidstone and Mid-Kent Natural History and Philosophical Society for 1871 refers to the successful attempt made during the year to introduce instruction by means of science classes in connection with the society. The senior classes of all the schools in Maidstone have been enrolled in one great class for the study of Physical Geography, and a class of over 300 pupils, constant in their attendance, receives weekly lectures on this subject. The instruction is given gratuitously by members or officers of the Society, and the apparatus has also been provided or constructed by them. A class is also in operation for the study of Inorganic Chemistry. The Society is by this means performing a most useful function in spreading a love of science among our school-boys.

AN error has been pointed out to us in the article entitled "English Rainfall in 1871" which appeared in our issue of April 18. One inch of rain is computed at 22,500 gallons per acre, instead of, as it should be, $\frac{6,272,640}{277,274} = 22,623$ gallons, or more exactly 22,622·532, per acre. The error applies to columns γ and δ of the table there given, and to the first half of the second paragraph following, the error being equivalent to 123 in every 22,500 gallons. The correct estimate of the rainfall on a square mile during the year is $22,623 \times 640 \times 22{\cdot}42 =$ 324,612,902 gallons, or 14,739,053 hectolitres, nearly.

HISTORY OF THE NAMES CAMBRIAN AND SILURIAN IN GEOLOGY[*]

(*Concluded from page* 37)

THE Lingula flags and Tremadoc slates have been made the subject of careful stratigraphical and palæontological studies by the Geological Survey, the results of which are set forth by Ramsay and Salter in the third volume of the Memoirs of the Geological Survey, published in 1866, and also, more concisely, in the Anniversary Address by the former to the Geological Society in 1863. (Geol. Jour. XIX. xviii.) The Lingula flags (with the underlying Menevian, which resembles them lithologically) rest in apparent conformity upon the purple Harlech rocks both in Pembrokeshire and in Merionethshire, where the latter appear on the great Merioneth anticlinal, long since pointed out by Sedgwick. The Lingula flags (including the Menevian), have in this region, according to Ramsay, a thickness of about 6,000ft. Above these, near Tremadoc and Festiniog, lie the Tremadoc slates, which are here overlain, in apparent conformity, by the Lower Llandeilo beds. At a distance of eleven miles to the north-west, however, the Tremadoc slates disappear, and the Lingula flags are represented by only 2,000ft. of strata; while in parts of Caernarvonshire and in Anglesea the whole of the Lingula flags and moreover the Lower Cambrian rocks are wanting, and the Llandeilo beds rest directly upon the ancient crystalline schists. In Scotland and in Ireland, moreover, the Lingula flags are wholly absent, and the Llandeilo rocks there repose unconformably upon grits regarded as of Lower Cambrian age. Thus, without counting the Tremadoc slates, which are a local formation unknown out of Merionethshire, we have (including the Bangor group and Lingula flags) beneath the Llandeilo, over 9,000ft. of fossiliferous strata, which disappear entirely in the distance of a few miles. From a careful survey of all the facts, the conclusion of Ramsay is irresistible, that there exist between the Lingula flags and the Llandeilo not merely one, but two great stratigraphical breaks in the succession; the one between the Lingula flags and the Lower Tremadoc slates, and the other between the Upper Tremadoc slates and the Lower Llandeilo.

This conclusion is confirmed by the fact that there exists at each of these horizons a nearly complete palæontological break. The fauna of the Tremadoc slates is, according to Salter, almost entirely distinct from that of the Lingula flags, and not less distinct from that of the so-called Lower Llandeilo or Arenig rocks (the equivalents of the Skiddaw slates of Cumberland). Hence, says Ramsay, it is evident "that in these strata we have three perfectly distinct zones of organic remains, and therefore, in common terms, three distinct formations." The palæontological evidence is thus in complete accordance with that furnished by stratigraphy. We cannot leave this topic without citing the conclusion of Ramsay, that "each of these two breaks necessarily implies a lost epoch, stratigraphically quite unrepresented in our area; the life of which is only feebly represented in some cases by the fossils common to the underlying and overlying formation." In connection with this remark, which we conceive to embody a truth of wide application, it may be said that stratigraphical breaks and discordances in a geological series may, *a priori*, be expected to occur most frequently in regions where this series is represented by a large thickness of strata. The accumulation of such masses implies great movements of subsidence, which, in their nature, are limited, and are accompanied by elevations in adjacent areas, from which may result, over these areas, either interruptions in the process of sedimentation, or the removal, by sub-aerial or sub-marine denudation, of the sediments already formed. The conditions of succession and distribution, it may be conceived, would be very different in a region where the period corresponding to this same geological series was marked by comparatively small accumulations of sediment upon an ocean-floor subjected to no great movements.

This contrast is strikingly seen between the conformable series of less than 2,000 feet of strata which in Scandinavia are characterised by the first three palæozoic faunas (Cambrian and Silurian) and the repeatedly broken and discordant succession of more than 30,000 feet of sediments[†] which in Wales are their palæontological equivalents. It must, however, be considered that in regions of small accumulation where, as in Scandinavia, the formations are thin, there may be lost or unrepresented zoological epochs whose place in the series is marked by no stratigraphical break. In such comparatively stable regions, movements of the surface sufficient to cause the exclusion, or the disappearance by removal, of the small thickness of strata corresponding to an epoch, may take place without any conspicuous marks of stratigraphical discordance.

The attempt to establish geological divisions or horizons upon stratigraphical or palæontological breaks must always prove fallacious. From the nature of things, these, whether due to non-deposition or to subsequent removal of deposits, must be local; and we can say confidently that there exists no break in life or in sedimentation which is not somewhere filled up and represented by a continuous and conformable succession. While we may define one period as characterised by the presence of a certain fauna, which, in a succeeding epoch, is replaced by a different one, there will always be found, in some parts of their geographical distribution, a region where the two faunas commingle, and where the gradual disappearance of the old before the new may be studied. The division of our stratified rocks into systems is therefore unphilosophical, if we assign any definite or precise boundaries or limitations to these. It was long since said by Sedgwick with regard to the whole succession of life through geologic time—that all belongs to one great *systema naturæ*. (Philos. Mag. IV. viii. 359.)

We have already noticed that Barrande, as early as 1852, gave the name of Primordial Silurian to the rocks which, in Bohemia, were marked by the first fauna; although he, at the same time, recognised this as distinct from and older than the second fauna, discovered in the Llandeilo rocks, which Murchison had declared to represent the dawn of organic life. Into the reasons which led Barrande to include the rocks of the first, second, and third fauna in one Silurian system (a view which was at once adopted by the British Geological Survey and by Murchison himself), it is not our province to inquire; but we desire to call attention to the fact that the latter, by his own principles, was bound to reject such a classification. In his address before the Geological Society in 1842 (already quoted in the first part of this paper) he declared that the discussion as to the value of the term Cambrian involved the question "whether there was any type of fossils in the mass of the Cambrian rocks different from those of the Lower Silurian series. If the appeal to nature should be answered in the negative, then it was clear that the Lower Silurian type must be considered the true base of what I had named the protozoic rocks; but if characteristic new forms were discovered, then would the Cambrian rocks, whose place was so well established in the descending series, have also their own fauna, and the palæozoic base would necessarily be removed to a lower horizon."

In the event of no distinct fauna being found in the Cambrian series, it was declared that "the term Cambrian must cease to be used in zoological classification, it being in that sense synonymous with Lower Silurian." (Proc. Geol. Soc. iii. 641 *et seq.*) That such had been the result of palæontological inquiry Murchison then proceeded to show. Inasmuch as the only portion of Sedgwick's Cambrian which was then known to be fossiliferous, was really above and not below the Llandeilo rocks, which Murchison had taken for the base of his Lower Silurian, his reasoning with regard to the Cambrian nomenclature, based on a false datum, was itself fallacious; and it might have been expected that when the Government surveyors had shown his stratigraphical error, Murchison would have rendered justice to the nomenclature of Sedgwick. But when, still later, a farther "appeal to nature" led to the discovery of "characteristic new forms," and established the existence of a "type of fossils in the mass of the Cambrian rocks different from those of the Lower Silurian series," Murchison was bound by his own principles to recognise the name of Cambrian for the great Festiniog group, with its primordial fauna, even though Barrande and the Government surveyors should unite in calling it Primordial Silurian.

He however chose the opposite course, and now attempted to claim for the Silurian system the whole of the Middle Cambrian or Festiniog group of Sedgwick, including the Tremadoc slates and the Lingula flags. The grounds of this assumption, as set forth in the successive editions of "Siluria" from 1854 to 1867, and in various memoirs, may be included under three heads: first, that the Lingula flags have been found to exist in some parts of his original Silurian region; second, that no clearly-defined base had been assigned by him to his so-called system; and third, that there are no means of drawing a line of demarcation between these Middle Cambrian formations and the overlying Llandeilo.

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[*] Reprinted from advance sheets of the *Canadian Naturalist.*

[†] The Longmynd rocks in Shropshire are alone estimated at 20,000 feet; but their supposed equivalents, the Harlech rocks of Pembrokeshire, have a measured thickness of 3,300, while the Llanberis and Harlech rocks together, in North Wales, equal from 4,000 to 7,000 feet, and the Lingula flags and Tremadoc slates, united, about 7,000 feet. The Bala group in the Berwyns exceeds 12,000 feet, and the proper Silurian, from the base of the Upper Llandovery or May Hill sandstone, attains from 5,000 to 6,000 feet; so that the aggregate of 30,000 feet may be considered below the truth. (Mem. Geol. Survey, iii. part 2, pages 72, 222, and Siluria, 4th ed. 185.)

With regard to the first of these reasons, it is to be said that the only known representatives of the Lingula flags in the region described by Murchison in his "Silurian System" are the black slates of Malvern, and some scanty outliers which, in Shropshire, lie between the old Longmynd rocks and the base of the Stiper-stones. The former were then (as has already been shown) supposed by him to belong to the Llandeilo, or rather to the passage-beds between the Llandeilo and Cambrian (Bala); while with regard to the latter, Ramsay expressly tell us that they were not originally classed with the Silurian, but have since been included in it. (Mem. Geol. Sur. iii. part 2, page 9; and 242, foot-note)

The Llandeilo beds were by Murchison distinctly stated to be the base of the Silurian system (Sil. Sys. 222); and it was further declared by him that in Shropshire (unlike Caermarthenshire) "there is no passage from the Cambrian to the Silurian strata," but a hiatus, marked by disturbances which excluded the passage-beds, and caused the Lower Silurian to rest unconformably upon the Longmynd rocks. (Ibid, 256; and plates 31, sections 3 and 6; 32, section 4.) But in "Siluria" (1st ed. 47) the two are stated to be conformable; and in the subsequent sections of this region, made by Aveline, and published by the Geological Survey, the evidences of this want of conformity do not appear. Murchison at that time confounded the rocks of the Longmynd with the Cambrian (Bala) beds of Caermarthenshire and Brecon. (Sil. Sys. 416). Hence it was that he gave the name of Cambrian to the former; and this mistake, moreover, led him to place the Cambrian of Caermarthenshire beneath the Landeilo. It is clear that if he claimed no well-defined base to the Llandeilo rocks in this latter (their typical region), it was because he saw them passing into the overlying Bala beds. There was, in the error by which he placed *below* the Llandeilo, strata which were really *above* them, no ground whatever for afterwards including in his Silurian system, as a downward continuation of the Llandeilo rocks (which are the basal portion of the Bala group), the whole Festiniog group of Sedgwick; whose infra-position to the Bala had been shown by the latter long before it was known to be fossiliferous.

It was, however, claimed by Murchison that no line of separation can be drawn between these two groups. The results of Ramsay and of Salter, as set forth in the address of the former before the Geological Society in 1863, and more fully in the Memoirs of the Geological Survey (vol. iii. part 2) published in 1866, with a preface by himself, as the director of the Survey, are completely ignored by Murchison. The reader familiar with these results, of which we have given a summary, finds with surprise that in the last edition of "Siluria," that of 1867, they are noticed in part, but only to be repudiated. In the five pages of text which are there given to this great Middle Cambrian division, we are told that the distinction between the Lower Tremadoc and the Lingula flags "is difficult to be drawn," and that the Upper Tremadoc slate passes into and forms the lower part of the Llandeilo, "into which it graduates conformably" ("Siluria," 4th ed. p. 46). In each of these cases, on the contrary, according to Ramsay, there is observed "a break very nearly complete both in genera and species, and probable unconformity;" the evidence of the palæontological break being furnished by the careful studies of Salter; while that of the stratigraphical break, as we have seen, leaves no reason for doubt. (Mem. Geol. Sur. iii. part 2, pp. 2, 161, 234). The student of "Siluria" soon learns that in all cases where Murchison's pretensions were concerned, the book is only calculated to mislead.

The reader of this history will now be able to understand why, notwithstanding the support given by Barrande, by the Geological Survey of Great Britain, and by most American geologists, to the Silurian nomenclature of Murchison, it is rejected, so far as the Lingula-flags and the Tremadoc slates are concerned, by Lyell, Phillips, Davidson, Harkness, and Hicks in England, and by Linnarsson in Sweden. These authorities have, however, admitted the name of Lower Silurian for the Bala group or Upper Cambrian of Sedgwick; a concession which can hardly be defended, but which apparently found its way into use at a time when the yet unravelled perplexities of the Welsh rocks led Sedgwick himself to propose, for a time, the name of Cambro-Silurian for the Bala group. This want of agreement among geologists as to the nomenclature of the lower palæozoic rocks, causes no little confusion to the learner. We have seen that Henry Darwin Rogers followed Sedgwick in giving the name of Cambrian to the whole palæozoic series up to the base of the May Hill sandstone; and the same view is adopted by Woodward in his Manual of the Mollusca. The student of this excellent book will find that in the tables giving the geological range of the Mollusca, on pages 124, 125, and 127, the name of Cambrian is used in Sedgwick's sense, as including all the fossiliferous strata beneath the May Hill sandstone. On page 123 it is however explained that Lower Silurian is a synonym for Cambrian, and it is so used in the body of the work.

The distribution of the Lower and Middle Cambrian rocks in Great Britain may now be noticed. The former, or Bangor group, to which Murchison and the Geological Survey restrict the name of Cambrian, and which they sometimes call the Longmynd, bottom or basement-rocks, occupy two adjacent areas in Caernarvon and Merionethshire; the one near Bangor, including Llanberis, to the north-east, and the other, including Harlech and Barmouth, to the south-east of Snowdon; this mountain lying in a synclinal between them, and rising 3,571 feet above the sea. The great mass of grits or sandstones appears to be at the summit of the group, but in the lower part the blue roofing slates of Llanberis are interstratified in a series of green and purple slates, grits, and conglomerates (some of the Welsh roofing-slates are, however, supposed to belong to the Llandeilo). (Mem. Geol. Survey iii. part 2, pages 54, 258). The Harlech rocks, in this north-western region are conformably overlaid by the Menevian, followed by the true Lingula-flags, or Olenus beds, of the Middle Cambrian. Upon these repose the Tremadoc slates, which are not known in the other parts of Wales. The third area of Lower Cambrian rocks is that already described at St. David's in Pembrokeshire, about 100 miles to the south-west; and the fourth, that of the Longmynd hills, about sixty miles to the south-east of Snowdon. The rocks of the Longmynd, like those of the other Lower Cambrian areas mentioned, consist principally of green and purple sandstones with conglomerates, shales, and some clay-slates. They occasionally hold flakes of anthracite, and small portions of mineral pitch exude from them in some localities. The only evidence of animal life yet found in the rocks of the Longmynd is furnished by worm-burrows, the obscure remains of a crustacean (the *Palæopyge Ramsayi*) and a form like *Histioderma*. This latter organic relic, with worm-burrows, and the fossils named *Oldhamia*, is found on the coast of Ireland opposite Caernarvonshire, in the rocks of Bray Head; which resemble lithologically the Harlech beds, and are regarded as their equivalents.

Still another area of the older rocks is that of the Malvern hills, on the western flanks of which, as already mentioned, the Lingula flags are represented by about 500 feet of black shales with *Olenus*, underlain by 600 feet of greenish sandstones containing traces of fucoids, with Serpulites and an *Obolella*. It is not improbable, as suggested by Barrande and by Murchison, that these 1,100 feet of strata represent, in this region, the great mass of the Lingula flags, and, we may add, perhaps the whole series of Lower Cambrian strata, which in Caernarvonshire and Pembrokeshire underlie them; since these sandstones of Malvern, like those of St. David's, rest upon crystalline schists, and are in part made up of their ruins.

These crystalline schists of Malvern, which are described by Phillips as the oldest rocks in England, and by Mr. Hull are conjectured to be Laurentian, seem, from the descriptions of their lithological characters, to resemble those of Caernarvon and Anglesea, with which they are, by Murchison, regarded as identical. The crystalline schists of these latter localities are by Sedgwick described as hypozoic strata, below the base of the Cambrian. Murchison, however, in the first edition of his "Siluria," adopted the suggestion of De la Beche that they themselves were altered Cambrian strata. In fact, they directly underlie the Llandeilo rocks, and were apparently conceived by Murchison to represent the downward continuation of these, upon which he had insisted. This opinion is supported by ingenious arguments on the part of Ramsay (Mem. Geol. Survey, iii. part 2, *passim*). I am however disposed to regard them, with Sedgwick and Phillips, as of pre-Cambrian age, and to compare them with the Huronian series of North America, which occupies a similar geological horizon, and with which, as seen in northern Michigan, and in the Green Mountains, I have found the rocks of Anglesea to offer remarkable lithological resemblances.

It may here be noticed that the gold-bearing quartz veins in North Wales are found in the Menevian beds, and also, according to Selwyn, throughout the Lingula flags. These fossiliferous strata at the gold mine near Dolgelly appear in direct contact with diorites and chloritic and talcose schists, which are more or less cupriferous, and themselves also contain gold-bearing quartz veins (Mem Geol. Survey, part 2, pp. 42, 45, and Siluria, 4th ed, 450, 547).

The following table gives a view of the lower palæozoic rocks of Great Britain and North America, together with the various nomenclatures and classifications referred to in the preceding pages. In the second column, the horizonal black lines indicate the positions of the three important palæontological and stratigraphical breaks signalised by Ramsay in the British succession (Mem. Geol. Survey, iii. part 2, page 2). In a table by Davidson in the *Geological Magazine* for 1868 (v. 305) showing the distribution of organic remains in these lower rocks, he gives, as the Festiniog group of Sedgwick, only the Dolgelly and Maentwrog beds of Belt (the Upper and Middle Lingula flags), and makes of the two divisions of the Tremadoc rocks a separate group; the whole being described as the Upper Cambrian of Sedgwick. This, however, is not the present grouping and nomenclature of Sedgwick, nor was it his earlier one. So far as regards Middle and Upper Cambrian, this discrepancy is explained by the fact already stated, that in 1843 Sedgwick proposed as a compromise the name of Cambro-Silurian for his Bala group, previously called Upper Cambrian; by which change the Festiniog or Middle Cambrian became Upper Cambrian. When the true relation between the Lower Silurian of Murchison and the Bala group was made known, Sedgwick, as we have seen, re-claimed for the latter his former name of Upper Cambrian; but this had meanwhile been adopted for the Festiniog group, in which sense it is still used by Lyell, Phillips, Davidson, Harkness, and Hicks. The Festiniog group, or Middle Cambrian, as defined by Sedgwick, however, included not only the whole of the Lingula flags, but the Upper and Lower Tremadoc rocks (Philos. Mag. IV. viii, 362).

LOWER PALÆOZOIC ROCKS OF EUROPE AND NORTH AMERICA

	BRITISH SUB DIVISIONS.	NORTH AMERICAN SUB-DIVISIONS	NOMENCLATURES OF SEDGWICK AND MURCHISON.	BARRANDE'S CLASSIFICATION.	ANGELIN'S DIVISIONS.
14	Ludlow.	Lower Helderberg	Silurian, *Sedgwick.* Upper Silurian, *Murchison.*	Third fauna, including Etages H, G, F, E.	VIII. VII, or Regiones E, and DE.
13	Wenlock.	Niagara, Clinton,			
12	Upper Llandovery	Medina, Oneida.			
11	Lower Llandovery.	Hudson-River, Utica,	Upper Cambrian or Bala group, *Sedgwick.* Lower Silurian, *Murchison.*	Second fauna, including Etage D.	VI. V. IV., or Regiones D, C, and BC.
10	Caradoc.	Trenton, Birdseye,			
9	Upper Llandeilo	Black-River.			
8	Lower Llandeilo.	Chazy.			
7	Upper Tremadoc.	Levis.	Middle Cambrian or Festiniog group, *Sedgwick* Primordial Silurian, *Murchison.*	First fauna, or Primordial fauna, including Etage C, and probably also Etage B.	III II. I, or Regiones B, and A, and Regio Fucoidarum.
6	Lower Tremadoc.	Calciferous.			
5	Dolgelly	Potsdam.			
4	Maentwrog.				
3	Menevian.	Braintree and St. John	Lower Cambrian or Bangor group, *Sedgwick.* Cambrian, *Murchison.*		
2	Harlech	——— ?			
1	Llanberis.	——— ?			

The only change which I have made in the groupings of the British rocks adopted by Sedgwick and by Murchison, is in separating the Menevian or Lower Lingula flags from the Festiniog, and uniting it with the Bangor group or Lower Cambrian. In this I follow, with Lyell and Davidson, the suggestion of Salter and Hicks.

In the third column, the sub-divisions are those of the New York and Canada Geological Surveys; in connection with which the reader is referred to a table published in 1863, in the "Geology of Canada," p. 932. Opposite the Menevian I have placed the names of its principal American localities, which are Braintree, Mass., St. John, New Brunswick, and St. John's, Newfoundland. With regard to the classification of Angelin, it is to be remarked that, although he designates II. as *Regio Olenorum*, and III as *Regio Conocoryphorum*, the position of these, according to Linnarsson, is to be reversed; the Conocoryphe beds with *Paradoxides* being below, and not above, those holding *Olenus*. The *Regio Fucoidarum* in Sweden has lately furnished a brachiopodous shell, *Lingula monilifera*, besides the curious plant-like fossil, *Eophyton Linnæanum*. (Linnarsson, Geol. Mag., 1869, vi. 393.)

T. STERRY HUNT

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, May 7.—Prof. Newton, V.P., F.R.S., in the chair. The secretary read a report on the additions that had been made to the society's collection during the month of March, 1872, amongst which were two red-necked bustards (*Eupodotis denhami*), presented by Mr. C. D. O'Connor and Governor Ussher, and a Beatrix antelope (*Oryx ebatrix*) from the Persian Gulf, received on deposit.—Mr. P. L. Sclater exhibited and made remarks on a skull of the Hairy Tapir of the Andes (*Tapirus roulini*) obtained by Mr. Buckley during his recent expedition to Ecuador.—Prof. Owen read the eighteenth of his series of memoirs on the extinct birds of the genus Dinornis and its allies, in which was contained the description of the pelvis and bones of the leg of *Dinornis gravis*, a supposed new species allied to *D. crassus*, and a general *résumé* of the described species of the genus *Dinornis*.—The Viscount Walden, F.R.S., communicated an appendix to his paper on the birds of Celebes, read at a former meeting of the society, and containing an account of twelve species to be added to the Celebean Avifauna. This raised the total number of known species of Celebean birds to 205.—Mr. Henry Buckley exhibited the eggs of three species of North American birds, which he believed had never previously been obtained. The eggs were those of *Falco polyagrus*, *Elanoides furcatus*, and *Ictinia Mississippiensis*.—Mr. H. E. Dresser exhibited the egg of *Querquedula marmorata*, collected in Spain by Major Irby, this being probably the first authenticated instance of the breeding of this bird in Spain.—A communication was read from Mr. W. H. Hudson, containing field notes on the habits of the swallows, of the genus *Progne*, met with in the Argentine Republic. To this was added some notes on the species by Mr. P. L. Sclater.—A communication was read from Mr. G. French Angus, containing descriptions of ten new species of land and marine shells, mostly from Australia.—A second communication from Mr. Angus contained the description of a new species of *Voluta*, proposed to be called *Voluta hargravesi*.—A paper by Mr. H. Adams was read, in which he described a new species of *Geotrochus* from the Island of New Britain, proposed to be called *G. fergusoni*.—A communication was read from Dr. J. E. Gray, F.R.S., containing a description of *Peltastes forsteni*, a species of land tortoise from Celebes.—Two communications were read from Mr. J. M. Brazier, giving descriptions of land and marine shells collected in Australia and Lord Howe's Island.—A communication was read from Mr. A. Anderson, containing some additional notes on the Raptorial birds of North Western India.—A paper by Dr. J. E. Gray, F.R.S., was read, describing a young Tapir from the Peruvian Amazons, which he proposed to call *Tapirus terrestris peruvianus*.—A communication was read from Dr. J. E. Bowerbank, F.R.S., containing the third part of his contributions to a general history of the *Spongiadæ*.

Entomological Society, May 6.—Mr. H. T. Stainton, F.R.S., vice-president, in the chair.—Mr. E. Saunders exhibited a series of species of Australian *Buprestidæ*, illustrating the great sexual differences existing in these insects.—Mr. F. Smith exhibited a large collection of *Hymenoptera*, chiefly *Aculeata*, sent by Mr. G Lewis from Hiogo in Japan. The whole collection was strikingly European in its aspect, though the species were in most cases different; the genera were all represented in Europe save one genus of ants.—Mr. Verrall exhibited an example of *Syrphus lasiophthalmus* with a peculiar malformation of two tibiæ, which appeared as though they had been broken, probably when the insect had just emerged from the puparium, and badly united afterwards. — Mr. Stainton exhibited an aspen leaf, sent by Lord Walsingham from Oregon, pierced by a multitude of small oval holes, caused by small mining micro-lepidopterous larvæ, which each detached an oval case formed of the cuticles of the leaf. These cases had produced a minute moth of the genus *Aspidisca*, which he also exhibited.—Mr. E. Saunders read "Descriptions of twenty new species of *Buprestidæ*."—Mr. H. W. Bates read a memoir on the Longicorn *Coleoptera* of Chontales, Nicaragua; enumerating 242 species sent home by Mr. Belt, and describing the new forms. This collection elicited two general facts—firstly, the homogeneity of the insect fauna of the forest region of tropical America, over, probably, 45 degrees of latitude, and, secondly, the existence of a distinct northern element, whose metropolis is Central America.

Chemical Society, May 2.—Dr. Frankland, F.R S., president, in the chair. Mr. E. Riley delivered his lecture on the Manufacture of Iron and Steel. The lecturer in his discourse treated of the influence of the elements associated with iron in the pig, and the part they play in the subsequent conversion of pig into wrought-iron and steel. Although in certain districts there is not much variation in the pig made, the same ore and fuel being constantly used, yet in others, as South Wales and Staffordshire, so many varieties of ore are employed that pig of all descriptions is produced. From the results of the analysis of samples of Yorkshire hot-blast pig (No. 1 to 6 iron) from the same works, it would appear that whilst the phosphorus is almost constant in all the kinds, namely, about 0 9 per cent, the quantity of sulphur decreases and that of silicon increases with the number. It is possible that the differences in the amount of sulphur present would explain the differences in the quality of the pig, for it is certain that sulphur makes grey iron white; but, at the same time, the different numbers of grey iron may be produced by differences in the rate of cooling. On examining the pigs from which the best wrought-iron is made, they will be found to contain silicon and phosphorus. Swedish iron, which contains no phosphorus and but little silicon, when used by itself, gives red short iron. It will be seen from this that silicon and phosphorus play an important part in the manufacture of iron. Hæmatite pig frequently contains as much as 4 or 5 per cent. of silicon. The chief constituents of pig-iron are, besides iron, carbon, silicon, sulphur, phosphorus, and manganese, traces of copper and titanium (the latter only in grey iron), frequently nickel and cobalt, and occasionally vanadium and arsenic. The per-centage of carbon in pig iron varies from 3 to 4 per cent., but the question as to whether it forms any definite compound with iron is open to great doubt. Mr. Snelus has shown that by sifting out the finer portions from the borings of Middlesboro' pig a material could be obtained containing 7 per cent. of carbon, and by elutriation one containing more than 41 per cent. The sulphur seems always to be derived from the sulphide of iron present in the fuel or ore, but from some experiments it would seem that an excess of lime may act on the sulphide in the coke and convert it into sulphide of calcium and metallic iron. Silicon is always present to a greater or less extent in iron. With respect to phosphorus, practically speaking, all that is present both in the ore and in the fuel passes into the iron. After some remarks on the comparatively small value of titanium as an ingredient of iron, the speaker discussed the quality and composition of the fuel employed in smelting, and then passed on to the process of refining. The time required for this seems to depend on the quantity of silicon present in the pig, much of it being separated during the operation, along with some sulphur and phosphorus, and a little carbon. The process of puddling was then described, and the merits of the various machines for superseding manual labour discussed, with especial reference to the results obtained with that of Mr. Danks; the great advantage of machine puddling being the uniform quality of the wrought-iron made. In conclusion the author made some remarks especially with reference to the occurrence of silicon in steel. This elaborate and exhaustive memoir was copiously illustrated by analyses.

BOOKS RECEIVED

ENGLISH.—The Earth's Crust: D Page (Blackwoods)—The Flora of British India, Part I.: Dr. J. D. Hooker (L. Reeve and Co)—Air and Rain, the beginning of a Chemical Climatology: R. Angus Smith (Longmans).—Man in the Past, Present, and Future: D. L Buchner, translated by W. S. Dallas (Assher and Co).—Naval Science, No 1: Edited by E. J. Reed (Lockwood).

AMERICAN.—The Geological Survey of Ohio; Report of Progress, 1870: J. S. Newberry.

DIARY

THURSDAY, MAY 16.

ROYAL SOCIETY, at 8.30—On the Specific Heat and other Physical Characters of Mixtures in Methylic Alcohol and Water: Dr. Dupré—On Supersaturated Saline Solutions, Part III: C. Tomlinson, F R.S, and G. Van der Mensbrugghe.—Remarks on the Sense of Sight in Birds: Dr. R. J. Lee.

SOCIETY OF ANTIQUARIES, at 8.30.—Prehistoric Implements in Eden-side, near St. Bees, Cumberland: R. D Darbishire, F.G.S.

CHEMICAL SOCIETY, at 8.

FRIDAY, MAY 17.

ROYAL INSTITUTION, at 9—On the more important Substitutes for Gunpowder: Prof. Abel, F.R.S.

SATURDAY, MAY 18.

ROYAL INSTITUTION, at 3.—On the Chemical Action of Light: Prof. Roscoe, F R.S.

GOVERNMENT SCHOOL OF MINES, at 8.—On Geology: Dr. Cobbold, F R S

MONDAY, MAY 20.

ANTHROPOLOGICAL INSTITUTE, at 8.—On a New Instrument for Measuring the Proportions of the Human Body: Joseph Bonomi—On Moral Irresponsibility resulting from Insanity: George Harris.

TUESDAY, MAY 21.

ZOOLOGICAL SOCIETY, at 9.—On the Royal Antelope and allied species of *Nanotragus*: Sir Victor Brooke, Bart.—Notes on the Anatomy of the Huia Bird: A H. Garrod

ROYAL INSTITUTION, at 3.—On Development of Belief and Custom: E. B. Tylor.

WEDNESDAY, MAY 22.

GEOLOGICAL SOCIETY, at 8.—On the Phosphatic Nodules of the Cretaceous Rock of Cambridgeshire: Rev. O. Fisher, F G.S—Some Observations on the Upper Greensand Formation of Cambridge: W. Johnson Sollas. Notes on Sand-pits, Mud Volcanoes, and Brine-pits, met with during the Yarkand Expedition of 1870: Dr. G. Henderson.

SOCIETY OF ARTS, at 8.—On Painted Metallic Hangings for Mural Decorations: G. Clark.

THURSDAY, MAY 23.

ROYAL INSTITUTION, at 3.—On Heat and Light: Prof. Tyndall, F.R.S.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, MAY 23, 1872

THE OXFORD SCHEME OF NATURAL SCIENCE[*]

ALL true lovers of English science, we might say all discerning lovers of their country, must have watched with anxious interest the efforts which in these latter times a few able and energetic men at each of our ancient universities have been making to strengthen and widen the study of Natural Science in those so-called seats of learning; and must have admired the zeal and wisdom with which they have fought against the stubborn resistance of the powers that be. To all such the recently-issued programme of the Board of Natural Science Studies at Oxford will have particular interest; for upon the wisdom of the regulations introduced by that Board will depend in great measure the future of Natural Science at Oxford.

In calling attention to this programme we shall so far rely on our well-known admiration of the talents and energy of the members of the Board as to take leave to say nothing of its good points. These will be read and known of all men who love science; and the results of the scheme, as far as its good features are concerned, will be the reward of its framers, whether, as we hope, they succeed in establishing Natural Science at Oxford in its proper dignity and power, or whether, as we fear, they eventually succumb to all those many influences which seem slowly but surely to be pressing the life out of both Oxford and Cambridge.

We think we shall be best furthering the interests of Science if we content ourselves with pointing out the blots of the scheme, blots which stand out in the stronger relief the more one dwells on what ought to be its immediate results, viz., the destruction of mere cram-work and the encouragement of the spirit of original research.

We learn that the student who wishes to take honours in Natural Science, must undergo a previous examination in the elements of Mechanics, Physics, and Chemistry. We have no remark to make on this, except to suggest that if the elements of Biology had been added, matters would have been vastly simplified, and much of the confusion to which we shall have presently to refer entirely done away with.

Having passed this previous examination, the student may elect to be examined in either Physics, Chemistry, or Biology, with or without certain special subjects, such as Geology, Zoology, or Botany.

Touching the examination in Physics we will simply express our regret that there is no definite statement that the examination in this branch will be partly of a practical kind. This is of course intended, but it is not specially stated. If the distinguished Oxford Professor of Experimental Physics knew what hard work his friends had to persuade the world that his superb laboratory is a real honest workshop, and not a gorgeous palace and magnificent show-room, he would have had the instructions touching the practical part of the examination in Physics printed in capitals or italics rather than omitted altogether.

With regard to the Chemistry we will only remark that the restriction of the practical part of the examination to the old-fashioned inorganic analysis seems to us to mark an opportunity lost. Is the detection and estimation of a base and an acid for ever to remain the be-all and end-all of practical chemistry? We trust not. A change in this respect must soon take place in our examining bodies, and Oxford might have led the way.

It is to the biological part of the scheme that we desire more particularly to call the attention of our readers. The student who wishes to go out in Biology may take the general subject of Biology either alone or together with certain restricted special subjects, such as Comparative Osteology, Ethnology, &c., or together with either of the larger special subjects, Zoology, Botany, and Geology. The general subject of Biology comprises General and Comparative Anatomy and Histology, both animal and vegetable, Human and Comparative Physiology, with Physiological Chemistry, and "the general philosophy of the subject." Our readers will naturally wonder what is meant by this last heading. They probably have been accustomed to consider that science is philosophy, and that the philosophy, for example, of comparative anatomy, grows out of the facts organically, is borne by the subject as plants bear flowers and fruits, and cannot be pinned on, like an artificial blossom on a garland, either in an examination or elsewhere. The mystery disappears when we turn to the list of books recommended (about which we shall have a word to say), where we find under the head of General Philosophy Agassiz' "Classification" and Whewell's "Inductive Sciences." Evidently the Board desires to try the students as the saints of old were tried in order to be made perfect; they tempt them with courses of evil reading to see whether the truth be in them or no. But to return: the scheme, as it stands at present, does very well for a man who goes in for the general subject of Biology on the strength of his acquaintance with the contents of the Oxford Museum, and his knowledge of that Physiology falsely so called which is built up on a comparison of the organs of one animal with those of another. The practical examination in dissections and histological preparations will most likely fairly test his proficiency in these matters, and if he takes up one of the minor special subjects he will probably have a good but limited idea of one particular series of facts, *e.g.*, the various forms of stomach presented by the animal kingdom. We doubt very much if the examination and preparatory course of study will have helped to make him a fruitful man of science, working in and for ideas. Probably the reverse.

But how much better off is he than the poor botanist who goes to seek at Oxford the means and help to make a working man of science of himself. He must first go through the general examination in Biology, must cram up text-books on Human Physiology, must read up all the futilities of the modern animal histologists, and dabble in the speculations of physiological chemists, to say nothing of his knowledge of the exoskeleton of the Arthropoda and other bits of special animal morphology, before he can turn to his heart's desire, the classification of Phanerogams. And he will cram; that will be the inevitable result.[*]

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[*] Notice of the Board of Studies for the Natural Science School of the University of Oxford, 1872.

[*] The prolixity of some members of the Board seems to have driven the framer of the section of Botany into the opposite extreme. We cannot think it intended that the examination in Botany should be so feeble as the programme seems to intimate.

Hardly better off is the physiologist, properly so called. What he needs as general education is a thorough knowledge of physics and chemistry, with a general acquaintance with the fundamental laws and simpler facts of animal morphology. Instead of that, this scheme takes him away from physics and chemistry, and tries to smother him with the specialities of morphology, to which he most probably has a decided natural repugnance.

Worst off of all is the geologist, who has to go in for all three general subjects before he can make his special knowledge weigh with examiners, and who therefore will be induced to keep on cramming heterogeneous knowledge up to the last moment; forasmuch as the statutes tell him that his place will depend "*on the joint result of his examination in all his subjects.*"

In our humble opinion it would have been far better to have made the previous examination to include the elements of Biology, and at the same time rather more searching as a whole. Every science scholar would then have known something of everything essential in Natural Science. Freed by this examination, he might have turned at once to that something of which he ought to know everything—to Geology, to Botany, to Animal Morphology, to Physiology, to Chemistry, to Physics. He would thus have, on the one hand, a sound foundation, with an insight into "the general philosophy," properly so called, of all science, and by early training in a special branch would have had his face set towards original work. We have no space left to criticise the various items of the special directions under Biology, Geology, &c.; but we cannot refrain from uttering our protest against the pernicious habit, carried to an excess in this programme, of recommending particular books for study. This always means "particular books for examination," and is the most potent nurse of cram. Nor is the evil mended much by making the list of books large and long; for rather another evil is then introduced, viz., that of giving authoritative sanction to bad books. We venture to assert that no man of science can look through the list of books appended in this programme to his own particular line without being compelled to admit that some of the books recommended are essentially bad. We have spoken freely because the matter is one we have at heart. We must confess that the scheme at Oxford, as it stands at present, is inferior to that at Cambridge (excepting always that fearful system of order of merit, which hangs like a millstone round all Cambridge studies), and decidedly inferior to what the scientific examinations of the University of London will be when some necessary changes have been made in its B.Sc.

MÄDLER'S HISTORY OF ASTRONOMY

Geschichte des Himmelskunde, nach ihrem gesammten Umfange, von Dr. J. H. von Madler, Emeritirtem Professor und Director der Sternwarte, Dorpat. 1 Band, i. ii. iii. Lieferungen. (Braunschweig, 1872. London: Williams and Norgate.)

A PERIOD when the love of astronomical study—long confined to a few select votaries, and dormant in general estimation—has attained an unprecedented and unlooked-for diffusion, is well suited for the appearance of such a historical view of the subject as may n
only form a book of reference and comparison for t
more advanced cultivator of the science, but may co
mend itself to the less instructed student, as well by t
accuracy of its statements as by the perspicuity of
views and the simplicity of its expression. The work
which the earlier portion is now lying before us, seen
as far as can be foreseen, well calculated to answer t
end, and Germany may be congratulated upon the co
mencement, at least, of what, ere long, ought to be ma
popularly accessible among ourselves. We possess, i
deed, already, in the "History of Physical Astronom
by Prof. Grant, a work of the highest pretension as to a
curacy and intelligence; but the subject is there regard
from a different point of view, and treated in a differe
manner, and there is abundant room for each of them.

For those who venerate the Observatory of Dorp
from the high position which it took and maintain
under the elder Struve, as well as for those who love
contemplate and examine in detail the wonderful featu
of our satellite, this work will possess a double intere
both as originating within the walls of that honour
building, and as proceeding from the pen of the leadi
selenographer of his own time. It is only to be hop
that the advancing years of the venerable author may n
interrupt the progress of what has been so auspiciou
begun. At present it has been carried only as far as t
establishment of Tycho *de* Brahe, as it seems to be t
inaccurate custom of the Germans to call that great ma
in his island of Hween, where "lived the Prince of Ast
nomy in external fitness, with princely aspect and sple
dour." But before the science had attained this d
recognition as worthy of royal patronage and aristocra
cultivation, it is needless to say that it had to pass throu
long periods of darkness and discouragement and di
culty, when its progress was retarded by superstitio
bigotry, chilled by heedless indifference, or entangled
the substitution of imaginary hypotheses for the pati
labour of protracted investigation; and in tracing throu
all these impediments its gradual and interrupted advan
the learned author has shown not only a full command
the subject, but great fairness and discrimination in
treatment; and if it might be hinted that the style
sometimes encumbered by a repetition of unvaried me
phors, yet it is always perspicuous and pleasing. T
only point as to which positive censure would not be
vidious is no fault of the author, but of the corrector
the press, who must be held answerable for some offens
errata in the Greek and Latin citations. For errors
English and Welsh, through which Bradley's benefices
Bridstow, in Herefordshire, and Llanddewi-velvre,
Pembrokeshire, appear as "Bradstone" and "Welfr
some intermediate authority must be responsible; but
may fear that our own rendering of German names is
invariably more accurate. In order not to interrupt
current of the history, much of the biographical ma
appears in the form of notes at the foot of the page,
quently of especial interest; we feel, however, the w
of occasional references. The intensity of the auth
annoyance at the monkish system may possibly lead so
readers to an occasional smile; but with the Papacy a
whole no religious difference has prevented him fr
dealing with the candour and justice of a true histori
Of Pliny's merits he has formed a very unfavourable ju

ment, and from this or some other cause, has not noticed a passage in that writer which seems to have been singularly overlooked, but which nevertheless possesses a certain degree of interest. It has been invariably asserted that Hipparchus was incited to the formation of his celebrated catalogue by the appearance of a new star, leaving it to be inferred that it was an object similar to the Great Star of 1572 (the possible return of which, by the way, Baron von Mädler refers to 1885, instead of the present year, as has been sometimes thought), or that of 1604. But it seems to have altogether escaped notice that the words of Pliny in reference to it expressly describe a movement which must have placed it in another class of bodies:—"Novam stellam et aliam in ævo suo genitam deprehendit: ejusque motu, qua die fulsit, ad dubitationem est adductus, anne hoc sæpius fieret, moverenturque et eæ, quas putamus affixas." Such is his statement; where he obtained it of course cannot now be ascertained; but from its explicitness it certainly carries at least a show of authority.

In adopting a more favourable idea of Ptolemy than has been admitted by many opponents of his system, the author has expressed an opinion well deserving of attention:—"When criticising the literary proceedings of Ptolemy, we should not forget how extremely different, as compared with our own, was the form which the mutual relation of authors took in those days. Instead of the hundreds of thousands, or even millions, of books which fill our libraries and catalogues, their number at that time might hardly amount to a thousand; the principal works especially were so few in number that every one, generally speaking, who read and wrote was acquainted with them. Ptolemy wrote for his own time. When he alleged anything which was the property of another without mentioning his name, nobody could then have been well deceived by it, and there could be no reasonable question of a design of plagiarism." And as an incidental parallel he remarks the use made in a similar manner by St. Paul of the expressions of Archias and Epimenides.

Some interesting, but perhaps not generally known, facts may find a suitable place in the present brief notice; such as the discovery on the site of what is conjectured to have been Cicero's house at Rome of a sun-dial, which may have been the identical one mentioned in one of his letters; the employment, in Seneca's time, of hollow glasses filled with oil to protect the eye in observing solar eclipses; the grandeur of speculation which led Cleomedes, some fifteen centuries ago, to assert that the earth would show but as a point to the sun, and from the fixed stars, even if it possessed intrinsic light, would be imperceptible; the discovery in Egypt, in 1854, of four wooden tablets covered with plaster, containing astronomical calculations—the almanac, in fact, of the great school of Alexandria in the reigns of Trajan and Hadrian; the recognition of Uranus by the ancient inhabitants of Tahiti. Relations such as these lend an additional interest to a narrative which, even without them, would not be felt as dry or tedious.

One more passage may be cited, as giving full evidence of that soundness of thought and feeling which thus (but not thus only) are shown to be united in the Baron von Mädler with the other qualifications of a historian:—

"If in those times a comet appeared, writings appeared immediately, especially in the form of religious exhortations, taking occasion from it to recommend repentance and amendment. Let no one suspect that we have even the slightest objection to offer to these admonitions. Much rather could we wish that at other times also, whether a comet were visible or not, they were employed with equal earnestness, and that the inscription on a comet medal of that date (1472)—

> God grant us from this comet-blaze
> To learn amendment of our ways—

were more laid to heart, especially as regards the second line. If cometary prediction had brought nothing worse to light than exhortations to amendment, we might with respect to this fancy (though the fancy itself, as such, would always remain objectionable) have been able to contemplate the whole with greater satisfaction."

T. W. WEBB

OUR BOOK SHELF

Botany for Beginners: an Introduction to the Study of Plants. By Maxwell T. Masters, M.D., F.R.S. (London: Bradbury, Evans, & Co., 1872.)

THIS is in no sense a cram-book. To take the trouble of learning it by heart, page for page, would not suffice for any botanical examination with which we are acquainted. This is a great advantage in an elementary scientific work. Not only does it enable the author to be entirely independent of the favourite points of particular examiners; but it permits him to pursue his own method of developing the subject in the learner's mind. In no science is this freedom of greater value than in botany. The text-books used and recommended by many teachers of botany would appear to have been especially designed to deter the intending student from the study of the science. Bristling at the outset with a formidable array of technical terms, which should never be introduced till a later stage of the instruction, they give a superficial countenance to the idea which is prevalent even with many who ought to know better, that Botany is a mere science of terms, unworthy to be placed by the side of Comparative Anatomy or Animal Physiology. Each teacher will no doubt have his own idea of the arrangement of his subject best calculated to interest the beginner, and to lead him on step by step to see the true dignity of the science. Dr. Masters's is recommended by his own experience as a lecturer for many years to one of our Metropolitan hospitals. He commences by taking in succession a series of flowers in the order in which they are to be met with as the spring unfolds—willow, poplar, ash, elm, tulip, hyacinth, apple, lilac, and so forth; and in plain and attractive language, bringing in technical terms at the outset only when necessary for the sake of accuracy, he explains the structure of their different parts, and the points in which they resemble or differ from one another. The more important phenomena of the physiology of plants are also brought under review as the descriptions of structure naturally lead up to them, though we think that more space might with advantage have been bestowed on this portion of the subject. A single page devoted to the decomposition of carbonic acid by the leaves, and twelve lines to the process of fertilisation of the ovule, are hardly sufficient to introduce the reader to these branches of physiology, which are not only of the highest importance themselves, but also of far greater interest to the student, if simply and intelligently brought before him, than the details of morphology or of classification. The substance of this little book has already appeared in the columns of the *Gardener's Chronicle*, and it is well illustrated with capital wood-cuts. We heartily recommend "Botany for Beginners" to teachers or parents who are desirous of interesting young persons in this science, and who can appreciate the value of a clearly-written, simple, and yet accurate elementary treatise.

A. W. B.

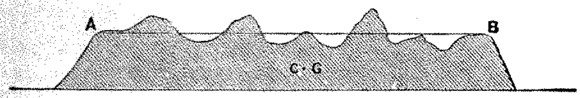
LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Error in Humboldt's Cosmos

THE following letter, from Major-General Sir Henry James, is addressed to Mr. J. Carrick Moore in reference to his communication which appeared in our columns under date April 18.

"I am much obliged to you for sending me NATURE, with your letter in it respecting the manner in which Humboldt has used the term 'Centre of Gravity of the Land,' by which he so far misled Herschel as to make him double the height of the mean elevation of the several continents. It is obvious that Humboldt determined the mean height of their surfaces, and not the height of the centres of gravity of the continents. He determined the height of A B, not of C G. The centres of gravity are, therefore, at half the heights given by Humboldt.



"I have the mean elevation of the surface of Forfarshire determined by the same method explained by you, and by which Humboldt determined the mean elevation of the surfaces of the continents; it was 856 feet above the mean level of the sea. The mean height of the surface of Europe (not of the centre of gravity) is, according to Humboldt, 671 feet, and this is quite as high as I should have expected it to be from the known height of Forfarshire, containing so large an amount of mountainous country. The mean height of the surface of Europe cannot be double this, or 1,042. The height of the water-shed between the Baltic, Black, and Caspian seas is only 1,100 feet.

"I think, therefore, both from the method of investigation and from the results, that you have taken a correct view of the subject, and that you have done science a great service in pointing out the error into which Humboldt has fallen, and by which he misled Herschel to so great an extent.

"John Carrick Moore, Esq." "HENRY JAMES

Southampton, April 22

Fertilisation of Dictamnus

AN arrangement for the distribution of the pollen of *Dictamnus Fraxinella*, which I noticed last June, may interest some of the botanical readers of NATURE. The plants will shortly be in flower, so that any one who is curious about the matter can see it for himself.

To suck the nectar of these flowers, bees stand upon the filaments, which are nearly horizontal; as the anthers are matured they are brought up in turn, generally two at a time, by the curving filaments, into such a position that they rub against the tail of a bee standing on the filaments. As soon as each pair of anthers is emptied they retire, and give place to another pair, and when the last are emptied the style curves up, and brings the stigma into the same position.

The difference in appearance between the empty and full anthers is very striking in dry weather.

Any one who knows the plants must have observed that they are very attractive to bees, and that they produce seeds in great abundance. CHRISTOPHER J. HAYDEN

Newspaper Science

THE general public cannot fail to acquire some very extraordinary as well as erroneous notions about many subjects relating to the progress and application of the different branches of Natural Science, if we are to judge from sundry scraps of information, daily communicated or reproduced for their instruction, in the columns of even the most influential newspapers. Amongst recent examples of this style style of information we might refer to the following:—

Geography.—Under the heading, "Oysters from South America," we find in the *Times* of May 16, the announcement that the steamer *Kaffraria* had last Sunday 'arrived at Hull from Norfolk, Virginia, having on board a cargo consisting of 500 or 600 tons of oysters, &c."

Geology.—The following is reproduced from the *Mechanics' Magazine* in the *Times* of May 16:—"*Per saltum*.—Nantwich in Cheshire has for some years past been gradually sinking, owing to the withdrawal of the lime from the salt lakes, which underlie the town," &c.

Chemistry.—In the *Times* of May 9 it is stated that "according to an analysis made by Prof. Zinno, the elements of the ashes that were thrown out from Mount Vesuvius are chlorine of soda, sulphate of lime, magnesia, allumen, iron, titanio, and silex."

Metallurgy.—The *Engineer* of April 12, in a paragraph on early iron making at Merthyr Tydvil, in South Wales, must rather astonish metallurgists by writing of "bones supplying sulphate of lime" in the process.

Architecture.—In a somewhat elaborate article on the new so-called Selenitic mortar, which in reality takes its name from the introduction of a little sulphate of lime, which when native forms the mineral called selenite in its manufacture, we are informed that "the name given to the improved mortar indicates to a certain extent the nature of the improvement; that it is in the direction of combining *selenious* acid with a base," &c. It would, indeed, be good tidings to chemists to find that selenious acid had become so cheap as to allow of its being used for such purposes; unfortunately, however, the last price lists inform us that selenium, from which it is made, still costs three shillings per drachm.

Although it has been said that it is not reasonable to expect scientific information from newspapers devoted to general topics, we still contend that we are entitled, when newspapers do make such statements, to demand that they shall at least be free from such gross blunders as those contained in the examples here cited, and to which many others might be added.

London, May 18 D. F.

The University of Freiberg

A LETTER signed "Undergraduate," in NATURE of May 9, confounds the School of Mines in Freiberg in Saxony with the University of Freiburg in Baden. The writer and others who wish to know where certain subjects are best taught in German universities will find much information in Ascherson's "Deutscher Universitäts-Kalender." This small book appears twice a year, giving the number of students and the names of lecturers and lectures offered each term in thirty-one universities of Germany, Switzerland, and the Baltic provinces of Russia.

Berlin, May 16 A. OPPENHEIM

Denudation of the Mendips

IN reply to the question of "Inquirer" in last week's number of NATURE, asking for explanation of a passage in my Address to the Geological Society, allow me to observe that geologists judge of the amount of denudation which hills formed by anticlinal axes, such as the Mendips and Ardennes, may have suffered, by prolonging across the range of hills the outcropping edges of the strata thrown up on the flanks of the axis, keeping each bed and each formation in its relative place. Thus, taking the thickness of the Somerset Coal measures, including the Millstone grit, to be on the north side of the Mendips about 9,000 feet, and of the Carboniferous limestone 1,500 feet; the whole of these, together with some upper part of the Old Red sandstone, forming together a mass of not less than 10,000 to 12,000 feet, have been removed from the area of the Mendips, the central axis of which is formed by strata of Old Red sandstone. In the case of the Ardennes, in addition to the Carboniferous strata, Devonian and Silurian strata are thrown up along the central axis at angles which prolonged form great arches, or rather a series of arches, over the hills; for here and there the intermediate synclinal curves bring in portions of the Coal Measures, which have thus been saved from denudation, while they show how much has been removed in the intermediate areas. The whole of the Coal measures, which are there rather thinner than in Somerset, the Lower Carboniferous series, which is much thicker than in England, together with the Devonian and part of the Silurian series, forming together a thickness probably of not less than 15,000 to 20,000 feet, are there removed from the central area.

It is, however, almost impossible to convey an exact notion of these great physical phenomena without illustrative sections and diagrams; and for these allow me to refer "Inquirer" to some papers in which such sections are given, and in which the subject, a very complicated one, is specially treated, viz., Professor Ramsay "On the Denudation of South Wales and the adjacent counties of England" (Memoirs of the Geological Survey of Great Britain, vol. i. p. 297); Dupont's "Essai d'une Carte Géologique des Environs de Dinant" (Bulletin de l'Académie Royale de Bruxelles, vol. xx. pp. 9 and 10); Réunion Extraordinaire de la Societé Géologique de France à Liège (Bulletin de la Société Géologique, 2 ser., vol. xx. p. 761); Elie de Beaumont's and Dufrenoy's Explication de la Carte Géologique de la France, vol. i. pp. 240-64; also to Lyell's Elementary Manual of Geology, article, "Denudation;" and to De la Béché's 'Geological Observer,'' p. 815.

Shoreham, Kent, May 18 JOSEPH PRESTWICH

VOLCANOES AND EARTHQUAKES

THE remarkable series of volcanic phenomena which have lately been exhibited at various parts of the earth's surface within so short a period of time, gives much matter for consideration, and must in due time afford us a rich harvest of facts with which to test the numerous theories which have been started to account for the occurrence of volcanic eruptions and of earthquakes. Even from our at present scanty information we have, I think, something to learn.

First in the series, so far as I am aware, was the very severe earthquake at Independence, Inyo County, California, which took place on Tuesday, March 26, commencing at 2 A.M., and lasting till 7 A.M, during which time "the earth was never for a moment perfectly quiet, and every few minutes heavy shocks, of a few seconds' duration, were occurring; in all there were more than fifty very heavy shocks." This place is only fourteen miles from the Black Rock, a volcano in the Sierra Nevada mountains, "the sides of which are covered with lava, and which is supposed to be an extinct volcano." It is stated that "during the time the shocks were most severe, flashes of light were seen to issue from the top of this mountain, and streams of fire ran down its sides."

The result of this earthquake is summed up in a few words as "the whole country turned topsy-turvy" (*Virginia City* (Nevada) *Enterprise*).

Then a few days afterwards came the terrific earthquake in Antioch, which commenced on April 3, and continued with greater or less severity, "in Aleppo, and as far east as Orfa, beyond the Euphrates," for more than a week, becoming very severe on April 10; here there appears to have been no actual volcanic phenomenon; but it appears from the letters of the Rev. W. Brown in the *Times*, that there exists in the immediate neighbourhood a mountain, "the peculiar conical form of which is very suggestive of an ancient volcano."

The latest African news tells us that "Several violent shocks of earthquake had occurred at Accra, on the Gold Coast, on April 14 and 15, causing considerable damage to the place." And as unwonted atmospheric disturbances have often been connected with volcanic phenomena, it may not be out of place to mention here the fearful hurricane which wrecked every vessel but one in the Harbour of Zanzibar on April 15.

And then on April 24 began the recent eruption of Vesuvius, which will be for ever memorable, not only on account of its magnitude and grandeur, but also, and still most so, by reason of the amazing intrepidity of the man, who, from a pure love of science, remained at his post, like the gatekeeper of Pompeii, throughout the whole of that terrible time, but happily was not, like that heroic soldier, buried in a shower of ashes; the world was spared the loss.

Now is there any connection between these phenomena exhibited in so distant parts of the earth's surface? One thing is certain, namely, that within the short space of a month all this has occurred, and one can hardly help thinking that somehow or other these volcanic countries must be connected underground; it has long been thought that Etna and Vesuvius are points on a volcanic area which passes north-west to the Eifel, Auvergne, and Iceland; has the neighbourhood of Antioch, with its unenviable notoriety for earthquakes, or the West African coast, anything to do with this area? But if so, what shall we say of the Sierra Nevada, why should its volcanoes be active at the same time? Why should the country there be "turned topsy-turvy" by earthquakes?

While pointing out these coincidences, we must not jump too hastily to conclusions from them; for on the one hand we are told that although the Antioch earthquake extended so far east, yet, to the north and south, even at a few miles' distance, nothing whatever was observed, and, on the other hand, that the Californian earthquake was of so superficial a character that "at Hot Springs, while severe shocks were felt on the surface, the men in the mines, 200 feet deep, felt nothing of them." Now the evidence goes to show that the latter earthquake was directly connected with the eruption of a volcano in the neighbourhood, so that, although the origin of the disturbance may be underground, possibly at a very considerable depth, the shocks are at a certain distance quite superficial, and moreover are transmitted in certain definite directions.

Taking all these facts together, they would rather seem to favour the conclusions that at any rate a great many, if not all, of the volcanic regions of the world are connected, and that they are not merely parts of the earth's surface which happen to be over isolated subterranean furnaces, but places where access to the exterior is more easy for the molten matter which lies underneath a great part, perhaps all, of the earth's crust. I must not be understood to be upholding the (shall I say exploded?) theory of the internal fluidity of the earth; I merely mean to point out that such coincidences in point of time ought to make one hesitate before rushing to the other extreme, and looking upon volcanoes as mere local eccentricities.

But it will be said, if there is any general commotion under even the volcanic area of Europe, why do not the extinct volcanoes of Auvergne break out again? Here is a difficulty which is not at all solved by the suggestion that at first occurs to one, that as the raising of the country has drained the enormous lakes, on the borders of which these volcanoes stood, there is no longer a supply of water to rouse them into action, for are there not lakes still in the Eifel, nay, are not those lovely lakes actually in the craters of extinct volcanoes?

Again, who has seen the wonderful natural harbour of Messina from the high ground above the town without believing it to be an extinct submerged crater? If there be still liquid rock below these craters, it may be that they are no longer the points of least resistance. And this is the probable explanation of their inactivity; for it must not be imagined that an eruption of Etna or of Vesuvius, or of any other volcano, necessarily means an ejection of ashes, lava, &c., from the crater, or from any crater; not at all, the weakest point in the vicinity gives way, and thus we have the numerous cones formed which surround every considerable volcano for some distance.

The mention of Auvergne leads me to make a few remarks on the disputed point, as to whether or not the volcanoes in that country have been in eruption within historic times, especially as I see that a correspondent in last week's NATURE has come to the conclusion originally drawn by "an eminent historian and antiquary, Sir Francis Palgrave," as long ago as 1844, and adopted by theological writers ever since, that because a bishop of Vienne established Rogation days on account of some alarming terrestrial phenomena which happened in his diocese, therefore the volcanoes of Auvergne were in action at the time. We have two documents which refer to this matter, a letter written by Sidonius Apollinaris (who lived in the very centre of the Chaine des

Puys, and on the border of a lake which was actually formed by the damming up of a stream by one of the most recent of the lava-currents) to Mamertus, Bishop of Vienne, in which he speaks of the earthquakes that had occurred in the neighbourhood of Vienne; of fire issuing from the earth and wild beasts taking refuge in cities; and the Rogation Homily of Alcimus Avitus, the successor to Mamertus, which mentions the same catastrophes.

Now in the first place Vienne is more than seventy miles in a direct line from the more recent Auvergne volcanoes; in the next, Sidonius himself makes no mention in his writings of any eruptions having taken place in his neighbourhood, although he wrote poems describing the beauty of the scenery: and lastly Auvergne is not mentioned by any ancient writer, neither by Cæsar, who encamped there and laid siege to Gergovia, a city situated on a table-land, with craters close at hand in almost every direction; nor by Pliny, who gives a list of all the then known volcanic countries, including some very out-of-the-way ones; nor by Strabo, nor by any of the poets, as a country where volcanoes were ever known to have been in action.

For these reasons, and because no volcano could have burst out near Vienne without leaving some traces of its existence, Dr. Daubeny concluded that the bishops of Gaul alluded to earthquakes; especially as "the underground thunder, the opening of fissures in the ground, the bursting out of flames and gases, the projection of water and of stones, the smell of sulphur, the alarm evinced by the animals of the spot and neighbourhood, the elevation or depression of the land, noticed by Sidonius and by Avitus in the passages referred to by Sir Francis Palgrave, are all reported as concomitants of the great earthquakes which have occurred in more recent times." Geologists have since accepted this conclusion as the correct one, in opposition to what I may call the theological position.

There was, however, a volcanic region which had not been visited by any English geologist, and which had not been described, viz., the basin of Montbrison, through which the Loire flows. Of this Mr. Scrope says in his work (2nd Ed., p. 28), "a further examination of this basin seems very desirable;" now as this district lies about half way in a direct line between the "Puys," about Clermont Ferrand and Vienne, it occurred to Dr. Daubeny that the disturbances spoken of as in the neighbourhood of Vienne, might have taken place around Montbrison, and accordingly in the autumn of 1866 he visited that locality, and I had the honour of accompanying him on the occasion. We examined carefully the volcanic hills of the neighbourhood, and could find no trace of recent volcanic eruptions; in his own description of this expedition published in the *Quarterly Journal of Science* for January 1867, and republished in his "Miscellanies" (vol. i. p. 74), just before his death, he says:—

"I am now prepared to say that, without pretending to have surveyed the entire district, I saw enough to convince me that no volcanic disturbance which had occurred within this area at so late a period as that alluded to could have escaped our notice, and that every indication of igneous action which presents itself throughout the country, bears marks of a much greater antiquity.

"Thus much, at least, I can venture to affirm, namely, that neither, craters, streams of lava, scoriæ, nor even cellular trap, are to be met with anywhere within the limits of this district. On the contrary, the only igneous rocks which came under our observation consisted of a compact basalt, containing nests of olivine, a material which could only have been elaborated by the aid of great pressure, and under a different configuration of the surface from that now existing."

The Doctor therefore reiterated his statement that "the lively picture drawn by Sidonius" should not "be regarded in any other light than as the offspring of a lively imagination, dwelling upon reports which had reached the author with respect to some fearful earthquake which may have occurred in the neighbourhood of Vienne."*

I will conclude by advising those who wish to study volcanic phenomena to go to Auvergne, they can do so at almost any time of the year, mid-winter, when it is far too cold for comfort, being the exception; they will there see results of volcanic action far more varied and instructive than at Vesuvius or even at Etna, and they will also be able to study the effects of denudation on a gigantic scale. Few geologists seem to appreciate the fact that within 24 hours of London is one of the largest, richest, and most beautiful of the volcanic countries in Europe.

W. H. Corfield

PHOTOGRAPHY AS AN AID TO SCIENCE

THE applications made of photography now-a-days are as various as they are numerous. Irrespective of the ordinary every-day uses to which the art is put in reproducing scenes and objects, or pandering to human vanity, there are, as we know, numberless ways in which it is constantly being employed as a faithful handmaiden to science. To the chemist, the surgeon, the engineer, and others, its aid is frequently of considerable importance, while to the astronomer and physicist the assistance it renders is at times indispensable. The accuracy and fidelity with which the pencil of light performs its functions, combined with the facility with which such reliable records are obtained, make photography indeed one of the greatest boons at the disposal of scientific men.

Let us take, for example, the solar records which are daily secured at the Kew Observatory. These photographs of the sun's disc, taken whenever practicable at a certain fixed period in the day, are often of considerable value, and form illustrations, as it were, of other scientific observations made at the same time. A series of prints of this kind, secured day after day, afford, in truth, a most interesting and instructive lesson to the student of astronomy, for the characteristics exhibited by the various photographs may serve as a corroboration, or otherwise, of scientific theories based upon other data and results. The nature and luminosity of the markings, or spots, upon the disc are rendered with unerring fidelity, and the way in which these are continually modified in shape and intensity, as likewise the rapidity with which they are seen to travel across from the east to west limb of the sun, to reappear again some twelve days afterwards upon the eastern edge, is all clearly and distinctly shown.

Again, another interesting application of photography to astronomic purposes is to be found in the reproduction of the stars as recently undertaken by Prof. Rutherford. In this instance the objects to be secured are so minute that special precautions are necessary in depicting them upon the sensitive film, so that their impressions may be distinguishable from accidental specks in the collodion plate. To prevent any such chance of mistake, Prof. Rutherford secures a double image of each luminary, the moving telescope to which the miniature camera is attached being halted for a short time (half a minute) between a first and second exposure of the plate, so that each star is represented by a double speck, so to speak, upon the negative, and is clearly to be distinguished therefore, from any accidental defect in the film; moreover, by stopping the telescope again after the period necessary for the second exposure, the professor is enabled to demonstrate the direction in which the stars are moving, for the brightest of them produce a tiny streak of light during the time that the camera remains perfectly still. A map or plan of the heavens is in this way secured, very slight and delicate in its nature, it is true, but yet one upon which implicit reliance can be placed when undertaking astronomical measurements. To those more particularly interested in operations of this kind, we may mention that a total exposure of six minutes sufficed for the depiction of these heavenly bodies in the camera.

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* Those who wish for further information will find the whole subject discussed in Dr. Daubeny's classical work on Volcanoes, and in his papers in the *Quarterly Journal of Science* for April 1866 and January 1867.

Turning to another branch of the subject—micro-photography—we find the camera used for several purposes as important almost as those to which we have just referred. In the study of medicine, for instance, and the many sections of Natural History, photography lends a helping hand, so firm and true that we are at once guided to our destination. The large, clearly-defined diagrams of microscopic objects and medical preparations, which we are wont to see at many schools and colleges, cannot be prized too highly, forming as they do the best and most reliable proofs in support of facts and data, and being indeed of value alike to the professor as the student. And perhaps while treating on this particular subject, we may be allowed to refer also to the use made of the micro-camera during the siege of Paris for conveying news from and to that city. We have all heard how batches of private letters and whole sheets of newspapers have been reduced by means of photography to within the most insignificant limits, and produced upon a transparent pellicle, of which a pigeon might without inconvenience carry several under its tail, and how these precious films, on arrival at their destination, were forthwith placed in an enlarging apparatus or under a microscope, to be amplified to their original dimensions. Paris, it is said, contained upwards of a thousand pigeons qualified to act as messengers, and when it is asserted that on one occasion one of these birds arrived at Tours with several thousands of private messages and despatches, we ought by no means to be surprised that the communications between the French metropolis and the provinces were so numerous and frequent.

In the chemical, physical, and meteorological sciences, and even in that of war, photography aids in many ways, and thus helps to the advancement and progress of our knowledge of these matters. But the art, or art-science, as we may call it, has in several instances done something more than render yeoman's service to higher attainments; it has also been the means of discovering phenomena which could by no other means have been ascertained. In illustration of this may be mentioned the recent researches of Dr. Ozanam, undertaken for the purpose of defining the character of the pulsations of the heart; an investigation which has brought to light facts of considerable physiological importance. The instrument used by Dr. Ozanam was a thin India-rubber reservoir of mercury, having a glass tube attached, in which the quicksilver mounted to a certain height; the reservoir, on being placed in the vicinity of the patient's heart, was influenced by the beating of the latter, and the rise and fall of the mercury in the tube was thus made to indicate the ebb and flow of the blood, precisely in the same manner as a barometer registers the variations of the atmosphere. Behind the tube was arranged, by means of clockwork, a moveable strip of sensitive paper, or other suitable material, and this, as it ran along, was impressed by light, and received, in the form of an undulating line, a register of the fluctuations of the mercury column. The sensitive film passed along at the uniform rate of a centimetre per second, so that, presuming there to be one pulsation in that period of time, the wavy line representing a single beat would occupy the space of one centimetre. Of course, by enlarging the result to ten or twenty diameters, it was then, as may be supposed, easy to see what had taken place during the hundredth or thousandth part of a second, or beat, and the knowledge thus acquired, Dr. Ozanam believes, will be highly useful in preparing the diagnosis of a patient. One fact, of itself very important, has already been discovered by the aid of this ingenious instrument, viz., that not only as Dr. Maurey had before asserted, there exists dicrotism, or a double beat, in the normal pulse, but that the pulsation is even triple and quadruple in its action. The photographic line showed indeed that the column of mercury (representing of course, the blood in the arteries) bounded with one leap to the top of the scale and then descended again to its original level by three or four successive falls. Four descriptions of dicrotism have in this way been proved to exist, the fall of the pulse sometimes taking place in successive horizontal lines and sometimes in ascendant lines, the column reascending two or three times before falling altogether.

Another instance of scientific discovery by aid of photography is afforded in the observations of the spectrum by means of the camera. In Rutherford's picture of the solar spectrum obtained in this manner, there are many portions and lines shown (the ultra-violet for instance) which, while imperceptible to the retina of the eye, impress themselves very distinctly upon the sensitive film; and thus the presence of phenomena is proved of which but little was previously known. Of course the eye again descries certain lines, the yellow ones, which are without action upon the negative plate, and are not, therefore, recorded in the photograph, and thus it is only by carefully noting the results of both methods of observation that a true reproduction of the spectrum is obtainable. As a fact, we may mention, that single lines which are but faintly rendered in the Angström and Kirchhoff tables have been recorded by photography as well-marked double lines, while in some instances actually Rutherford shows indications by means of the camera of which there appears no vestige whatever in the records of other scientific men. In certain spectroscopic observations therefore, where special reliance is required to be placed upon the results, not only must an ocular observation be made, but the photographer's evidence must also be taken before any conclusions can be drawn from the aspect of the spectrum.

And before concluding we must not forget to refer to a still more recent instance in which photography has befriended the scientific investigator; we allude to the successful, although perhaps somewhat imperfect attempt which has been made by Prof. Young, to photograph the protuberances of the sun in ordinary daylight. A distinct reproduction of some of the double-headed prominences on the sun's limb has thus been obtained by the Professor; and although as a picture or mathematical record the impression may be of little value, still there is every reason to believe, now that the possibility of the operation is known, that with better and more suitable apparatus an exceedingly valuable and reliable record may be secured. Prof. Young employed for the purpose a spectroscope containing seven prisms, fitted to a telescope of 6½ inch apperture after the eyepiece of the same had been removed; the miniature camera, with the sensitive plate, was attached to the end of the spectroscope, the eyepiece of which acted in the capacity of a photographic lens, and projected the image on the collodion film. The exposure was necessarily a long one, amounting to three minutes and a half, and for this reason, as likewise on account of the unsteadiness of the air and the mal-adjustment of the polar axis of the equatorial, causing the image to shift its place slightly, the details of the image were somewhat blurred and destroyed. Moreover the eyepiece of the spectroscope was unsuitable for photographic purposes, and only in the centre yielded a true reproduction of the lines free from any distortion. A larger telescope will be required to secure a more defined image, and then, if more strict attention is paid to the clock-work arrangements and to the chemical manipulations, we may anticipate that a really valuable and important result will be obtained by this novel mode of observation.

H. BADEN PRITCHARD

AN EXPERIMENT TO ILLUSTRATE THE INDUCTION ON ITSELF OF AN ELECTRIC CURRENT

IT is well known that the sudden development of a current in a conductor is opposed by an influence analogous to the *inertia* of ordinary matter. A powerful movement of electricity cannot be suddenly produced; neither can it be suddenly stopped. One consequence is that a periodic interruption of a circuit in which a constant electromotive force acts is sufficient, when the self-induction is great, to stop all sensible current, even although the interruptions themselves may be of very short duration. Before any copious flow can be produced the circuit is broken, and the work has to be begun over again. Whether in any particular case the influence of self-induction is paramount, or not, will depend also on the *resistance* of the circuit, and on the rapidity of the intermittence. The magnitudes which really come into direct comparison are the interval between the breaks, and the time which would elapse while a current generated in the circuit, and then left to itself, falls to a specific fraction (such as one-half) of its original magnitude. In ordinary cases the duration of transient currents is but a small part of a second of time, so that, in order to bring out the effects of self-induction, the breaks must recur with considerable rapidity.

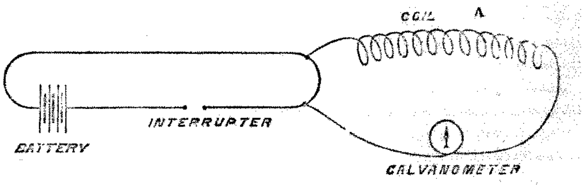
There is, however, one remarkable exception to the general rule, which occurs when, alongside of the principal coil to which the sluggishness is due, there exists an independent course along which the electricity can circulate. For instance, suppose that a coil with two wires, such as is often used for electro-magnets, is so arranged that one wire is included in the principal circuit, while the ends of the others are joined. The effect of the second circuit is then to neutralise the self-induction of the first, and so to increase largely the current that passes through it. Let us trace the progress of the phenomenon; supposing that the first circuit has been closed for a sufficient time to allow of the development of the full current which can be excited by the actual electromotive force.

The moment the rupture is complete, the current in the first wire must stop, but another of the same magnitude and direction is at once developed in the neighbouring circuit. In fact, in virtue of its *inertia*, the electrical motion tends to continue with as little change as possible, a result which is attained in great degree by the formation of the second current to fill the place of the first. In a short time the induced current would diminish and become insensible under the operation of resistance (analogous to ordinary friction); but we are supposing that before this takes place to any considerable extent the contact is renewed, and the electromotive force again begins, in the first circuit, to push the electricity on. It is now that the peculiarity of the arrangement manifests itself. The current instantly transfers itself back again to the first circuit, which thus, without any delay, has the advantage of the full current which the electromotive force can sustain. If it had not been for the second circuit and its current, the development in the first would only have been gradual, and by supposition so slow that it would be checked by another interruption before any considerable progress could be made. In short, the self-induction of the principal circuit is virtually destroyed.*

In my experiment the principal circuit consisted of a Smee cell and one wire of a coil belonging to a large electro-magnet, and which I may call A. The interrupter was a tuning-fork, arranged after Helmholtz, and set into regular vibrations of about 128 per second by an independent current and battery. The fork itself was forged by the village blacksmith, and the whole affair was home-made. Across one prong was placed a sort of rider of copper wire, dipping on either side into a mercury cup, and so arranged that during the vibration its ends should enter and leave the mercury, thereby establishing and interrupting the continuity of the circuit. The current was measured by means of a short wire galvanometer whose electrodes were connected with two neighbouring points of the circuit in such a manner that a small but constant proportion of the entire current passed through the instrument. The second wire of the coil, A_2, which is similar to the first and *put on with it*, formed the second circuit, when its ends were joined by a short wire. In order to increase at pleasure the effects of induction, iron wires or rods of about a quarter of an inch in diameter were provided, whose insertion in the coil materially increased the decisiveness of the result.

In the first place, the deflection produced on the galvanometer when the circuit was permanently completed was 58°, which fell to 39° when the interrupter was at work, the circuit of A_2 being open, and without iron. On closing A_2 the deflection rose to 46°. A_2 was again opened, and one iron wire introduced, which gave 30°. Two wires gave 25°, while the introduction of thirty reduced the deflection to 12°. Again closing A_2, the reading was 43°, raised to 44° only by the removal of the iron. It was clear that the second circuit almost secured the first from the influence of induction, which otherwise greatly reduced the electrical circulation. I may add that the arrangement was very efficient, the galvanometer needle remaining perfectly steady, so that the readings could be taken with ease and accuracy.

Another experiment made at the same time (about two years ago) may be noticed, if only for its contrast with the preceding. The coil A, being removed from the main circuit, was included in the branch with the galvanometer, as shown in the figure. Here neither the insertion of iron nor the closing of A_2 made any difference; the circuit containing the coil remaining always closed, whatever might be the condition of the other. In such circumstances the average current indicated by the galvanometer is independent of the self-induction of the coil, varying only with the resistance in the branch, and with the *average* difference of potential at the points of derivation.



J. W. STRUTT

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* Mathematicians familiar with the theory of electricity will follow this by putting the three induction co-efficients (in Maxwell's notation, L, M, N) equal, and the resistance of the second circuit, S, equal to zero.

SOME REMARKS ON THE HABITS OF SOME CEYLON ANIMALS, AND NOTES ON METHODS FOR KEEPING THEM ALIVE IN CONFINEMENT

AFTER my duties as member of the Eclipse Expedition were over, I spent some time in Ceylon collecting natural history specimens for the Oxford Museum. Besides preserving a large series of animals in solutions, I obtained through the kindness of my friend, Mr. G. H. K. Thwaites, F.R.S., of Peradeniya, whose kind hospitality I enjoyed, and to whom I am indebted for nearly all my best specimens and information concerning them, various living examples of the Ceylon fauna, and I kept them with more or less success in confinement. Some notes as to my experiences with regard to them may be of interest to the readers of NATURE.

Passerita mycterizans.—A brilliant emerald green tree snake, with horizontal pupil. I obtained a fine specimen about 4½ ft. long. I put him in a small wooden box with wire-gauze cover. As usual with this species, he would not feed, but drank frequently with great eagerness. Snakes often die in confinement for want of water. In the case of tropical snakes, it should not be forgotten that the water must be warmed as soon as colder latitudes are reached. This snake is now in the Regent's Park Zoological Gardens, and has now been two months without food. The keeper tells me that a specimen formerly in the Gardens lived six months without eating.[*]

Lyriocephalus scutatus.—These lizards live in large numbers in the Royal Botanic Gardens, Peradeniya, frequenting the moist shady banks of the Mahawillaganga, which bounds the Gardens in one direction. The animals sit all day on tree trunks, with their head uppermost. They can run very fast, but are easily caught, as is the case with most lizards, with a slip noose of palm fibre. They allow the noose to be put over their heads with the greatest ease. When first caught they are very fierce, and display their array of sharp teeth and bright scarlet mouths whenever a finger is moved near them, and they bite hard whenever they get a chance, holding like bull-dogs, as I often experienced to my cost when feeding them. I could not get them to feed themselves either whilst in Ceylon or on the voyage home. I therefore fed them by hand, opening their mouths forcibly by pulling on the pouch-skin and pushing worms down their throats. After a time they chewed and swallowed the worms readily on their being put in their mouths. I also poured water down their throats. I kept a stock of worms alive at the bottom of the cage in moist earth. Kelaart, in his Nat. Hist. of Ceylon, says that Lyriocephalus takes boiled rice freely in confinement; and Dr. Günther, "Indian Reports," p. 129, quotes him to that effect. I think this must be an entirely erroneous statement. I forced rice down the animals' throats, but they never seemed to relish it, and they never touched it of themselves whilst under my care, nor have they done so since they have been in the Regent's Park Gardens. The lizards are remarkable for their curious lyre-shaped heads, and the large knobs on the ends of the snouts of the adults. In young specimens the knob is very little developed. It is present in both sexes. The lizards change colour with great rapidity when excited, even whilst held in the hand. They were brought home in a tall wooden cage, by the advice of Mr. Thwaites so arranged that they could rest in their usual vertical position on some rough bark nailed on to the sides of the cage for the purpose. They clung on to these supports and the perforated zinc front during all the voyage home, and in their den in the reptile house in the Gardens they are always to be seen clinging to the branches head uppermost. The cage was kept on the voyage from Alexandria to Southampton in the engine-room of the P. and O. steamship *Mooltan*, as was also the box with the tree snake, and both were taken up to town from thence wrapped in a double blanket. Four of the lizards are now alive in the Regent's Park Gardens. They are still fed by hand, but one has been seen by the keeper to help himself to worms. Some time ago an attempt was made by Mr. Houldsworth to bring Lyriocephali home alive, but his specimens unluckily died in the Channel. Perhaps they were not hand-fed.

The ground in Ceylon swarms with burrowing reptiles of various kinds, and Mr. Thwaites's coolies used to dig me up as many as twenty in an afternoon. They all came from the moist river bank. Most abundant in the various lots I received was always *Nessia monodactyla*, a lizard which, having taken to underground habits, has become snake-like, and retains the merest rudiments of both its limbs. I kept all my underground reptiles in damp moss in joints of the gigantic bamboo. The *Nessias* move with great agility through the moss, and lived well in confinement. I hardly ever saw one in which the tail was not a reproduction.

Rhinophis Bythii.—These were not half so nimble as the *Nessias*, and seemed rather more delicate.

Typhlops braminus.—These small blind-worm-like snakes were not so abundant as the *Rhinophidæ* or *Nessias*, but there were generally one or two in each batch. They are very active, escape through the smallest crack, and are with difficulty retained in the hand. I kept the *Nessias*, *Rhinophidæ*, and *Typhlopidæ* together in a bamboo. I gave them earth-worms. The worms disappeared, but also did the *Typhlopidæ*. These could not have escaped; but were probably eaten by the *Rhinophidæ*.

Cæcilians.—Epicrium glutinosum.—I had only four specimens brought me of *Cæcilia*. They came from the same bank as the *Nessias*, &c. They move along the ground with a slow helpless wriggling motion, feeling their way with their remarkable exsertile labial tentacles. These tentacles are in constant motion, being alternately protruded and retracted. They are evidently the animals great stand-by in the special-sense way, and probably contain interesting terminal nervous organs. The Cæcilians have a certain amount of prehensile power in their tails. When placed in water they are very active, moving like eels, and seeming to enjoy themselves thoroughly. I kept them in moss in a bamboo, and put worms with them, which disappeared in no small quantities.

Mygale marmorata.—This spider, a full-sized one, ate clean up five large cockroaches in the first two nights I had it. A day later it cast up a large pellet, composed of the chitinous skins of its victims, just as a rapacious bird casts up feather pellets. It spun a small quantity of irregular web against the side of its cage. As far as I could observe, the spider did not feed again in the three weeks during which I kept it alive.

Scorpion hubrostonos.—These large black scorpions I could not get to feed at all, though I tried them with insects and raw meat.

Land Planarians.—Bipalium Diana, *B. Proserpina*, *B. Phœbe*, and *Rhynchodemus Nietneri.*—I obtained numbers of specimens of these huge Planarians, some as much as eight inches long. I tried several times to keep them alive amongst moist leaves, but unfortunately failed. They never lived longer than four or five days, then appeared to deliquesce into a slimy mass of corruption.[*] The slime of these Planarians is so tough that they can suspend themselves by a thread formed of it, and I have several times had them lower themselves thus from my hand to a table by means of a thread six or seven inches long. I have only seen *Bipalium Diana* and *Proserpina* do this, not *Rhynchodemus*. The cellar slug, *Limax agrestis*, uses a mucous thread for suspension in a similar manner (Binney's "Terrestrial and Air-breathing Molluscs of the United States," vol. ii. p. 39). When in motion, the Bipaliums throw out a series of short tentacular-like papillæ from the front edge of their semicircular anterior extremity. I had all the animals I have mentioned, except the Planarians, alive as far as Suez, but unfortunately they were all killed by the intense cold of the night journey across the desert to Alexandria, except the Lyriocephali and Passeritia, which I took in the carriage with me and kept warm. The train was so crowded that I was obliged to put the rest in the van. I especially grieved over the loss of the Cæcilians; they were especially well and healthy, and I feel certain I should have got them home alive had it not been for this mishap. I had hoped to be able to get them to breed, and to watch their development.

H. N. MOSELEY

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[*] Since the above was written this snake has not only swallowed a young bird, but has also attempted to swallow another snake of a different species confined in the same cage.

[*] Mr. Darwin had much better success with the South American Land Planarians.—"Darwin's Journal of Researches," 1860 Edition, p. 27.

ON THE CRYSTALLISATION OF SILVER, GOLD, AND OTHER METALS*

THERE are few chemical experiments so well known as the growth of the "lead tree" a specimen of which is on the table, together with a "silver tree" that is said to have been made by the late Professor Faraday. These carry our minds back to the time of the alchemists, who called the first "arbor Saturni," and the second "arbor Dianæ;" and they may be looked upon as the types of a large number of phenomena, in which the salt of one metal in solution is decomposed by some other metal. My assistant, Mr. Tribe, and myself have been lately examining these replacements, the metallic crystals which are thus produced, and the forces that act through the liquid.

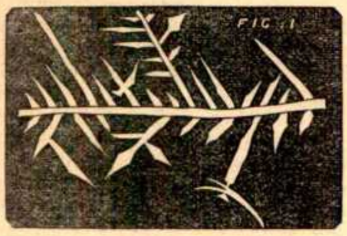
FIG. 1

Our more special attention has been given to the mutual action of copper and nitrate of silver. If these two substances be brought into contact by the intervention of water, there grow upon the red metal what may be well called "trees," and though the analogy between crystals and plants is a very superficial one, yet the resemblances of external form are striking enough, and a nomenclature drawn from the garden seems the most expressive.

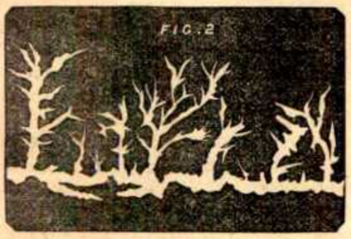
FIG. 2

It is very beautiful to watch the growth of these silver crystals round a piece of copper under the microscope; a blue glass underneath adds to the effect, and they are best seen when they reflect a strong light thrown upon them. They may also be thrown upon a screen as opaque objects, but the beauty and lustre of their surface is in this way lost.

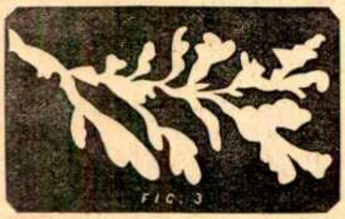
FIG. 3

The crystals of silver thus produced differ both in colour and form, according to the strength of the solution. If it be very weak, say one per cent, the copper is fringed with black bushes of the metal, which, in growing, change their colour to white without any alteration of crystalline form that can be detected by a powerful microscope. A stronger solution gives white crystals from the commencement, which frequently assume the appearance of fern-leaves; while the growth from a still stronger liquid reminds us rather of a furze bush. If the nitrate of silver amount to 15 per cent., or thereabouts, there occurs a steady advance of brilliantly white moss; and if the solution be saturated, or nearly so, say 40 per cent., this moss is very sturdy, often ending in solid crystalline knobs, or strretching out into the liquid as an arborescent fringe.

In all these cases, however, when the solution in front of the growing crystals has been somewhat exhausted, certain prominent or well-circumstanced crystals seem to monopolise the power, and to push forward through the remaining portions of the liquid. This gives rise to beautiful branches which assume a variety of graceful forms, which it is hopeless to attempt to pourtray by diagrams, but of which the subjoined figures give some of the more characteristic outlines greatly magnified. The weak solutions produce feathery crystals somewhat as in Fig. 1, consisting of a straight central stem from which grow on either side crystalline rays that terminate in a sharp point, and frequently become themselves the centre stems of a similar crystalline structure. In the outlying growth of a moderately strong solution the apparent regularity of the crystalline form is lost; the main stem is built up of a confused mass of hexagonal plates, while the side branches are an agglomeration of minute pointed crystals turning in every direction, and producing such jagged outlines as are drawn in Fig. 2. In stronger solutions still the branches lose every appearance of straightness, and they are built up of hexagonal plates so studded with crystalline specks that the whole has the rounded appearance depicted in Fig. 3. The arborescent crystals that succeed the fringes from a saturated solution, are smaller in their foliage than the last, and end in little spherical or botryoidal knobs.

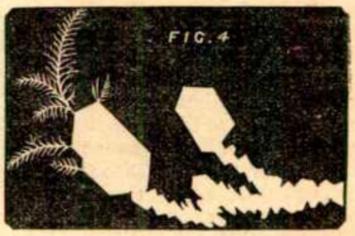
FIG. 4

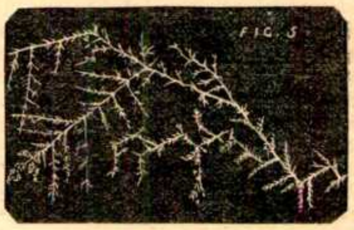
FIG. 5

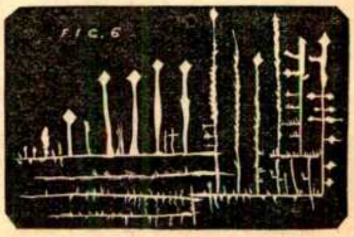
FIG. 6

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* Lecture delivered at the Royal Institution of Great Britain, February 16, 1872, by John Hall Gladstone, F.R.S.

Beside these various forms, there occur all kinds of crystalline combinations, as, for instance, the spray sketched in Fig. 4, where the rough branches have terminated each in a large hexagonal plate, and the flowing past of a weakened solution has afterwards caused the growth of delicate fern-leaves. Often, too, a large expansion will take place in every direction, though joined to the parent stem by an almost invisible thread ; or from the point of a long crystal there will branch out to right and left crescent-shaped structures, a process the commencement of which is seen in one of the side rays of Fig. 1. The last traces of silver in the liquid will frequently give rise to delicate crystalline filaments wandering over the surface of the glass, as in Fig. 5.

If a piece of zinc be placed in a solution of neutral terchloride of gold, containing 9 per cent. of salt, there is an immediate outgrowth of black gold, which speedily changes to an advancing mass of yellow or perhaps lilac metal in lichen-like forms, from which proceed beautiful fringes of yellow or black, ending generally in such arborescent forms as are represented in Fig. 7. As these branches push into the yellow liquid, it becomes colourless even in advance of their points, and it frequently happens that yellow crystals of some salt shoot out in front of the crystallising metal, which follows them and builds up its advancing fronds at their expense. This is shown in the figure. The gold will generally shoot its yellow branches rapidly round the margin of the drop. Such a running branch has been seen to stop on touching at one point a loose piece of gold, which immediately in its turn became active, and commenced to sprout on its farther side. Copper salts give round nodules, which have no crystalline appearance when deposited from moderately weak solutions, but a very strong solution of the chloride—about 40 per cent.—yields with zinc first a black thick growth, then arborescent fringes of red metal, terminating in crystals of very appreciable size.

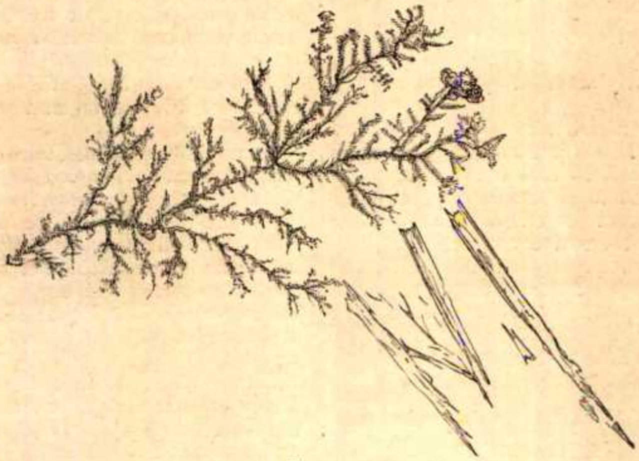

FIG 7

The fringes referred to in the case of these three metals are still more characteristically developed by bismuth. When a solution of terchloride of bismuth acts on zinc, there is an immediate outgrowth of black fringes, such as Fig. 8, where they are seen on an illuminated field. These, as they advance, become more and more arborescent, and as the crystalline character develops itself more they change from black to grey. Sometimes bismuth presents itself in botryoidal masses, but the tendency to form these fringes is very strong.

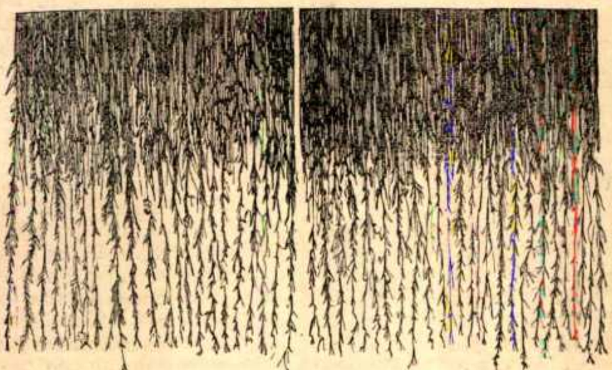

FIG. 8

Chloride of antimony with zinc also gives these black fringes. Lead salts yield crystals resembling those of silver; but leaves of irregular hexagonal plates prevail, and often grow to a large size. A solution of acetate of thallium, containing 20 per cent. of salt, quickly gives a beautiful forest of thorny crystals. Sulphate of cadmium gives rise to a small leaf-like growth on zinc ; but a strong solution of chloride produces an appearance of sticks covered with small spines or knobs. The new metal indium is thrown down upon zinc in the form of thick white crystals. The deposition is promoted by touching the zinc with a piece of iron.

Tin gives beautiful results. If zinc be placed in a solution of stannous chloride, it is quickly surrounded with an outgrowth of prolonged octohedra, and as these advance into the liquid it is easy to observe that the additions of new metal commence at the apex, and that the wave of chemical change proceeds down the lateral edge, occupying some seconds of time in depositing the new layer of material. Frequently, also, there is a luxuriant growth of large flat leaflets, or of symmetrical structures resembling fern-leaves, but with the fronds arranged at right angles, or combinations of these with octohedra, as shown in Fig. 6. These fern-leaves often begin of a dull grey colour; but, as they advance, suddenly change to a brilliant white.

The particular form of these crystalline growths depends, therefore, primarily, on the specific character of the metal; but this is greatly modified by the strength of the solution.

The forms assumed by native metals resemble those produced by this process of substitution. In some cases, indeed, it seems almost certain that the deposition of these minerals was effected in the same way, as, for instance, the silver which occurs sometimes in tufts, sometimes in large crystals, on the native copper of the Lake Superior district. Gold is frequently found in cubes more or less rolled, but the leaf gold from Transylvania bears a striking likeness to the crystals that form in our laboratory experiments. Silver is often found native as twisted hairs or wires of metal—a form that never occurs in the decomposition of its nitrate by copper, but which can be artificially produced in another way.

There has been noticed a singular tendency in old silver ornaments and coins to become crystalline and friable. Here is an ancient fibula from the island of Cyprus, supposed to be at least 1,500 years old, which, through the greater portion of its substance, presents a fracture something like that of cast-iron, and its specific gravity has been reduced in round numbers from 10 to 9. It contains a little copper. This property of certain metals or their alloys to change in condition and volume, is worthy the attention of those whose duty it is to make our standards. Experiments should be instituted for the purpose of learning what metals or combinations of metals are least subject to this secular change.

These metallic crystals are Nature's first attempt at building. This material is the simplest possible—in fact, what chemists look upon as elementary. But how is the building carried on? What are the tools employed? Where are the bearers of burdens that bring the prepared pieces and lay them together according to the plan of the Great Architect? We must try to imagine what is taking place in the transparent solution. The silver, of course, existed at first in combination with the nitric element, and for every particle of silver deposited on the growing tree, an equivalent particle of copper is dissolved from the surface of the plate. The nitric element never ceases to be in combination with a metal, but is transferred from the one metal to the other. On the "Polarisation Theory," the positive and negative elements of the salt constantly change places and enter into fresh combinations, one consequence of which would be a gradual passage of the nitric element from the growing silver to the copper plate. This actually takes place, and there is a diminution of the salt at the ends of the silver branches, giving rise to an upward current, and a condensation of nitrate of copper against the copper plate, which gives rise to a strong downward current. These two currents are seen in every reaction of this nature. In the case of silver and copper, however, it has been proved that the crowding of the salt towards the copper plate is more rapid than would follow from the usual polarisation theory. The instrument employed for determining this point was a divided cell in which two plates, one of silver and the other of copper, connected together by a wire, are immersed each in a solution of its own nitrate, contained in each division of the cell, and separated from one another merely by parchment paper. The crystals of silver deposited on the silver plate in this experiment are very brilliant.

There are other indications of the liquid being put into a special condition by the presence of the two metals which touch one another. Thus zinc alone is incapable of decomposing pure water; but if copper or platinum be deposited on the zinc in such a manner that the water can have free access to the junction of the two metals, a decomposition is effected; oxide of zinc is formed, and hydrogen gas is evolved. At the ordinary temperature the bubbles of gas rise slowly through the liquid, but if the whole be placed in a flask and heated pure hydrogen is given off in large quantity. We have also found that iron or lead similarly brought into intimate union with a more electro-negative metal, and well washed, will decompose pure water.

As might be expected, the action of magnesium on water may be greatly enhanced by this method; and a pretty and instructive experiment may be made by placing a coil of magnesium in pure water at the ordinary temperature, when there will be scarcely any effect visible, and then adding a solution of sulphate of copper. The magnesium is instantly covered with a growth of the other metal, and at the same time the liquid seems to boil with the rapid evolution of hydrogen bubbles from the decomposed water.

When, however, the force of the two metals in contact has to traverse a layer of water, the resistance offered by the fluid prevents its decomposition. This must also be an important element in the decomposition of a metallic salt dissolved in water, and in fact we have found that the addition of some neutral salt, such as nitrate of potassium, increases the action—apparently by diminishing the resistance of the liquid. If, too, we increase the quantity of the dissolved metallic salt, we get more than a proportional increase of deposited metal. Thus, in an experiment made with the different strengths of nitrate of silver on the table, the following results were obtained in ten minutes, all the circumstances being the same except the strength of the solution:—

1 per cent. solution dissolved ·025 grm. copper.
2 " " " ·078 "
4 " " " ·224 "

In fact it has been found that in solutions not exceeding 5 per cent., twice the amount of nitrate of silver dissolved in water gives three times the amount of chemical action; and this is true with other metals also in weak solution. It may be that this is not the precise expression of a physical law, but it agrees at least very closely with the results of experiment.

The power arising from this action of two metals on a binary liquid may be carried to a distance and produce similar decompositions there. This is ordinary electrolysis. Metals have often been crystallised from their solutions in this way, and Mr. Braham has made excellent preparations of crystalline silver, gold, copper, tin, platinum, &c., by using poles of the same metal as that intended to be deposited upon them. The forms thus obtained are precisely analogous to those produced by the simple immersion of one metal into the soluble salt of another, and illustrate still further the essential unity of the force that originates the two classes of phenomena.

NOTES

THE speech of the Prime Minister at the meeting held last week in support of the fund to pay off the debt at King's College was a striking comment on some recent utterances of members of the Government to the effect that Science is well able to support itself, and needs no assistance from the State. The claims of this fund on public assistance were earnestly and eloquently urged on the ground that King's College was an institution founded to promote the union between Science and Religion, and therefore the happiness of mankind. In the course of the meeting it was mentioned that the works connected with the Thames Embankment have entirely destroyed the dining-hall of the College, entailing a loss of 1,400*l.* or 1,500*l.*, for which no legal redress can be obtained; and this although noble dukes receive compensation to the extent of thousands of pounds for the injury inflicted on the privacy of their gardens by the same works.

ON the occasion of the annual conferring of degrees by the University of London on the 15th inst., Mr. Lowe, who is a member of the Senate of the University and its representative in Parliament, expressed an opinion in favour of making Greek an optional subject at the Matriculation Examination, to be substituted either by an additional modern language, or by some branch of natural or physical science. The proposed change has now been before the body of graduates for the last two years, but has not yet received the sanction of Convocation. At the last meeting, on the 14th inst., the subject was referred back to the Annual Committee of Convocation for further consideration. In the course of the same speech, Mr. Lowe urged benefactors of education to leave money for the endowment of scholarships at the University, rather than of professorial chairs, on the ground that the pay of lecturers ought to be in proportion to the amount of instruction they give, *i.e.* to the number of their pupils. Mr. Lowe appears, however, to forget that quality, as well as quantity, is required in teaching, and that this quality can only be secured by original work, to devote himself to which the professor must be to a certain extent independent of the emoluments derived from actual teaching.

THE special correspondent of the *Daily News*, writing from Zanzibar under date of April 19, states that no letters had been received there from either Dr. Livingstone or Mr. Stanley up to that date, and that war was still going on in Unyanyembe between the Arabs and the natives. The terrible hurricane of the 13th had wrecked every vessel in the harbour of Zanzibar except the *Abydos*; the harbour was then one mass of wreck, and European residents expect that a famine may be the result, the cocoa-nut and clove-trees, the chief products of the island, having been almost entirely destroyed, and that trade will be brought to a standstill for some time. Mr. Horace Waller forwards to the *Times* a letter just received from Mr. Oswell Livingstone, in which he speaks of the expedition being detained in Zanzibar by the hurricane up to April 20; and Prof. Corfield sends one to the same journal from Dr. James Christie, physician to the Sultan of Zanzibar, who says that he believes Dr. Livingstone is alive and well, and that Mr. Stanley has relieved him at Ujiji, and that he would not be surprised to meet them both in Zanzibar any day.

WE are very glad to be able to announce that the United States Senate has unanimously passed a bill, appropriating 50,000 dols. to meet the expenses of the observations upon the transit of Venus in 1874, on the part of the National Observatory in Washington. The bill has been introduced into the Lower House, and will doubtless soon become a law.

PROF. PALMIERI of Naples has received an address, signed by seventy citizens of Rome, expressing their admiration of his character and conduct, and congratulating him on the success of his efforts to save numerous victims from destruction in the late terrible eruption of Vesuvius. A communication from Rome states that he is to be nominated a senator of the Kingdom of Italy.

THE Royal Danish Society of Copenhagen offers the following prizes for competition during the ensuing year:—For a description of the spectra of the planets Venus, Mars, Jupiter, Saturn, and Uranus, which shall determine the question relating to the position and special nature of the principal lines, accompanied by a critical comparison of the results previously obtained by Huggins, Secchi, Vogel, and, as respects Jupiter, by Le Sueur, of Melbourne—the gold medal of the Society, together with a sum of money of fifty Danish ducats (450 frs.). For a thorough research into the organic reproduction of one of the groups of setiferous Annelidæ, the Naididæ, Scyllidæ, or Serpulidæ, especially with relation to the question whether the same individuals are both gemmiferous and sexual, or whether the sexual and organic modes of reproduction are strictly separated in different individuals and generations, accompanied by the necessary drawings—the gold medal of the Society. The essays may be written in Latin, French, English, German, Swedish, or Danish; and must be addressed before the end of October 1873 to the secretary of the society, M. J. J. S. Steenstrup.

SOME little time ago we noticed in our columns an effort that was being made to raise a memorial to the late Sir R. Rede, the founder of the Rede Lecture at Cambridge, and we are glad to be able to state that in consequence several fresh subscribers have added their names to the list, so that the fund now amounts to 80*l.*, which is exactly one half of the sum required for the window that it is proposed to fill with stained glass in the church where the remains of this far-seeing friend to knowledge rest. The Rede Lecture is to be delivered in the Senate House, Cambridge, on Friday, 24th inst., by Mr. Edward H. Freeman, D.C.L., on the Unity of History, and it is hoped that members of the University, as well as others interested in the promotion of science, will contribute towards the completion of the memorial of which a partial commencement has been made by inserting a portion of the glass. The Rev. Professor Selwyn, D.D., has kindly consented to receive subscriptions at Mortlock's Bank, Cambridge, "Rede Memorial Fund;" and Mr. Norman Lockyer, F.R.S., the late Rede lecturer, in London, at 6, Old Palace Yard, Westminster. The names of the subscribers will be found in our advertising columns.

WE have to record with great satisfaction the appointment of Mr. M. J. Barrington Ward, B.A., F.L.S., to be one of Her Majesty's Inspectors of Schools. Mr. Barrington Ward was a scholar of Magdalene Hall, and first of his year in Natural Science at Oxford. He has recently occupied the post of Science-master at Clifton College.

THE post of teacher of Chemistry at Clifton College will be vacant at the end of the present term.

AMONG recent deaths of persons eminent in Science, *Harper's Weekly* mentions that of Dr. Samuel Jackson, former Professor of the Institutes of Medicine of the University of Pennsylvania, which took place at Philadelphia on the 5th of April last. Dr. Jackson was born in Philadelphia in 1787, and was therefore eighty-five years of age. He held the position of active professor for twenty-eight years, and retired in 1863. He was well known as a physician and surgeon of great eminence, and for a long time occupied a leading position. He was also an author of some celebrity, and popular as a lecturer. His most important work was "The Principles of Medicine," first published in 1832, and which has gone through numerous editions.

A COMMUNICATION was presented to the National Academy of Sciences at Washington, at its annual meeting, on April 16 last, from Prof. Agassiz, dated Monte Video, February 26. In this he expresses his gratification at finding evident traces of glacial action in the vicinity of Monte Video, as shown by the occurrence of phenomena which were quite satisfactory to his mind. He leaves the question undecided as to the origin of the erratic boulders found scattered over the surface, but hopes that his further investigation in the southern hemisphere will enable him to supply the necessary data.

PROF. CORFIELD will commence a course of twelve lectures on Hygiene and Public Health at University College, London, on Tuesday next, at 12 o'clock.

AT the last exhibition of the Royal Horticultural Society an interesting feature was introduced in the shape of prizes offered for the best dinner-table decorations. The competing tables were laid out in two tents in the gardens, and were an object of great attraction. Many of them were remarkable for the taste displayed in the arrragements.

A FUND was started some time ago to promote the investigation of the Wealden formation of Sussex, especially with reference to the question of the supposed underlying coal strata. Large subscriptions have already been received for this purpose; the Duke of Devonshire has promised 250*l.*, and Lord Leconfield 100*l.* It is believed by many geologists that coal in large quantities may occur in strata beneath some of the longitudinal folds of the Wealden denudation, and form a continuation of the Belgian coal beds.

THE Ethnological Collection of the Oxford Museum has lately been enriched by the presentation to it of some North Australian spears, by Captain Halpin, of the *Great Eastern.* Captain Halpin, in his capacity as chief of the Expedition which laid the cable to Australia last year, visited Port Darwin, and there obtained these weapons. One of the spears has a stone head. The stone of which it is formed is a sort of shale. The head is large, very sharp, and of the unground type. The Oxford Ethnological series is, under the hands of Prof. Rolleston, becoming a very valuable and complete one indeed, and those who are in possession of authentic crania or weapons of interest from an ethnological view, cannot do better than contribute to it. Canon Greenwell has lately presented the whole of his valuable collection.

THE *American Naturalist* is responsible for the following story, which we could hardly have credited except on such authority:—"Central Park Museum.—Destruction of Mr. Hawkins' Restorations.—A *Times* reporter called yesterday on Mr. B. Waterhouse Hawkins in order to ascertain the truth of the allegations made in a communication which appeared in yesterday's *Times* in reference to the destruction of his restorations in the Central Park Museum. Mr. Hawkins stated that all he had done during twenty-one months to restore the skeletons of the extinct animals of America (of the *Hadrosaurus*, and the other gigantic animal, which was thirty-nine feet long), was destroyed by order of Mr. Henry Hilton, on the 3rd of May last, with sledge-hammer, and carted away to Mount St. Vincent, where the remains were buried several feet below the surface. The preparatory sketches of other animals, including a mammoth and a mastodon, and the moulds and sketch models, were destroyed. Mr. Hilton did this, said Mr. Hawkins, out of ignorance, just as he had a coat of white paint put on the skeleton of a whale which Mr. Peter Cooper had presented to the Museum, and just as he had a bronze statue painted white. Mr. Hilton told the celebrated naturalist who had come from England to undertake the work, that he should not bother himself with 'dead animals,' that there was plenty to do among the living. When the skeletons were dug up again, by order of Col. Stebbins, they were found broken in thousands of pieces. Prof. Henry, of the Smithsonian Institution, when he heard of this piece of barbarism, would not believe it. 'Why,' he exclaimed, 'I would have paid them a good price for it.' Mr. Hilton, however, preferred to destroy the work of the naturalist which had cost the City at least twelve thousand dollars." The proceeding appears to have met with universal reprobation from the Americans. The *Naturalist* refers by way of contrast to the record in our columns of the preservation of the great megalithic monument at Avebury, through the public spirit of Sir John Lubbock.

THE poorest flora in the world is probably that of the island of St. Paul in the Indian Ocean, an account of which appears in the "Verhandlungen der k. k. zoologisch-botanischen Gesellschaft in Wien" for 1871. It consists, as far as flowering plants are concerned, of six grasses, a sedge, a *Plantago*, and a *Sagina*. Of these the two latter only are undescribed species, and all the remainder have probably been introduced.

THE Acclimatisation Society of Auckland, New Zealand, has printed its fifth Annual Report. The efforts of the Society during the preceding year have been chiefly devoted to the introduction of insectivorous birds from England and Australia, and of fresh-water fish. In both these respects the Society has met with greater success than in previous years, and appears to be performing work of great service to the colony. The gardens of the Society have also been considerably enlarged.

SEVEN parts are now published of the new edition of Griffith and Henfrey's Micrographic Dictionary, edited by Dr. Griffith, the Rev. M. J. Berkeley, and Prof. T. Rupert Jones, of which we have already noticed the issue of the first number. They bring down the articles as far as *Confervoideæ*.

DR. CHARLES C. ABBOTT has reprinted his elaborate and profusely-illustrated article on the "Stone Age of New Jersey," which has been running through several numbers of the *American Naturalist.*

THE Danish war steamer *Fylla* has been ordered by the Danish Government to take soundings and survey landing-places for the submarine telegraph line intended to connect Scotland with Canada *viâ* the Faroe Islands. It is to be hoped that these soundings will be accompanied by dredgings, and that they will be carefully made, as much of Dr. Carpenter's early researches in deep-sea dredging, which excited so much attention at the time and since, were made in the neighbourhood of the Faroe Islands.

THE earthquake in California on March 26 appears to have been felt over a very large area, and in some places to have been very severe. At Leone Pine, in the country north of the Mojane river, twenty-three people were killed, and thirty wounded; fifty large houses were destroyed, and the town is in ruins. Similarly Camp Independence, Inigo county, is in complete ruins, the earthquake having been most severe in that region. Large fissures are reported, miles in length, and 50 to 200 feet wide, and twenty feet deep, opened along the eastern base of the Sierra Nevada, near Big Pine Camp. At other places in the vicinity, the ground is heaved up in great ridges; large springs have stopped running, and others have broken out. Heavy snow slides have occurred in the Sierras, and large rocks rolled down the mountain side, blocking up the road. The shocks lasted at intervals from 2.20 to 6.30 A.M. Many people at Camp Independence were hurt, but none were killed. The shock was probably the heaviest south-eastward towards Arizona, in the desert country which has hardly any population.

THE April number of the *American Journal of Science* contains an account by Prof. Marsh, the indefatigable palæontologist, of his discovery of a new species of *Hadrosaurus*, a giant lizard; one of which, found in New Jersey, from its enormous size, constitutes one of the chief attractions of the Academy of Sciences of Philadelphia, where it is deposited. The present animal is scarcely one-third the size of the New Jersey specimen. It was discovered near the Smoky Hill River, in Western Kansas, and is named *Hadrosaurus agilis*.

ON THE MINERAL CONSTITUENTS OF THE BREITENBACH METEORITE *

THE Siderolite of Breitenbach was acquired for the British Museum in the year 1863. It was found (in 1861) at Breitenbach in Bohemia, at a spot not very far distant from the Saxon frontier, or indeed from Rittersgrün, in Saxony, a place in which a very fine mass that bears a close resemblance to the Siderolite of Breitenbach, was almost contemporaneously found. A little way to the west of the centre of the line joining Rittersgrün and Breitenbach lies Steinbach, a village in the environs of Johanngeorgenstadt, near Schwartzenberg; and here in 1751 was also found a mixed meteoric mass in which, as in the two already mentioned, iron, sponge-like in its structure, encloses siliceous minerals that do not present a familiar aspect. The three meteorites are, in fact, so similar to one another and so dissimilar to any others in European collections, that there can be little doubt they belonged originally to the same meteoric fall.

Stromeyer † in the year 1825 examined a siderolite in which he found as much as 61·8 per cent. of silica. This remarkable result, together with the numbers of his analysis, he interpreted as indicating the presence of a magnesian trisilicate, probably meaning thereby a sesquisilicate (magnesium epideutosilicate). The specimen which he analysed he described as coming from Grimma, in Saxony. This specimen was, in fact, a portion of a mass preserved in the collection of the Duke of Gotha, and doubtless believed by Stromeyer to be a portion of a stone which was known to have fallen in the middle of the sixteenth century in a wood near Naunhof, in the neighbourhood of Grimma. Chladni ‡, however, held this view to be untenable, grounding his opinion on the completeness of the meteorite preserved at Gotha, both as regards its form and its crust, while he adds that the Naunhof mass must have been far too great to allow of its being transported, and, indeed, that it had never been rediscovered. It is in every way probable that the material Stromeyer really had taken to work upon was from a Saxon locality, and in fact a specimen from a fall, to which the Rittersgrün and Breitenbach siderolites belong. Breithaupt § believes the fall in question to have been the "*Eisenregen*" which occurred at Whitsuntide, 1164, in Saxony, when a mass of iron fell in the town of Meissen ‖.

An inspection of a polished surface of either of these masses reveals the iron in patches of irregular form, which exhibit the characteristic crystalline structure of meteoric irons when etched. The interspaces are partly filled by meteoric pyrites (troilite) in small patches, recognisable by its pinchbeck brown colour, the rest of the surface being occupied by a greenish and greyish-brown crystalline magma. It is of the ingredients of the last-mentioned portion of the meteorite that I shall first speak. On treating the whole mass with mercuric chloride at 100° for some hours the iron and the troilite are dissolved, and the magma before alluded to remains unattacked. But it has now lost its compound structure, and is found to consist of three substances: (1) highly crystalline, bright green, or else greenish-yellow grains; (2) rusty brown, sometimes nearly black, sometimes also nearly colourless grains of a mineral that presents crystalline features, but on which definite crystalline planes are of great rarity; and (3) crystalline grains of chromite.

The first of these three minerals proved to be a ferriferous enstatite, or bronzite; the second is a mineral which corresponds in all respects, except its crystalline form, with the tridymite of Prof. Vom Rath. In respect of their forms, however, it is difficult to suppose that the two minerals are identical.

Bronzite of the Breitenbach Siderolite

The specific gravity of this mineral is 3·238, that of the silicates in the Steinbach siderolite, as determined by Stromeyer, having been 3·276, and as estimated by Rumler 3·23. The hardness is 6.

The blackened aspect of some of the bronzite was due to a mere superficial coating of iron oxide, arising doubtless from the oxidation of a portion of the nickeliferous iron. It was invariably found that this film was easily removed by hydrogen chloride, leaving the bronzite of a bright green colour, and that the action of the acid on the mineral extended no further.

Two analyses of this mineral were made, the one by the hydrogen fluoride method of distillation[*], the other by fusion with mixed alkaline carbonates, and the results were as follow:—

	I.	II.	Mean.	Oxygen.
Silicic acid	56·101	56·002	56·051	29·89
Magnesium oxide .	30·215	31·479	30·847	12·34
Iron protoxide . .	13·583	13·295	13·439	2·97
	99·899	100·776	100·337	

These numbers correspond very closely with the formula $(Mg_{\frac{4}{5}}\ Fe_{\frac{1}{5}})\ Si\,O_3$.

Asmanite—a new mineral, being Silica crystallised in the Rhombic System, as a Constituent of the Breitenbach Siderolite.

The second mineral associated with the bronzite in this meteorite is free silica, possessing the lighter specific gravity presented by quartz after fusion, and crystallised in forms that belong to the orthorhombic system. To this mineral, which is distinct in its system and forms from the tridymite of Vom Rath, I propose to give the name Asmanite, A'sman being the Sanscrit term (corresponding to the Greek ἄκμων) for the thunderbolt of Indra. In bulk it forms about one-third of the mass of mixed siliceous minerals. The grains of this mineral are found mixed with those of the bronzite, after the iron, the troilite, and the chromite have been removed. They are very minute and much rounded, and, though entirely crystalline, they very rarely indeed present faces that offer any chance for a result with the goniometer; indeed out of the several thousands of these little grains comprised in some two grammes that were isolated of the mineral, it was only possible to find with a lens about a dozen specimens with sufficiently distinct crystallographic features; and of these only four or five proved to be available for examination and comparison. In several, however, the optic axes were plainly to be distinguished when properly examined with a Norrenberg's polarising microscope; and by this means the angles given by planes belonging to zones otherwise too incomplete for a reliable result were brought into comparison on different crystals.

The parametral ratios of Asmanite are

$$a : b : c = 1·7437 : 1·0000 : 3·3120.$$

The faces of the octaid forms are almost invariably rounded. Fair approximate measurements, however, of three of the tantozonal faces in one crystal were obtained.

That the mineral belongs to the rhombic and not to an unaxial system is emphatically evidenced, independently of the measurements, by its optical characters, as shown in its very distinct and widely separated optic axes. As has been said, the first mean line is the normal of the face 1 0 0, that to face 0 0 1 is the second mean line. The first mean line is parallel to the axis of least optical elasticity, so that the crystal is positive in its optical character. The apparent angle, as measured in air, of the optic axes was approximately determined as 107° to 107° 30'. The axes for the red rays are slightly more dispersed than those for the blue.

The crystalline grains which constitute this ingredient of the meteorite, when first obtained, are of a rusty brown and sometimes even black colour; treatment for a short time with dilute hydrogen chloride, however, entirely removes this iron stain and leaves the granules in a state of colourless purity, in which state they are readily distinguished from the grains of the accompanying bronzite.

The specific gravity of the mineral gave the number 2·245. Its hardness is 5·5.

Two analyses were made by different methods, and the results are given below.

I 0·3114 substance, distilled with pure hydrogen fluoride, gave 1·1136 gramme of potassium fluosilicate, 0·0035 gramme iron oxide, 0·0018 calcium oxide, and 0·0132 gramme magnesium phosphate.

These determinations denote the following percentages:—

Silicic acid	97·43
Iron oxide	1·124
Calcium oxide	0·578
Magnesium oxide	1·509
	100·641

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* Abridged from a paper contributed to the Philosophical Transactions by Prof. Maskelyne, F.R.S.

† Pogg. Ann. iv. p. 195

‡ Berg. und. Hütt. Zeitung, xxi. p. 322

§ Feuer-Meteore, pp. 326 and 212

‖ Feuer-Meteore, p. 198

[*] Philosophical Transactions, 1870, p. 189.

II. 0·2653 gramme of carefully selected substance, evaporated with an excess of ammonium fluoride, left 0·0021 gramme residue, chiefly iron oxide.

This determination denotes the following percentage composition:—

Silicic acid	[99 21]
Iron oxide, &c. . . .	0 79
	100·00

Besides the distinct cleavage parallel to the plane 0 0 1 already alluded to, and the other, less distinct, parallel to the planes of the form (1 0 1), there seem also to be divisional planes or, rather, surfaces along which the crystals break up with the greatest facility; even drying them on blotting-paper proving often sufficient to destroy the integrity of specimens that might otherwise seem to promise good results to the goniometer.

Partsch,* in his description of the Vienna Collection of Meteorites, identifies as a specimen of the Steinbach siderolite a fragment with a label, "Native iron, jagged and hackly, with quartz in grains and a yellow fluor-spar" (*gediegenes, zahnicht und zackicht gewachsenes Eisen mit kornichtem Quarz und gelblichtem Flussspath*).

Breithaupt†, in his paper describing the Rittersgrün Siderolite, makes its chief silicate to be peridot. It is doubtless bronzite. In addition to troilite and schreibersite, he records the presence of "another mineral, the composition of which is not yet determined."

It should be mentioned that, with a view to test the relative solvent action of alkaline carbonates on quartz and the meteoric silica, weighed portions of each were digested with a ten per cent. solution of sodium carbonate for ten hours at 100° C. under precisely similar conditions. Of the quartz 7·843 per cent. had dissolved, of the Breitenbach silica 9·437 per cent.

The Shalka Meteorite.

An examination of the Shalka meteorite, also detailed in the same memoir, gave as a result that this meteorite consists mainly of a bronzite of the formula $Mg_{\frac{3}{4}} Fe_{\frac{1}{4}} Si O_3$, and is therefore identical in composition with, though very different in appearance and structure from, that of the meteorite of Manegaum.

HEAT GENERATED BY METEORIC STONES IN TRAVERSING THE ATMOSPHERE

AS supplementary to my short article on the "Maximum Velocity of Meteoric Stones Reaching the Surface of the Earth" (NATURE, Sept. 14, 1871, vol. iv. p. 398), permit me to add the following observations in relation to the source and the amount of the heat generated by such masses in traversing the atmosphere.

It is well known that the observations of Glaisher, Petit, Daubrée, and others, establish the fact that these bodies enter our atmosphere with velocities which are truly planetary. For example, the meteorite of Orgueil moved with a velocity of at least 20 kilometres (12·43 miles) per second, while in other cases velocities have been observed which could not be observed at less than from 15 to 30, or even 40 miles per second. The enormous resistance encountered by such bodies in traversing the air (as we have seen in the previous article), speedily extinguishes this high velocity, so that they retain but a comparatively moderate velocity on reaching the surface of the earth.

Now, the "Dynamical Theory of Heat" assures us that whatever energy is lost by the moving body is transformed into heat, which is either retained by the body or is communicated to the air. The amount of heat thus generated can be estimated, provided the diminution of velocity and the mass of the meteoric stone are known. More than 23 years ago Dr. Joule pointed out this transformation of energy into heat as the true cause of the ignition of these extra-terrestrial masses in traversing the air (*Vide* Phil. Mag., 3rd Series, vol. xxxii., p. 349, May 1848)

According to Joule's experiments, one French unit of heat = 423 5424 kilogrammetres or units of energy. Hence, one

$$\text{French unit of energy} = \frac{1}{423·5424} = 0·00236104 \text{ French units of heat.}$$

If, therefore,

v = velocity of stone upon entering the atmosphere,
v_1 = ,, ,, ,, when near the earth's surface,
g = acceleration produced by gravity,
w = weight of moving stone in kilogrammes:

Then if v, v_1, and g are taken in metres per second, we have,

$$\text{Loss of energy} = w \, . \, \frac{v^2 - v_1^2}{2g}.$$

This loss of energy transformed into heat =

$$0·00236104 \times w \, . \, \frac{v^2 - v_1^2}{2g} \text{ French units of heat.}$$

Assuming $g = 9·80942$ metres per second, we have,

French units of heat generated $= 0·000120345 \times w \times (v^2 - v_1^2)$.

This gives the total amount of heat evolved by the loss of energy.

Now, if we knew the distribution of the heat thus generated, it would be easy to estimate the increase of temperature which it would occasion in the matter to which it is imparted. If we assume all of the heat evolved to be retained by the moving body, it is evident that the increase of temperature ($v^2 - v_1^2$ being the same) is inversely proportional to the product of the weight into the specific heat of the moving mass.

Hence, if c = specific heat of stone in relation to water as unity, we have,

$$\text{Increase of temperature in C}^\circ = 0·000120345 \times \frac{w\,(v^2 - v_1^2)}{w \times c}\text{; or}$$

$$\text{Increase of ,, ,,} = 0·000120345 \times (v^2 - v_1^2) \times \frac{1}{c}. \quad (a)$$

Under the assumption that all of the heat generated is retained by the moving mass, it is obvious that w is eliminated; for, while the heat evolved varies as w, the mass to be heated also varies as w; so that the weight is without influence, in so far as increase of temperature is concerned.

In order to apply this formula to any given case, we must know v, v_1, and c in relation to the moving stone. Perhaps we should not be over-estimating v by assuming it to be 30 kilometres per second, or nearly equal to the velocity of the earth in its orbit. In like manner we should certainly not be under-estimating v_1 by assuming it equal to 500 metres per second. Substituting these values in formula (a), and we have,

$$\text{Increase of temp. in C}^\circ = 0·000120345\,(30000^2 - 500^2) \times \frac{1}{c}\text{; or}$$

$$\text{Increase of ,, ,,} = 108530 \times \frac{1}{c}.$$

Finally, in the absence of experimental results in regard to the specific heat of meteoric stones, we are certainly not under-estimating c by assuming it equal to 0·22.

Substituting this value of c in the last equation, and we have,

Increase of temp. in C° = 492,184°.

Of course, by far the larger portion of the heat generated by the loss of energy of the moving stone would be imparted to the air along its trajectory; but assuming that only $\frac{1}{100}$th part of it is retained by the stone, it would be more than sufficient to account for the phenomena of fusion and detonation which frequently accompany the transit of such bodies through our atmosphere.

In the case of small masses, it is clear that their high velocities would be more rapidly extinguished by the resistance of the air than is the case with large masses. In the small mass the transformation of energy into heat being accomplished in a shorter time, a greater amount of the evolved heat would be retained by the stone than in the large mass whose velocity is more gradually checked by the resisting medium.

Hence, when the smaller masses plunge into the upper atmosphere, the matter may be volatilised or utterly dissipated by the intensity of the suddenly-evolved heat. In this minutely-divided condition the material of the stones would float about in the atmosphere, and ultimately reach the surface of the earth in the form of meteoric dust.

It is well known that the observations of Benzenberg, Quetelet, Herrick, Newton, and others, assign to the so-called "falling stars" velocities equal to, if not surpassing, the velocities of meteoric stones. According to the foregoing suggestion, these may be nothing more than small meteoric stones which are volatilised in the upper regions of the atmosphere long before reaching the surface of the earth.

Thus, the phenomena of the occasional fall of meteoric stones and the almost incessant appearance of the falling stars which nightly furrow the celestial vault, may be correlated with the principle of transformation of energy. At all events, all the luminous, thermic, and detonating phenomena attending the fall of such bodies seem to be fully accounted for by the enormous amount of heat thus generated by their passage through the atmosphere.

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* Die Meteoriten im k. k. Hof-Mineralien-Kabinette. Wien, 1843, p. 95.
† Berg und Hütt Zeitung, xxi p 321.

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SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, November 1871. This number opens with a lengthy paper by Græbe and Liebermann on anthracene derivatives; they have studied in detail anthracene carbonic acid, and the behaviour of anthraquinine with phosphoric chloride and potassic hydrate. They have also experimented with the anthraquinine sulpho acids; at the present time researches on this subject possess considerable technical interest, as it is from disulphanthraquinonic acid that artificial alizarin is prepared, which seems to be fast supplanting the use of the madder root in dyeing. Bottger and Petersen follow on a subject nearly allied to the former, "on the compounds of anthraquinone containing nitrogen." Zinneman contributes a paper on an improvement in the method of distillation; the method is somewhat similar to that usually employed in chemical laboratories, that is to cause the vapour to pass up a long upright glass tube on which several bulbs have been blown, the exit tube being at right angles nearly at the top; the new portion of the apparatus consists in placing in the long tube a number of bell-shaped cages made of platinum wire gauze, which fit closely to the sides of the tube, and through which the vapour must pass or filter as it rises in the tube. With this apparatus the author has made an extensive series of experiments on the purification and determination of the boiling points of some of the most important of the ethyl compounds, on various acids of the same series, on the separation of propyl and butyl alcohols from the products of fermentation, and on the separation of the iodides of ethyl, propyl, and butyl. Judging by the results of these experiments, the new contrivance seems to work extremely well, and to be an improvement on the original form of apparatus. Several short papers complete this number, but they are not of extreme interest.

Journal of the Chemical Society, March 1872.—The composition of the natural tantalates and niobates forms the subject of a very important paper by Rammelsberg. Numerous analyses of various minerals containing the two rare elements, tantalum and niobium, are given, the results of which, however, cannot be usefully condensed.—Dr. Tilden presents a note "On the crystalline principle of Barbadoes Aloes;" the author has obtained a chloro-substitution product corresponding to the bromo-derivative, described some years since by Dr. Stenhouse, that is aloin in which three atoms of hydrogen are replaced by chlorine. The remainder of this number is occupied by the abstracts of foreign papers, many of which are of great interest.—Dr. de Coppet finds that supersaturated solutions of sodic sulphate can be prepared by dissolving the anhydrous salt in cold water; it is, however, necessary that the anhydrous salt should be heated above 33°, and cooled out of contact with the air, as it is found that the anhydrous sodic sulphate, obtained by drying the ordinary salt (containing ten molecules of water) at a temperature lower than 33°, always acts as a nucleus in determining the crystallisation of a supersaturated solution of this salt. From this it appears that from crystallised sodic sulphate different bodies are obtained depending on the temperature at which the salt is dried.—A long abstract of Linnemann's important paper "On an improvement in the method of fractional distillation," and also of his researches on the normal propyl alcohol and its compounds are here given.—Another curious example of the lowering of the boiling point of mixtures of water and other liquids forms the subject of another abstract. Is. Pierre finds that a mixture of water and butyl iodide distils at 95°-96° (butyl iodide boils at 122·5°), and that a constant mixture of 79 parts of iodide and 21 of water is found in the distillate. Ethylic iodide (B.P. 72°) and water distil at 66°, only 3 to 4 per cent. of water condensing with the iodide. The abstracts as a whole are shorter and more condensed than they were originally; in some cases this must be regretted, though in others it is, perhaps, an advantage.

THE *Quarterly Journal of Microscopical Science* for April 1872, contains memoirs on the Development of the Enamel in the Teeth of Mammals, as illustrated by the various stages of growth demonstrable in the evolution of the fourth molars of a young elephant, and of the incisor teeth in the fœtal calf, by Prof. Rolleston, M.D. This paper is a reprint.—Note on Immersion Object-glasses for the Microscope, by Dr. Royston Piggott, chiefly directed towards the assertion that the aperture of an immersion lens can never exceed 80°, which this author denies.—Observations of Pathological Changes in the Red Blood-corpuscle, by J. Braxton Hicks, M.D.—On the Artificial Production of some of the principal Organic Calcareous Formations, by Prof Harting, of the University of Utrecht. This is an abridged report of researches undertaken with the view of producing, independently of living organisms, certain calcareous formations, which are met with in animals as integral parts of their skeleton, and this by causing calcium carbonate and phosphate to combine in the nascent state with organic substances.—On the Peripheral Distribution of Non-medullated Nerve-Fibres, by Dr. E. Klein; the third part of a memoir, of which the previous portions appeared in this journal. It treats of the relation of the non-medullated nerve-fibres to small arteries, small veins, and to capillary vessels in the muscular substance of the frog's tongue; and the termination of nerve-fibres in the ciliated duct in the tail of the rabbit.—On the Structure of Tendon, by J. Mitchell Bruce, partly in support and partly in con-troversion of the views of Boll, as detailed in Max Schultze's *Archiv.*—The Embryology of Chrysopa, and its bearing on the Classification of the Neuroptera, by A. S. Packard, Jun., M.D. Reprinted from the *American Naturalist.*—Preliminary notice of Researches on the Anatomy of the Serous Membranes in Normal and Pathological Conditions, by Dr. E. Klein and Prof. Burdon Sanderson, forming a part of investigations on infectious diseases, undertaken for the Medical Department of the Privy Council. These investigations relate to more than 250 animals, especially rabbits and guinea pigs, many frogs, several cats, dogs, some rats, and one monkey.—The remainder of the journal is occupied with abstracts, notices, and reports.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 16.—"On the Specific Heat and other physical characters of Mixtures of Methylic Alcohol and Water, and on certain relations existing between the Specific Heat of a Mixture or Solution, and the Heat evolved or absorbed in their formation," by Dr. A. Dupré.—"On Supersaturated Saline Solutions," Part III., by Charles Tomlinson, F.R.S., and G. Van der Mensbrugghe.—"Remarks on the Sense of Sight in Birds, accompanied by a description of the Eye, and particularly of the Ciliary Muscle in the genus *Raptores*," by Mr Robert Lee.—Supplement to Mr. Lee's communication on the Sense of Sight in Birds. A Description of the Eye in *Rhea americana*, *Phœnicopterus antiquorum*, and *Aptenodytes Humboldtii*.

Geological Society, May 8.—Mr Joseph Prestwich, F.R.S., in the chair.—The following papers were read:—1. "Notes on Atolls or Lagoon Islands." By Mr. S. J. Whitnell. The author commenced by indicating certain facts which lead him to think that the areas of atolls are not at present sinking, and referred to one instance (that of Funafuti or Ellice Islands), in which he thought there were signs of a slight upward movement. He noticed the occurrence of a furrowed appearance, or a series of ridges or mounds, in some islands, each of which he regarded as produced by a single gale. He also described a freshwater lagoon, about three miles in diameter, as occurring in the Island of Quiros. Mr. Thorpe was acquainted with the atolls around the coast of Ceylon, and thought that the local traditions, untrustworthy as such sources usually were, might afford some evidence as to the date of their origin. The tradition of Ceylon was that the Maldive and Laccadive Islands had within the memory of many been connected with Ceylon. If this were so the evidence was in favour of the area being one of subsidence. Mr. D. Forbes, when in 1859 he spent some months in the Pacific, had been requested by Mr. Darwin to examine into the evidence as to the origin of these atolls by elevation, and had found that the asserted cases of the existence of masses of coral at a considerable elevation above the sea merely arose from blocks having been transported inland by the natives. Though, however, there was no evidence of elevation, it was still possible that such had in certain cases taken place, as there were still active volcanoes in this region. The freshwater lakes he attributed to the drainage of the islands

2. "On the Glacial Phenomena of the Yorkshire Uplands." By Mr. J. R. Dakyns. The author stated that in Derbyshire and Yorkshire, south of the Aire, there is no glacial drift on the eastern slope of the Pennine chain, except where it is broken through by the valleys of the Wye and of the Aire and Calder. The basis of the Aire and the country northward are thickly covered with drift, which contains no rocks foreign to the basis, and thus points to formation by local action. The author ascribed this to the glaciation of the country in part by glaciers, and in part by a general ice-sheet. Evidence of the latter he finds in the fact that drift occurs only on one side of the valleys, namely, on the lee-side of the hills with respect to the source of the drift materials. Traces of the action of glaciers are the great amount of scratched and rounded pebbles in the mounds of drift, which increases in proportion to the distance from their source: the presence of great piles of drift at the junction of valleys, as if by the shedding of the lateral moraines of two glaciers; and the existence of mounds of pebbles and of an alluvial deposit wherever a rock-basin crosses a valley. The Kames or Eskers, which are frequent in the valleys, he ascribed to the deposition of moraines in the sea instead of on land. Prof. Ramsay agreed with the author as to the existence of these rock-basins in the Yorkshire area, and as to the absence of marine drift on great part of the slope of the Pennine chain. The terminal moraines had to some extent become obscured by the washing of soil by rain; but their ancient existence in many of the Yorkshire valleys was indisputable. The features of the country, were, moreover in many instances such as could not be reconciled with the deposition of the drift by marine action. 3. "On a Sea-coast Section of Boulder-clay in Cheshire" By Mr. D. Mackintosh, F.G.S. The principal object of the author was to draw attention to the fact of the occurrence of numerous sea-shells in a lower boulder-clay at Dawpool, as thoroughly glacial in its appearance, structure, and composition, as any clay to be met with along the shores of the Irish Sea, and differing in no essential respect from the *Pinel*, which runs up the slopes and valleys of the Lake District. He pointed out a number of very important distinctions between the Lower and Upper Boulder-clays of Cheshire, referring especially to the light grey or blue facings of the fractures of the latter. He gave a list of a number of large boulders, greenstone and Criffell granite predominating, though among the smaller stones Silurian grit was most prevalent. The author likewise explained the mode of striation of the stones found in the clay, and the position they occupied in reference to their flattened surface. The paper was illustrated by samples of the two clays, a number of shells in various states of preservation, and about forty specimens (most of them named and their parentage assigned) of Silurian grit and argillite, greenstone, several varieties of felstone and porphyry, felspathic breccia, Criffell and Eskdale granites, and granites of unknown parentage, Wastdale or Ennesdale syenite, quartz, Carboniferous Limestone, chalk flints (?) local gypsum, sandstone, &c. In a letter Mr. Searles V. Wood, Jun., stated that he regarded the Boulder-clay containing the shells as later than the newest of the East-Anglian beds, and the Upper clay as probably equivalent to the Hessle clay. The fragmentary shells sent had been determined by Mr. J. Gwyn Jeffreys, who found eleven species represented among them, and stated that they agreed with the shells from Moel-Tryfaen and Macclesfield. He remarked especially on the occurrence of *Astarte borealis*, a species now extinct in the British area. Prof. Ramsay remarked, with regard to the Bridlington beds which had been cited, that they were probably preglacial, and not glacial. He thought that eventually it would be proved that during the Glacial Period there had been several oscillations in this country both in level and temperature. With respect to temperature, the calculations of Mr. Croll showed the extreme probability of such variations being due to astronomical causes; and these were best illustrated by reproducing his figures in the form of a diagram, showing the curves and oscillations of temperature. 4. "On Modern Glacial Action in Canada.—II." By the Rev. William Bleasdell, M.A. In this paper the author communicated some facts illustrative of the action of ice in Canada, in continuation of a former paper. Fidlar's Island, in the rapids of the river Trent (flowing into the head of Lake Ontario), has been removed within the last eighteen months. Patrick's Island, a mile lower down, is also rapidly disappearing. Salmon Island, in the Bay of Quinte, between Amherst Island and the mainland, which had an area of about an acre fifty years ago, has disappeared, leaving a shoal with about four feet of water over it; and three neighbouring islets, known as The Brothers, are in course of removal. The removal of these islands is due to the action of drift-ice. The author also referred to the formation of ground-ice in the Canadian rivers. Prof. Ramsay mentioned that Sir William Logan had informed him that shore-ice in Canada, charged with boulders, had been known to produce grooves on the face of cliffs as well marked as those of glacial times. He had also mentioned the case of a boulder transported by ice which was of such a size as to have occasioned the wreck of a vessel which had struck upon it.

Mathematical Society, May 9.—W. Spottiswoode, Treas. R.S., in the chair.—Mr. J. W. Glaisher gave an account of his paper "On Functions with Recurring Derivatives." Mr. Tucker (Hon. Sec.) read portions of communications from Prof. J. Clerk Maxwell, on Equations of Motion, and on the Transformation of Solids.—Prof. Clifford made some remarks on a theory of the exponential function derived from the equation $\frac{du}{dt} = pu$. Prof. Cayley, Dr. Hirst, Mr. S. Roberts, and others, took part in a subsequent discussion on the degenerate forms of curves.

Chemical Society, May 16.—Dr. Debus, F.R.S., vice-president, in the chair.—"On the Influence of Pressure upon Fermentation," Part I., by Mr. H. T. Brown. The results of his experiments were that under diminished pressure the amount of gas unabsorbed by potash is greatly increased, and that it contains a proportionally large amount of hydrogen. Acetic acid and aldehyde are also formed under these circumstances, so that it would seem that water is decomposed during the alcoholic fermentation, especially when it takes place under diminished pressure. Papers "On the Electrolysis of Sugar Solutions," by Mr. H. T. Brown; "On the determination of the solubilities and specific gravities of certain Salts of Sodium and Potassium," by Messrs. D. Page and A. D. Keightley; and "An examination of the recent attack on the Atomic Theory," by Mr. Atkinson, were then read. An animated discussion on the Atomic Theory ensued. Mr. C. O'Sullivan then read his elaborate memoir "On the transformation products of Starch."

Anthropological Institute, May 20.—Dr. Charnock in the chair.—Mr. J. Bonomi exhibited and described a new instrument for measuring the proportions of the human body, being specially applicable to the identification of criminals, and adapted for a rapid and very easy method of measurement in military and other large establishments.—A paper was read by Mr. George Harris, V.P., "On Moral Irresponsibility resulting from Insanity." He concluded that persons are not responsible for their actions in cases where they labour under delusions to such an extent that their conduct is not only influenced, but determined, by their belief, although they may still continue to reason correctly. Irresponsibility should be allowed also in those cases where they are impelled by irresistible morbid impulses; and where from disorders of the nervous system they are suffering from violent and uncontrollable irritability. He criticised the legal definition of irresponsibility in cases of that nature. As to suicide affording a proof of insanity, that must depend on the circumstances of each particular case. The author pointed out the contradictory theories laid down respecting insanity, and alluded also to the conflicting opinions expressed by medical men.

Geologists' Association, May 3.—James Thorne, F.S.A., vice-president, in the chair. "On Columnar Basalts," by Mr. John Curry. As a preliminary, the process of the formation of columnar mud was briefly described, the analogy between this process and that of the formation of columnar basalt being such that the same diagrams served for illustrations in both cases, though, in the former case, heat penetrates the fine clayey sediment, and produces columns of dried mud; in the latter, cold advances into the molten lava, and changes it into solid rock, which frequently has a rude but sometimes a perfect columnar structure. Hexagonal columns are the most perfect. In reference to the production of these columnar structures, the chief and most notable conditions are unequal temperatures, which the author designated heat and cold, closely situated at the contact surfaces of dissimilarly constituted bodies. In the above instances the dissimilar bodies in surface contact are, first, the atmosphere and fine clayey sediment; second, the oceanic water and the molten lava. While the solidifying of the lava is being effected, the solid and molten parts may be considered as distinct bodies. It is at and near the surface contacts that the exchanges of heat are most efficient in giving structure to the forming body. The actions of heat and cold, having a directive influence in the formation of hexagonal basaltic columns, were shown in the illustrations. In the concluding part of the paper the large surface contact of the ocean with the bed on which it rests, and the contact of the atmosphere with the land and water surfaces, were adverted to as areas where the exchanges of heat had done and were now doing much work in producing structures. Jointage, cleavage, and various other rock structures are ascribed as being mostly due to such work. Ice structure is another marked example. It bears a striking analogy to the hexagonal columnar structure of basalts, inasmuch as it often breaks up into columns by slow melting. The ice of a lake sometimes shows such a breakage when it melts in the spring.—"On a visit to the Diamond Fields of South Africa, with notices of geological phenomena by the wayside," by Mr. John Paterson. On the geological questions connected with the diamond fields the author propounded some new views based on a minute and careful examination of the appearances which presented themselves to him on his visit to the diamond-fields. He discredits the theories which would refer the presence of diamonds in Griqualand West to any distant sources; and thinks the evidence incontestable that the marl soil, as he named it, in which the gems are now found, is the true matrix soil of the diamond. This marl soil he considers to be the metamorphosed carboniferous shales of the country, and the change which has worked upon these shales, by which they have been transformed from the black carboniferous shale into the whitish ashy marl in which the diamonds are found, he attributes to intrusions of greenstone trap, which traverse that country from N.E. to S.W. in continually recurring dykes. Mr. Paterson gave some very interesting details of the extent and richness of the diamond diggings in South Africa, and in his picture of the Gong-Gong and Delport Diggings as "Great Rushes" in digger's phrase, resembling in extent and richness Colesberg Kopje, but now nearly worked out, not by the hand of man in a few years, but by the angry waters of the Vaal River through many ages, he found much groundwork of hope that the diamond discoveries of South Africa are to be no fleeting passing industry, but a continuous employment, not only for many years but for many ages.

Society of Biblical Archæology, May 7.—Mr. R. Cull, F.S.A., in the chair.—"On Underground Jerusalem; more particularly in reference to the Plateau of the Haram es Shereef." By Mr. William Simpson. The paper described Omar's search under the guidance of Sophronius, the patriarch of Jerusalem, for the site of the Temple, as one of the first explorations into the topography and archæology of the Holy City. The transference of holy places from one point to another was explained as involving confusion and adding to the difficulties of arriving at reliable facts. The principal theories respecting the site of the Temple and the Holy Sepulchre were defined, and their merits touched upon so far as to indicate the progress of the questions connected with them. The importance of a careful study of the various styles of building in the Haram Wall was pointed out so as to get a date, if possible, as a ground upon which to start. A most interesting part of the paper was a description of the Great Sea, excavated out of the solid rock, under the Temple site, and the supply of water to it from the pools of Solomon near Bethlehem. The great importance of the water system for the Temple uses having an essential bearing on the question of the topography, and the question was still one which required further knowledge and study to arrive at a definite result.—"On the so called Moabite Stone, described in a late Letter to the *Times*." By Mr. B. G. Jenkins. The author considered that the letter and the inscription bore their own condemnation; for the stone could not be Moses' memento of the conquest of a land he never attacked.—"Observations on the dimensions of the Great Pyramid and the Royal Coffer." By Solomon M. Drach.

PARIS

Academy of Sciences, April 29.—M. Chasles read a paper containing theorems relating to the obliques drawn through the points of a curve under angles of the same size.—M. F. Lucas communicated some general theorems on the equilibrium and movement of material systems.—M. L. Melsens presented a note on some effects of the penetration of projectiles into various media, and on the impossibility of the fusion of leaden bullets in the wounds produced by fire-arms.—A memoir was read on the electrical jet in rarefied gases, and especially on its mechanical power, by MM. A. de la Rive and Sarasin. Their experiments showed that the velocity of rotation of the jet diminishes as the density of the gas is increased, but not in the same proportion, and that the jet possesses considerable mechanical force.—MM. P. A. Favre and C. A. Valson presented a continuation of their memoir on crystalline dissociation, containing the second part of their investigation of the alums.—M. Fizeau presented a report on a memoir by M. Croullebois on the elliptical double refraction of quartz.—A continuation of M. P. Desains' researches upon the reflection of heat was read, and a note by M. D. Gernez on the absorption-spectra of selenium and tellurium, their protochlorides and protobromides, and of the protobromides of iodine and alizarine were presented by M. H. Sainte-Claire Deville.—Several papers and notes on auroras were read, including the continuation of M. J. Silbermann's memoir on the causes and laws of auroras, a note by Father Denza on auroral phenomena observed in Italy in March and April 1872, and a note by M. Guillard on indications of an aurora borealis observed at Lyons on April 8.—A second note by M. W. de Fonvielle on the hypothesis of the magnetisation of the sun was also read.—M. Delaunay presented a note by M. G. Héraud on the tides of lower Cochin China, with especial reference to the determination of the diurnal and semi-diurnal waves.—A note by M. Chapelas on a meteor observed at Rheims on the night of April 19-20, and one by M. Perris on a meteor seen at Agde on the evening of April 24 were read.—M. Aimé Girard presented an investigation of the salt marshes and salt industry of Portugal, in which he describes the details of the extensive manufacture of sea-salt carried on in that country.—M. Bussy presented a note by M. J. Personne on the presence of selenium in the sulphuric acid manufactured in France.—A note by the Abbé Laborde on the action of oxygen upon certain vegetable infusions was communicated by M. Pasteur. The author, after hermetically sealing a vegetable infusion in a glass vessel, and keeping it for a month unchanged, produced from it successive small quantities of oxygen by electrolysis. The infusion still remained unaltered, notwithstanding the replacement of the vacuum by an atmosphere of oxygen; but when the vessel was opened to the air vegetation soon made its appearance.—M. Pasteur also communicated a note by M. Griessmayer on the question of the assimilation of ammonia by yeast.—A note by M. H. Byasson on the physiological action of formic æther was presented by M. Robin.—M. Blanchard presented a note on the appearance of unusual numbers of *Bibio hortulanus*, a dipterous fly, in Paris.—A paper by M. P. de Gasparin on the constitution of clays was read. This paper contained an analysis of a very fertile clay soil from near Nîmes.—M. de Quatrefages communicated a note by M. E. Rivière on the human skeleton discovered in the caverns of Boussé-Roussé, in Italy, on March 26, 1872. This skeleton, which was associated with unpolished flint implements, presents no simian characters, and the cranium most nearly resembles those of Cro-Magnon.

May 6.—M. Chasles presented a note by M. H. Durrande on the general properties of the displacement of a figure of variable form.—A note by M. T. D'Estocquois on the movement of water in drains was read.—M. J. Bourget read a memoir on the economic coefficient in the thermodynamics of permanent gases.—M. Wurtz presented a note by M. G. Salet on the light emitted by the vapour of iodine, in which the author stated that that vapour may be heated to redness.—A letter from M. Tacchini to M. Faye, with regard to the Society of Italian Spectroscopists, and a reply to some of the statements contained in it by M. Faye, were read. They contained a discussion of various matters connected with our knowledge of the sun.—A note on atmospheric ozone by M. L. Palmieri was presented by M. C. Sainte-Claire Deville. The author stated that air, by passing through a glass tube, ceases to colour iodised starch paper. M. Sainte-Claire Deville confirmed this statement from M. Houzeau's observations.—A letter from M. Donati on auroras was communicated by M. Delaunay.—M. Le Verrier presented a note by Father Denza on a sand-shower which fell in Italy during the night of the 19-20 April.—A letter by M. Silbermann on the relations between meteorological phenomena and volcanic eruptions was read.—A note was read by M. C. Daniel on a process of decorative painting upon tin, and on this M. Dumas made some remarks.—M. Frémy presented a note by M. Prinvault on the transformation of pyrophosphates into phosphates by the action of boracic and sulphuric acids.—A note by M. Yvon on the quantitative determination of copper by cyanide of potassium was communicated by M. Bussy. This note relates to one on the same subject by M. Lafollye, read on April 22; M. Yvon states that the reaction then employed was indicated in 1859 by M. Buignet, points out certain inaccuracies in M. Lafollye's paper, and describes a process of his own.—M Wurtz presented a note by MM. C. Girard and G. de Laire on the formation of diphenylamine with reference to a recent note by MM. Dusart and Bardy.—M. Arrez forwarded a letter relating to his process of treating vines attacked by *Phylloxera vastatrix*, and upon this M. Dumas made some remarks.—Prof. Milne-Edwards presented a note by M. A. F. Marion on the reproductive organs of *Oria Armandi* Clap.—M. Bouley presented a note by M. S. Arloing on the nature of the blood-globules; and M. C. Robin some investigations by M. P. Bouland on the normal curvatures of the rachis in man and animals—M. Ziegler presented a paper on a physiological fact observed in leaves of *Drosera*, containing some very curious observations on the effects produced on that plant by albuminoid substances after contact with the human hand.—M. Paul Gervais described the lower jaw of a new species of fossil ape from the lignites of Monte Bamboli in Italy. He regards this animal as forming a new genus standing at the end of the series of anthropomorphous apes after the gorilla and before the baboons and macaques. He gives it the name of *Oreopithecus Bambolii*.—M. Delesse communicated an investigation of the deformations which the French geological formations have undergone.—A note on the granite, sand, and clay with flints of the department of the Seine and Oise, by MM. Potier and Donvillé, was read, as also a paper on the valley of the Vezère, by M. F. Hément. The latter, communicated by M. de Quatrefages, contains accounts of traces of prehistoric man found in the valley.

BERLIN

German Chemical Society, May 13.—Dr. Bulk reported on the action of fuming sulphuric acid on aniline blue. He obtained tri-phenyl-rosaniline, in which four molecules of SO_3H replace four atoms of hydrogen, Nicholson's process yielding conjugated acids with from one to three molecules of SO_3H. Hofmann's violet behaves in a similar way to fuming sulphuric acid.—C. Scheibler stated that Erlenmeyer's observations on the non-existence of Gerhardt's para-thionic acid have been previously made by himself.—C. Rammelsberg has analysed a beautifully crystallised specimen of cast iron, confirming thereby his view that the various commercial grey and white irons are isomorphous mixtures of the pure element with combined carbon, and with non-combined carbon.—Dr. A. W. Hofmann reported on researches made by Dr. Geyger and himself on the action of aniline on azo-diphenyl-diamine ($N_3(C_6H_5)_2H$) namely:—$C_{12}H_{11}N_3 + C_6H_7N = C_{18}H_{15}N_3 + NH_3$. The body, violet and soluble in alcohol, combines with one atom of HCl. Its formation and constitution correspond to the magdala red which has been obtained through the action of naphthyl-amine on azo-dinaphthyl-diamine. The body has the same composition as violaniline, obtained by oxydising pure aniline, when three molecules are condensed into one by losing six atoms of hydrogen. Azo-toluyl-diamine, $N_3(C_7H_7)_2H$, treated with aniline, gives rise to an analogous compound, having most likely the composition of rosaniline, $C_{20}H_{19}N_3$, from which it seems, however, to differ. A colouring matter recently offered in the trade, under the name of saffranine (mixed with large quantities of carbonate of lime), dissolving with a yellowish red in water, with a violet colour in concentrated hydrochloric, with a green colour in concentrated sulphuric acid, has been obtained in a similar manner; a mixture of aniline and toluidine being exposed to the action of nitrous acid and afterwards oxydised. Its chloride contains $C_{20}H_{20}N_4HCl$, the formula of which corresponds in appearance to a salt of amido-rosaniline, although its constitution is most likely very different. Perkins' analyses of mauveine agree with the formula of phenylated saffranine, $C_{26}H_{24}N_4$; and some of its reactions, notably its behaviour to hydrochloric and sulphuric acid, coincide with this suggestion, these acids producing violet and green colours as with saffranine. But it is not easy to understand how the fourth atom of nitrogen should enter into mauveine under the ordinary conditions of its formation.—Th. Peterssen published the analysis of a metasilicate of sodium, $Na_2SiO_3 + 5$ aq.—Barl and Hubner found that nitrobenzol and similar substances form hydrocyanic acid when fused with potash. A similar observation has been published by Wohler in 1828.—Geromont has obtained isobutyric acid from bibromo-pyrotartaric acid.—Michaelis found PCl_3Br_2 to form unstable combinations with Br_2 and Br_4, comparable to the addition of water of crystallisation to a saturated compound.

BOOKS RECEIVED

ENGLISH.—Conversations on Natural Philosophy, 14th edition: Mrs. Marcet (Longmans)—The Geometry of Conics, Part 1.: C Taylor (Deighton and Bell)—Esse and Posse, a comparison of Divine eternal laws and powers: H T Braithwaite (Longmans)

FOREIGN.—Deutscher Universitats Kalender fur das Sommer-Semester, 1872: Dr F. Ascherson, Berlin.—Sonnensystems: Meibauer.

DIARY

THURSDAY, MAY 23.

ROYAL INSTITUTION, at 3—On Heat and Light: Prof. Tyndall, F.R.S.
LONDON INSTITUTION, at 7 30—On the Artificial Formation of Alcohols from their Elements: Prof H. E. Armstrong.

FRIDAY, MAY 24.

ROYAL INSTITUTION, at 9.—On Babbage's Calculating Machines: Prof. Clifford.
LINNEAN SOCIETY, at 3 p m.—Anniversary Meeting.
VICTORIA INSTITUTE, at 4 p m.—Anniversary Meeting.
QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, MAY 25.

ROYAL INSTITUTION, at 3.—On the Chemical Action of Light: Prof. Roscoe, F R.S.
GOVERNMENT SCHOOL OF MINES, at 8.—On Geology: Dr. Cobbold, F.R.S.

MONDAY, MAY 27.

ROYAL GEOGRAPHICAL SOCIETY, at 1 p.m—Anniversary Meeting.
LONDON INSTITUTION, at 4.—On Elementary Botany: Prof. Bentley.

TUESDAY, MAY 28.

ROYAL INSTITUTION, at 3.—On Development of Belief and Custom: E. B. Tylor.

WEDNESDAY, MAY 29.

ROYAL SOCIETY OF LITERATURE, at 8 30—On the Province of Conjecture in Literary Criticism: Dr. C. M Ingleby.

THURSDAY, MAY 30.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
ROYAL INSTITUTION, at 3—On Heat and Light: Prof. Tyndall, F.R S.
LONDON INSTITUTION, at 7.30.—Experimental Evidence against the Spontaneous Generation of Living Things: W. N. Hartley, F C.S.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, MAY 30, 1872

BOTANY IN THE OXFORD NATURAL SCIENCE SCHOOL

THE merits and demerits of the "Notice by the Board of Studies for the Natural Science School of the University of Oxford" will by this time have been abundantly discussed among all circles of teachers of Natural Science. That the former preponderate over the latter will hardly admit of a question; and if we once more call attention to the defects of the present "Notice," it is only in order that, by full and free discussion, a thoroughly satisfactory scheme may be at length elaborated. We have already pointed out the subordinate part which Botany plays in the programme, and this defect it is our present object to illustrate more at length. We imagine that the object of the framers of the scheme must have been to lay down in the Preliminary Honour Examination a foundation which will serve to give a solid basis for any superstructure that may be raised upon it—in other words, to supply the student with an adequate mental training to prepare him for whatever special branch he may afterwards take up. Let us see how this works in the case of Botany. The Natural Sciences which will be most useful to the future botanist in enabling him to gain a comprehensive insight into his own subject, are the outlines of Animal Physiology, Geology, and Organic Chemistry. Of not one of these subjects need he necessarily possess any knowledge in order to pass the Preliminary Honour Examination; but, on the other hand, must read up Mechanics, Physics, and Inorganic Chemistry. It seems to us a mistake to make the physical side of Science of such preponderating weight in the preliminary examination, to the entire exclusion of the biological.

If we now turn to the Final Examination in Biology, we still find that although it is specially stated that under Biology are included both Zoology and Botany, yet that his own special subject is still kept entirely in the background. Among the "list of books recommended for use in the preparation for the General Examination in Biology" are a number in General Anatomy and Histology, Comparative Anatomy, Human Physiology, Comparative Physiology, and the General Philosophy of Biology, but not one in any department of Botany, with the exception that in a foot-note we are informed that under the term "General and Comparative Anatomy and Histology" "vegetable structures" are included. Otherwise the examination is exclusively one, not in Biology, but in Animal Physiology and the Comparative Anatomy of Animals. The works, indeed, on this subject to which the intending botanist is referred would require a very long course of study to master. The list is so extensive, and the range so great, that it must necessarily have the effect of deterring many an aspirant for distinction in the Natural Science School.

The arrangements under the special subject of Botany seem to us no less open to objection. The first impression conveyed is that the subject can only have been admitted at all under protest, and that it is looked upon as of decidedly subordinate importance to all the rest. While Mineralogy and Crystallography have three pages devoted to them, Geology five, and Zoology four, Botany is compressed into one, and the information and assistance given is of the most meagre. The books "provisionally recommended" are ten in number; but what is meant by a provisional recommendation we do not know. The Board must surely have made up its mind as to whether the works are to be recommended or not; and we venture to say that some even of these ten are books that ought not to have obtained the sanction of such a body. Not only does the list err on this side; but books which are familiar to every botanical teacher as the best elementary works are not to be found in it. It must be remembered that, as far at least as structural and systematic botany are concerned, the student is now at the outset of his career; and yet what will be thought of a list of books recommended to his notice which makes no mention of either Oliver's "Lessons" or Lindley's "School Botany"? In Geographical and Geological Botany, the recommendations comprise the portions bearing on these subjects of Henfrey's "Elementary Course" and Balfour's "Manual of Botany." Now, we think it is generally admitted that these are the least satisfactory portions of the works quoted; and we have no hesitation in saying that the student will gain from them no adequate knowledge of the present state of these sciences. Not a hint is given of the existence of original memoirs or essays, such as those of De Candolle, Grisebach, Heer, Unger, Hooker, and Carruthers. Again, in the other special subjects the excellent practice is adopted of referring students to monographs or treatises on special branches of the subject, which will not be the least instructive part of his reading. In the botanical list we find no reference even to works so familiar to every student as Mohl "On the Vegetable Cell," Hofmeister "On the Higher Cryptogamia," or the writings of Robert Brown. We venture to say that from the careful study of any one of these works, or even of such smaller treatises as Dr. Hooker's lecture on "Insular Floras," or the introductory essay to his "Flora of Tasmania," the student will gain a deeper insight into the philosophy of his science than from the greater number of the books in the recommended list.

But the greatest defect in the botanical section we take to be the very small stress that appears to be laid on practical work. In accordance with the prominence which is given throughout the whole scheme to histology, it is true that the candidate will be tried with "dissections and descriptions of preparations, illustrating the minute structure and organs of plants;" but he is not informed that his practical acquaintance with morphology and the principles of classification will be tested by requiring him —as is done at all the examinations at the University of London, constituting the most fruitful source of "pluck"— to describe and refer to their natural orders plants presented to him in the room. We the more lament this omission, because it will but have a tendency to confirm the low estimate in which the science of Botany is held even by many biologists, who look upon it as a mere science of terms, leading to no large and comprehensive principles, and susceptible of indefinite "cram." Until Botany is rescued from this degrading position, and teachers learn that it is as much a science of experiment and observation as Animal Physiology or Comparative Anatomy, we shall always have to lament the dearth of philosophical botanists so remarkably exhibited in this country at the present time; and to hear it spoken of even by biologists with a covert contempt.

We make these comments with no desire to detract from the great work which the Board of Studies for the Natural Science School at Oxford is doing, in attempting to elevate Natural Science to a prestige equal to that of the older studies at our national universities. In this endeavour we wish them heartily all success, and are fully sensible of their earnestness to effect this object. But in order to secure success, it is necessary that any mistakes in the early steps must be freely and candidly pointed out, and that the plan of the campaign must be made as faultless as possible. We know that there are those at Oxford who are fully sensible of the deficiencies in the programme to which we have called attention, and who have fought a losing battle for a more thorough and comprehensive, and at the same time more eclectic, plan. We would encourage those to persevere in their endeavours, believing that they must ultimately prevail, and that from this beginning a scheme of instruction in Natural Science will ultimately arise which will be a model for the whole kingdom.

MOUNTAINEERING IN THE SIERRA NEVADA

Mountaineering in the Sierra Nevada. By Clarence King. (London: Sampson Low, Marston, Low, and Searle, 1872.)

A VERY pleasant admixture of science and personal adventure, from the hand of one who is evidently a sincere lover of nature, and is gifted with considerable descriptive power. Men and manners in the Far West are depicted with much humour; and one chapter, entitled Kaweah's Run, narrating the escape of the author from a couple of brigands who attempted to hunt him down, will show that a Government surveyor's work in America is apt to be more exciting than pleasant. It is a good while since we have read a book so thoroughly unaffected and fresh; redolent of the clear air of those lofty Sierras where (hear it, ye Alpine climbers, who, in your haunts, daily curse Jupiter Pluvius!) fine weather is the rule. The description of some of Mr. King's scrambles is enough to make the Alpine Club rush off in a body to Mount Whitney; but we cannot help suspecting that his neck would have more than once been safer had he known the rules of that fraternity and carried a good *piolet.*

Mr. King does not intend his book for a scientific treatise, but there are some valuable notes scattered up and down its pages, and with these we must chiefly concern ourselves. The first chapter gives a good sketch of the geology and physical geography of the Sierra Nevada district. It was submerged till Jurassic periods, the ocean shallowing much in the later Triassic time. Then were produced the long mountain waves which stretch from Mexico probably into Alaska, reaching as far east as Middle Wyoming, and forming one broad zone of crumpled ridges, whose westernmost and loftiest member is the Sierra Nevada. Rivers carved the land into cañons, and the sea gnawed its western shores during all the Cretaceous and much of the Tertiary period, in the later part of which the coast ranges were rolled up, facing the Sierra Nevada, and converting the California valley into a great inlet of the sea. Then, from newer and older ranges alike, began an epoch of furious volcanic activity, till at last the fires burnt low and the greater number went out altogether. To this succeeded a period when, as in North-Western Europe, great glaciers flowed down the valleys, polishing the rocks and leaving behind them a huge trail of moraine. Now they have shrunk back into snow-fields; and it is only here and there, as about Mount Shasta, that we find any mention of true glaciers in Mr. King's book.

The magnificent cañons, which have more than once been mentioned in the pages of NATURE, are frequently and vividly described. This is the author's opinion of their origin: "Although much is due to this cause (the cutting power of rapid streams) the most impressive passages in the Sierra valleys are actual ruptures of the rock, either the engulfment of masses of great size, as Professor Whitney supposes in explanation of the peculiar form of the Yosemite, or a splitting asunder in yawning cracks. From the summits down half the distance to the plains the cañons are also carved out in broad round curves by glacial action." It may seem presumptuous in one who has never seen the region to differ from Mr. King and his chief, at the same time we cannot help suspecting that here, as in the Alps, it will be shown ultimately that streams have been the principal agents in forming gorges, and that, though they may have been guided by rifts and certainly by joints, no traces of the original fissures can now be found.

Among the scientific "plums" of light description scattered throughout the pudding, we may notice the following:—The granite of some of the mountains of the Yosemite valley exhibits spheroidal structure on a colossal scale, "concentric layers like the peels of an onion each one about two to three feet thick." This structure never descends into the mass for more than a hundred feet. The author notices a peculiar flaky structure on the surface of ice-worn granite (p. 147) developed, as he believes, by the great pressure which it has undergone. A curious case of granite polished by sand friction is also recorded on p. 146, reminding us of the polished basalt on the shore of Fife. Earth pillars in the cañon of the McCloud glacier (Mount Shasta) are described, "from one to seven hundred feet high, each capped with some hard lava boulder which had protected the soft (trachyte) *débris* beneath from weakening." A curious cavern in a lava floor in the same region—roughly tubular in shape and more than half a mile long—doubtless produced, like those in Iceland, by the outburst and escape of the still liquid interior of the hardening stream, is worth notice; as well as the fresh-water deposits of a lake which existed through the Cretaceous and Tertiary periods between the Rocky Mountains and the Blue Mountains of Oregon. Nor must we in conclusion forget the humorous tale of how the author, after being sternly rebuked by the palæontologist of the survey for loving snow-peaks better than fossils, repented and found a cephalopod in the auriferous slates of Mount Bullion, and so determined their age. We note but one desideratum, and that is a map, which we trust will be supplied should the book reach, as we hope it will, another edition.

T. G. BONNEY

HINRICHS' CHEMISTRY

The Elements of Chemistry and Mineralogy, by Gustavus Hinrichs, A.M. (Griggs, Watson, and Day, Davenport, Iowa, U.S.)

THIS is the second volume of Prof. Hinrichs' series of science instruction for schools. In the former volume the elements of Physics were given; this volume contains the elements of Heat, Chemistry, and Mineralogy, and will be followed by a third, called the "Students' Cosmos." The author has thrown himself entirely into the practical method of teaching the physical sciences—the student has first to perform an experiment, and then draw his own deductions from it. In chemistry, perhaps, more than in the other sciences, this system leads to the best results; for on all sides laboratory practice is recognised as essential to its true understanding. If, however, large laboratories and costly apparatus are required, its introduction in our schools cannot become universal. The author has, however, shown in this volume that by excluding special branches, a considerable knowledge of the elementary methods of laboratory practice may be furnished, almost free of charge, by any school to all its pupils. At a time when science instruction in our schools is attracting so much attention, a series of volumes like the present is peculiarly valuable, and thanks are due to Prof. Hinrichs for his bold effort to show how the physical sciences should be taught.

The subject "Heat" occupies two chapters. The first deals with the sources of heat, modes of heating, radiation and induction, thermometry, calorimetry, and fusing and boiling; the second treats of the relation of heat to mechanical work. These are extremely clear and practical; we think better than those which follow. The third chapter is on "Dissociation and Electrolysis." After a few examples of the splitting up of compounds into their elements have been studied, the student is led naturally to the definitions of elementary bodies, of compounds, and mixtures. The next chapter is confined to the elements and compounds, and some of the principles of chemical nomenclature; to this chapter we must certainly take exception, the author has introduced a novel and arbitrary classification of the elements, which, we think, will tend to confuse the student. He groups the elements into nine genera, giving the characteristic properties of each: thus we have the kaloids, analogous to potassium; calcoids, analogous to calcium; cuproids to copper, and so on; under the last head we find classed, copper, silver, gold. We cannot see any reasons for such grouping, for neither in their chemical nor in their physical properties do these three elements correspond.

The author divides chemical substances into monaries, binaries, ternaries, and serials; the monaries are the elements themselves, the binaries the compounds of two elements, the ternaries of three, whilst the serials comprise organic bodies; there is, however, no reason in the author's definitions why the greater number of the serials should not be classed under the ternaries. The term "serials" the author has taken from the fact that numerous organic compounds can be classed together to form series of substances, differing from each other by a definite increment. The next chapter treats of the synthesis of acids and bases, and chapter 6 is devoted to chemical processes. Under the head of "substitution," the quantitative relations of the elements to each other are brought out. We do not think, however, that the difference between the terms "atomic weight" and "equivalent" is by any means sufficiently defined. A considerable space is devoted to the phenomena of double decomposition and to the complex processes, such as fermentation, &c., which concludes the chemical portion of the work. A chapter on mineralogy follows, but on this it will be difficult to give an opinion, as the method the author uses is novel, but, according to his account, quite satisfactory. The book on the whole is most carefully written, so that the student cannot fail in his experiments provided he follows his instructions; these also are so given as to lead to economical and precise methods of working. At the end of the book a number of blank pages are left for the pupil to fill up with his notes of experiments performed, and results obtained, forming quite a new feature in this class of works. The chapter on the "Chemical School Laboratory" we should recommend to the notice of our teachers, as it gives a description of the author's system of teaching, which, we believe, has succeeded extremely satisfactorily in the case of physics, and, we trust, will be equally successful in chemistry.

OUR BOOK SHELF

The Figure of the Earth. By Archdeacon Pratt. (4th Edition. London: Macmillan and Co.)

THIS is the fourth edition of a well-known book, of which we shall unfortunately not now have any more new editions from the hand of its lamented author. The book has grown much since its first edition as a separate work in 1860. The chapter on the attraction of table lands, mountains, oceans, &c., has been much enlarged since the first edition, and also the chapter on the determination of the figure of the earth by geodetic operations. A chapter, most valuable to the student of physical mathematics, is inserted on the determination of the ellipticity of the earth (considered as a body whose surface is one of its own equipotential surfaces) from pendulum experiments, the moon's motion, and the precession of the equinoxes, respectively. The student of this subject must carefully bear in mind that no observations taken exterior to the surface of the earth can throw any light whatsoever on the internal arrangement of its matter, inasmuch as, according to the well-known theorems of Gauss, there are an infinite number of ways in which that matter might be conceived as being arranged so as to produce the same external effect. The observations above noticed, however, are calculated to throw light on the question as to whether the surface may, within the limits of approximation, be considered as a surface of equilibrium.

In fact, it is known that in any event the external effect of the earth may be precisely effected by the distribution in a concentrated form of the whole matter of its interior over its surface.

The important proposition that any function, which does not become infinite within the limits considered, can be expanded in a series of Laplace's functions, is proved by Mr. Pratt in the text by rather a long method, in order to get over a certain apparent objection as to discontinuity. The following proof of that proposition seems short, and not open to objection.

Let A and B be two points on a sphere of centre O. Let the co-ordinates of any point R referred to A be p and q, where p is the cosine of the angle between OR and OA, and q the angle between the plane AOR and a fixed plane. Let μ, ω, be similar co-ordinates of R referred to B, and let μ', ω', be the values which μ and ω assume at the point A. Let $F(\mu'\omega')$ be a function of μ' and ω', which, when μ' and ω' become μ and ω, may be written $F(\mu, \omega)$. If δS be an element of the surface of the sphere, whose radius we shall take as unity, then δS may be expressed by $-\delta p \,.\, \delta q$, or by $-\delta\mu \,.\, \delta\omega$, according as occasion requires.

It is obvious from spherical trigonometry that

$$p = \mu\mu' + \sqrt{1-\mu^2}\sqrt{1-\mu'^2}\cos(\omega-\omega')$$

and that therefore in the expansion

$$\frac{1}{(1+x^2-2xp)^{\frac{1}{2}}} = 1 + P_1 x + P_2 x^2 + \ldots$$

The quantities P_1, P_2 &c, satisfy Laplace's equations in μ and ω, and also in μ' and ω'.

Differentiating this equality, multiplying by $2x$, and adding, we get

$$\frac{1-x^2}{(1+x^2-2xp)^{\frac{3}{2}}} = 1 + 3P_1x + 5P_2x^2 + \ldots + (2n+1)P_nx^n +$$

Integrating each side of this equation over the whole surface of the sphere, and equating the results, we have

$$\int_0^{2\pi}\int_{-1}^{+1}\frac{1-x^2}{(1+x^2-2xp)^{\frac{3}{2}}}\,dp\,dq =$$
$$\int_0^{2\pi}\int_{-1}^{+1}(1+3P_1x+5P_2x^2+\ldots)\,d\mu\,d\omega$$

The first of these two integrals is readily found to be equal to 4π, being thus independent of x.

$$\therefore \int_0^{2\pi}\int_{-1}^{+1}(1+3P_1x+5P_2x^2+\ldots)\,d\mu\,d\omega = 4\pi$$

Now as x approaches unity, every term in the series whose limit is represented by the integral

$$\int_0^{2\pi}\int_{-1}^{+1}\frac{1-x^2}{(1+x^2-2xp)^{\frac{3}{2}}}\,dp\,dq$$

becomes more nearly equal to zero, except the terms in the immediate vicinity of the value $p = 1$, which increase in value, *i.e.* in the neighbourhood of the point A. Hence, as we diminish x, the ratio of

$$\int_0^{2\pi}\int_{-1}^{+1}F(\mu\omega)\left\{1+3P_1+5P_2x^2+\ldots\right\}d\mu\,d\omega \text{ to}$$
$$F(\mu'\omega')\int_0^{2\pi}\int_{-1}^{+1}\left\{1+3P_1x+5P_2x^2+\ldots\right\}d\mu\,d\omega$$

that is to $4\pi : F(\mu'\omega')$ becomes continually more nearly a ratio of equality, since $F(\mu'\omega')$ is the value towards which $F(\mu\omega)$ continually approximates as we draw nearer to the point A. Hence we have in the limit

$$4\pi.F(\mu'\omega') = \int_0^{2\pi}\int_{-1}^{+1}F(\mu\omega)(1+P_1+5P_2+\ldots)\,d\mu\,d\omega$$

and since P_n satisfies Laplace's equation of the nth order in $\mu'\omega'$

$$\therefore \int_0^{2\pi}\int_{-1}^{+1}F(\mu\omega)P_n\,d\mu\,d\omega$$

also satisfies it, because $\mu'\omega'$ are constants so far as this integration is concerned. Hence $F(\mu'\omega')$ is expanded in a series of functions satisfying Laplace's equations.

JAMES STUART

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Volcanoes of Central France

THE eruptions of A.D. 458-460, whose showers of pumice or ashes reached and alarmed the city of Vienne, then the capital of the chief State in Gaul, and led to the institution of the Rogations (now called Litany) and the "Rogation Days," cannot have proceeded from the province of Auvergne, as Mr. Green supposes (NATURE, May 16). That province, containing about half the French volcanoes, is the most distant of the three volcanic ones from Vienne, and moreover is held to have been quiescent in that age (as well as ever since); because the eminent writer, Sidonius Apollinaris, who had settled there, and wrote a poem on its scenery, betrays no knowledge of its volcanic phenomena. So, at least, Sir Charles Lyell has repeatedly insisted. It is true that, writing before the date of the Vienne calamities, his silence proves nothing; but as fully half the French craters are not in the Auvergne, but between that province and Vienne, namely, in either the Velay or Vivarais, within about fifty miles of that city, and ranged along almost a quadrant (the S.W. quadrant) of its horizon, there can be little doubt that some of them were the scene of the "portentous fires," and sources of the "Sodomitic showers" that alarmed the Burgundian capital, and led St. Mamertus to institute these fasts. Of Mamertus himself there remain no writings, and the memory of any historic eruptions in France appears to have died out from that very century to the present; though none in all history were better attested, none within many centuries of Pliny's even so well. For it is strange that no later chroniclers mention anything but the earthquakes and some fires of buildings; the sole authorities for the eruptions being their contemporaries, the above Sidonius and the bishop who succeeded Mamertus in his see, and these two being the sole men in Gaul of that generation whereof any document remains. The former writes to Mamertus himself a very fulsome, adulatory, but necessarily a materially true memoir of the facts; and the latter allusively recounts them in a sermon to the very flock among whom the observances had begun. It seems impossible to conceive better witnesses to any event whatever, and they are literally all the contemporary writers extant.

The late Sir Francis Palgrave appears the first modern to have disinterred this page of forgotten history, in the *Quarterly Review* of October 1844 See also a most extraordinary paper on it in the *Gentleman's Magazine* of May 1865, commented on by me in the *Reader* of the next month (p. 683). As the original passages, however, of Sidonius and Bishop Avitus have not been reproduced, I enclose literal translations of them, if you think they would interest your readers. The style of their time must be allowed to be detestable, but not quite without parallels.

E. L. GARBETT

"Sidonius to Lord Patriarch Mamertus, health! It is reported the Goths have advanced their camps on to Roman soil! To this kind of eruption we wretched Auvergnats are always the gate. For we afford to the enemies' malice peculiar satisfaction; because, as they have not yet marked their bounds from Ocean to Rhone by the course of the Loire, they (under Christ's mercy) find their sole hindrance from our opposition only. Indeed, the tracts and regions of the surrounding country the eager assault of their threatening power would long ago have devoured. But in this, our so bold and dangerous a resolution, we trust not in our hearts either to the crumbling face of ramparts, the rotten barrier care (*studium*), or the failing defence of sentinels for assistance, but are only soothed by the comfort of the Rogations introduced, of which you were the author; which, being to be founded and instituted, the Auvergne people has begun to practise, if not with equal result, certainly with not inferior zeal, and on this account does not turn its back to the surrounding terrors. For it does not escape our research (*latet nostram sciscitationem*) how, in the first times of these supplications being instituted, by the terrors of what manner of prodigies the city divinely committed to you was being emptied. For at one time the walls of the public fortifications were shaken down by the continual earthquakes; at another the fires, often attended with flame, were smothering (*tumulabant*) the frail roofs with a load of showered ashes (*superjecto favillarum monte*). Now the vast lairs (*stupenda cubilia*) in the forum harboured the boldness (O portentous tameness!) (*pavenda mansuetudo!*) of deer. When you, amid that flight of the nobles and the common people, the state of the city being desperate, quickly had recourse to new imitations of the ancient Ninevites, lest your despair, too, should mock the divine admonition. And truly at that time you could the least distrust God without sin, after your experience of his mercies. For once, when a certain city had begun to blaze, your faith had glowed more than the fire," &c. (He proceeds to relate the extinction of a former conflagration by the prayers of Mamertus; but this and the remainder of the letter, though translated by Colenso to throw ridicule on the people's religion, has evidently no bearing on the calamities and portents of A.D. 458-60, but refers to an earlier event)—*Epistles of Sidonius Apollinaris.* Book vii. Ep. I. (From Migne's *Patrologia*, tom. 57, p. 563)

"There pervades, indeed, the vital way (or faith) not through the lands of Gaul only, but almost the whole world, the fertilising stream of these Rogation Day observances, and cleanses the earth infected with vices by the wholesome flux of an annual expiation. More special reason, however, have we in this same institution for service and rejoicing, because from hence in a manner it flowed for the benefit of all. From our source at the first it spread ; and perhaps even (we may say) it pertains to some dignity or privilege, the first beginning of such an institution. At any rate, when an ineffable distress (*necessitas*) tamed down the proud hearts of our Viennese to this manner of humiliation, our Church, perceiving the cause of her chastisement (*ægritudinis*), caught to herself not as chiefly before all others, but as alone among all, feeling the need there was for the present observance to be instituted, far more eagerly a remedy than a primacy (or precedence). And, indeed, the causes of the terrors of that time, I know that many of us recollect well (*recolere*). For truly the repeated fires, the frequent earthquakes, the mighty noises, threatened to add to such a cremation (*cuidam funeri*) of a whole world some equally prodigious entombment (*bustuale*). For in the populous haunts of men the tame appearance of the beasts of the forests was observed ; God knows whether deluding our eyes or driven there by the portents. But whichever of these two it might be, it was perceived to be alike monstrous, whether thus in reality the wild natures of the beasts were tamed, or whether so frightfully in the views of the spectators phantoms of false visions could be formed. Amid these things various were the notions of the populace, and divers the opinions of different classes. Some, concealing what they felt, ascribed to chance what they would not allow to (be matter of) weeping. Others, of healthier mind, discovered truly the new iniquities (*abominabilia*) aptly agreeing to the natures and significance of the ills. For who, in the oft-seen fires, would not dread Sodomitic showers? Who, in the shaking elements, would not believe either falls of roofs (*culminum*) or openings of the earth to be at hand? Who, when seeing, or certainly thinking he saw, the naturally timid deer advancing through the straits of doorways, even to the sides (colonnades) of the forum (*ad fori latera*), would not presage an impending doom of desolation?" (He then recurs, like Sidonius, to the story of the earlier conflagration of a palace or town hall, arrested by Mamertus, which leads to the confusion of these two calamities by all later chroniclers, and loss of memory of the eruptions, and showers of favillæ)—*Homily of Avitus concerning Rogations.* "*How the Custom of the Rogations arose.*" (*Migne Patrologia, tom.* 59. *p.* 289.)

SOME years ago my attention was especially directed to the date of the latest eruptions in Auvergne, as usually supposed to be indicated by the appointment of the Rogation Days, A.D. 469, by Mamertus (rather than Mamercus), Bishop of Vienne. A reference to original authorities convinced me that there is no satisfactory evidence of anything beyond long-continued earthquakes of such severity as to drive the wealthier part of the population out of the city, and, as it would seem, the wild beasts into it. Much is said about fire, but the rhetorical and inflated expressions of those living nearest to the event may be applied to either volcanic or domestic conflagration; and there is great reason to believe that the latter only was intended, in the apparent absence of volcanic foci in the neighbourhood. These, according to Scrope's map, all lie at a considerable distance (if I recollect aright, twenty or thirty miles); and though it is of course possible that the site of some nearer outburst may have been hitherto unnoticed, the expressions used hardly warrant the trouble of any laborious search for it. Should any of the residents in the neighbourhood of Vienne be conversant with geology, they would be able to furnish decisive evidence on the subject. The original story is a curious one, but it has not lost in the telling.

Hardwick Vicarage, May 25 T. W. WEBB

The Approaching Transit of Venus

IN NATURE of the 4th of January last Mr. J. Carpenter gives an interesting sketch of the arrangements in progress for observing the forthcoming Transit of Venus. He states that French and German astronomers have decided on establishing a station of observation at Muscat (Mascate) or at some place between that nasty little port and Teheran. Now, as a point along this line is considered so favourable by Continental astsonomers, will you allow me through your pages to call Mr. Airy's attention to the peculiar advantages of Jask in this respect. Cape Jask, on the Mekran Coast, is situated, roughly, in lat. 25½ N. and long. 57½ E. We have here a large and intelligent English telegraph staff, and work a double line of telegraphs to Europe. We have three large stone-built bungalows (houses) with strong, flat, cement-covered roofs, which are approached by spacious staircases. The large bungalow, forming the clerks' quarters, is about 250 feet long, 20 high, and 40 broad. It is divided in the centre by a sort of tower, in which are situated the stairs leading to the roof. The latter would be a most convenient place for erecting the astronomical instruments, &c. There is no telegraphic communication with Muscat, and it is about two days' sail, with a fair wind, from Jask, which is the nearest telegraph station. Should the Astronomer Royal decide on sending out a couple of observers here, I promise them a hospitable reception and every assistance. The fortnightly mail steamers between Bombay and Bussorah, pass within fifteen or twenty miles of this place, and could be easily induced by Government to call in and land the party.

Mr. Latimer Clark, who visited this station towards the end of 1869, will, I daresay, if called upon, be able to give some further particulars, and can vouch for the accuracy of my statements.

J. J. FAHIE

Persian Gulf Telegraph Dept., Jask Station

Recent Climatic Changes

MR. HOWORTH's letter on "Recent Climatic Changes" in NATURE of the 9th May, is most instructive and interesting, more especially to those who have visited the Arctic Sea ; but on one point I must venture to differ from him, that is, when he expresses his belief that the Esquimaux migrated from the northward in consequence of the increasing rigour of the climate in high northern latitudes.

I have seen the Esquimaux at the mouths of the MacKenzie and Coppermine Rivers and at Repulse Bay in longitudes 135°, 115°, and 87° West, respectively. At all these places I found their traditionary belief to be, that they came originally from the west, across a narrow sea (probably Behring Strait), followed the coast line eastward, then southward along the west side of Hudson's Bay; some of them making their way to the east coast of that great bay and to Labrador by crossing the comparatively narrow channels separating these places from Southampton, Mansfield, and other islands, at the entrance of Hudson's Bay.

As Victoria and Wollaston lands, and other places still farther north, were probably at that time (as some of them are at present) well stocked with game, part of these people in their eastward drifting would naturally turn to the northeastward, until they reached North Lincoln and Ellesmere lands in lat. 77° or 78° North, from which they probably crossed Smith Sound to Greenland, along the west shore of which they would then have gradually spread southward.

Thus the Skrellings who destroyed the Norse colonists of South Greenland, came, as Mr. Howorth says, from the north. Indeed, they could not have come from any other direction, except by making a long sea voyage, for which their frail craft (if they had any canoes at that time) were by no means well fitted.

That the "Saga" writers knew that Esquimaux were to be found in Labrador before they were seen in Greenland, goes far, I think, to support the view I have expressed; because, if coming from the west, they could much more easily and speedily reach Labrador than the southern parts of Greenland; whereas had they come originally from the north, the facilities for arriving at these places would have been reversed.

I have been told by one of the greatest authorities, perhaps the very highest, on such subjects, that it is not likely that the Esquimaux originally came from Asia, as the form of their heads differs most materially from that of the heads of those Asiatics whom in other respects they most resemble.

This seems almost an unanswerable fact or argument against the correctness of the tradition of the Esquimaux, and the theory I have advanced, which very likely may have no novelty in it.

In opposition to this very strong fact, may I suggest the possibility that the change in the mode of life of the Esquimaux in their new country (to which they were probably forcibly driven), and a change in the manner of carrying the child in infancy, may have caused a material alteration in the form of the head, whilst other peculiarities of face and form remained nearly the same.

The Esquimaux infant is carried in the hood of the mother's coat, and its head is perfectly free, with no pressure on one part more than another.

The peculiar form of head of the Red Indian of America may in a greater or less degree be caused by the kind of cradle used. The baby is fastened in its cradle in such a manner that the whole weight of the little creature's head rests almost constantly on the back part or occiput: the effect would be to reduce the longitudinal and increase the lateral diameter of the skull.

Of course I do not allude to the intentional and artificial alteration of the form of the skull, as practised among the Chenooks.

The discovery of many reindeer and muskcattle by the Swedish (? German) expedition on the East Coast of Greenland, as mentioned by Mr. Howorth, where previously none had been seen, may be accounted for by these animals—after having been much hunted and harassed by the natives near Smith's Sound and Melville Bay, on the north-west extremity of Greenland—migrating to the east shore, where, finding food and rest, they remained unmolested to increase and multiply, which they do very rapidly under such conditions.

Deer, muskcattle, and hares were found in great numbers, and very tame and in good condition on the Parry Islands, in latitude 76°, and on Banks Land in latitude 74° N., by several of the recent Arctic expeditions, and these do not appear to have migrated southward in the autumn. I have no doubt that were those localities visited by a band of hunters, these animals would after a few years become shy and timid, and finally move off to a more safe position, either north or south, as their own instinct or the trending or nature of the country might lead them. This I have known to occur frequently in America farther to the south.

These sudden and unforeseen migrations (being an exception to the usually very regular habits of the animals) are among the chief causes of the suffering and deaths by starvation among the Esquimaux.

Although what I have written above has been the result of my own observation, it may have been spoken or written by some one else before, much better than I can pretend to do. If so this communication will find its way, as it deserves, to the waste-basket.

JOHN RAE

A Scientific "Bone-Setter"

THE interesting article on "Bone-Setting" in NATURE for May 9 induces me to narrate my own experience. More than twenty years ago, in the city of New York, while swinging upon parallel bars in the gymnasium, I fell backwards, and to save my head threw out my left arm, thus catching the fall upon the palmer end of the radius, and, as it proved, fracturing the neck of the radius at the point of articulation with the ulna. I sent for one of the most eminent surgeons, then Professor and surgeon to a large hospital, but several hours elapsed before his arrival; and by that time the swelling and inflammation at the elbow had all the appearance of a sprain, and the fracture was not detected. Some days afterwards the surgeon discovered that there had been a fracture, and that a false adhesion had begun. This was broken up, and the arm set in splints, according to the approved method. After the usual time the bandages were removed, but the forearm was incapable of flexion, extension, or rotation. Every appliance was used to restore it to its normal condition, such as lifting, friction, sponging, &c., but without effect. The arm became useless, and began to shrivel. It was examined by the first surgeons of New York and other cities. Some thought that the radius had adhered to the ulna, others that there was a deposit of interosseous matter, but none could suggest a remedy.

Nine months after the accident I chanced to be in Philadelphia, and called upon Dr. Rhea Barton, who, though he had retired from practice, consented to look into my case. After careful examination, he said, "If you will consent to suffer the pain (it was before the use of chloroform) I will agree to restore the arm." He went on to say that pressure demonstrated a slight crepitation at the joint, and also a slight elasticity; and this assured him that the trouble was in the ligaments; that in consequence of the long imprisonment of the arm in splints, while under inflammation, a ligamentous adhesion had taken place, and the synovial fluid had been absorbed. He then applied one hand firmly to the elbow and the other to the palmer extremity of the radius, and, diverting my attention by anecdote and wit, thus relaxing the resistance of the will to pain, he gave a sudden wrench, there was a sound like the ripping of cotton cloth, and the arm lay outstretched before me, quivering with pain, but capable of motion. Mechanical appliances for a few weeks so far completed the restoration that I have ever since had about four-fifths of its normal use and power.

Now, Dr. Barton did, upon scientific knowledge, what the "bone-setter" does empirically—"by manipulation, suddenly and forcibly tearing asunder the adhesions" formed between the ligaments and the bone; and he assured me that the whole difficulty would have been averted had the arm, when under treatment for fracture, been gently moved at times according to nature. I think he has published a monograph upon this point, but I cannot now refer to it.

JOSEPH P. THOMPSON

Berlin, May 22

Pathological Legends

MR. TYLOR speaks of vampires as illustrations of Savage Animism, and regards them as inventions to explain wasting disease. The records of such unseen agents point to two classes of vampires, one of which has nothing to do with wasting disease. To take two extreme cases: the story of Grettir's conflict with Glam the house churl, contrasts with the Vampire Cat of Nabeshimes, as told by Mitford in the "Tales of Old Japan." The Northern hero seeks the evil one and overpowers him, but his success is dearly bought, for evil temper and nervousness never leave him, and his after life is unlucky from these two causes. The Japanese Prince is visited nightly by the counterfeit of his lovely concubine, he pines away, and is only saved by the energy of a retainer, who slays the fair persecutor. Here are types of two kinds of malady; one is truly wasting, the other is of that kind which ends in apoplexy, epilepsy, acute mania, or if death is not speedy and sudden, dyspepsia may reduce the hero to Grettir's state without obviously impairing his strength. The Japanese story gives the common superstition among polygamous people with whom progressive exhaustion is not uncommon, as "Hawke's Voyages" quaintly explain. The Grettir Saga gives a pagan version of what figures more than once in Christian legends as saintly intervention. Thus, the Scandinavian invader blasphemes the English saint, who straightway appears to him, and points the finger; the blasphemer drops down dead. Glam, the churl, gorges himself with food, and goes to the hill, the next morning he is found crushed and distorted, and the horror of his punishment is proportioned to his crime, for he ate meat on a Church fast, and it was doubtless sweet to his neighbours to recall the fact that they heard his shrieks when sitting in church. Glam's successors perish violently, one of them being found convulsed and broken on Glam's cairn, just as in more places than one in Scotland men have been found in convulsions near places which superstition had made terrible on account of some great crime. But Grettir, for twenty years after his fight with Glam, leads a life of incessant fighting as an outlaw. He cannot go alone, his nerve is shaken, he sees things in the dark, and his temper is irritable. It is of course impossible to separate out the various forms of unseen agency to which men in rude times were subject. But the Vampires of the North and Incubi are members of the same family; the Vampires of Asia belong to another family. The former represent indigestion, the results of gross overeating and drinking, aggravated, doubtless, by the circumstances that the opportunities of excess were not frequent, and that semi-starvations occurred often between copious meals. The demons are mostly men; in all cases they give rise to violent conflicts, in which, if a man dies, his distorted convulsed body suggests the presence of a corporeal enemy, a reasonable enough notion among those to whom natural death meant, in the case of a strong man, death by the sword. The latter represents the results of lechery in some form or other; there is no tale of conflict, though now and then sudden death is accompanied by convulsions such as, we know, frequently terminate cases of general paralysis and *Tabes dorsalis*. The correspondence between the Northern Berserker and the furious Malay who runs amuck, is interesting in reference to this contrast. The insanity of the Berserker is that of an individual; the persecution of the Northern vampire falls on the whole family of the sufferer; and, while it is difficult under ordinary circumstances for any large number of people to become simultaneously affected by genuine fury (though Mr. Wallace furnishes a possible analogy to ancient custom in what we may call a sociable Amok), the contagion of fear makes it easily intelligible how even a district might come to see and hear what had no existence save in the disturbed imagination of one. The Incubi and Suscubi of the Middle Ages in Europe may be paralleled at the present day in asylums, and, now as then, are met with among those who have placed themselves in conditions similar to the unhealthy ones of the Convent.

In reading over the confessions of the witches in England and Scotland, it is strange to find how exactly the language employed expresses the frequently described sensations of women labouring under uterine and ovarian disease. Doubtless, not all thus suffered; but the confusion once made and heard would be repeated by the unhappy imbeciles from whom chiefly the witch ranks were recruited. JOHN YOUNG

Glasgow University

The Vervain and Yellow Fever

ALLOW me to say a few words on the report of the English Vice-Consul at Ciudad, Bolivar, concerning the efficacy of the Vervain plant as a remedy for yellow fever and black vomit (NATURE, March 21, p. 412). The plant in question is *Stachytarpha jamaicensis*, Vahl, a very common and rather troublesome weed, called in Spanish America "Verbena," in the British West Indies "Vervain," and in the Brazils "Gervaô." Its medicinal properties have been greatly exaggerated, though it is certainly somewhat aromatic and astringent; but in yellow fever and black vomit its efficacy is next to nothing. For a good description of the plant and some notes on its supposed and real virtues, I refer to Auguste de St. Hilaire, "Plantes usuelles du Brésil," plate 39 (Paris, 1824). Another tolerably good figure is given by Sloane, "The Natural History of Jamaica," plate 107, 1, who mentions several diseases against which it was used in his times (and probably still), stating finally, that "it is good against charms" A. ERNST

Caracas, Venezuela, May 6

ELECTRIC VALENTINE

TELEGRAPH CLERK ♂ TO TELEGRAPH CLERK ♀

"THE tendrils of my soul are twined
With thine, though many a mile apart;
And thine in close-coiled circuits wind
Around the magnet of my heart.

"Constant as Daniell, strong as Grove;
Seething through all its depths, like Smee;
My heart pours forth its tide of love,
And all its circuits close in thee.

"O tell me, when along the line
From my full heart the message flows,
What currents are induced in thine?
One click from thee will end my woes."

Through many an Ohm the Weber flew,
And clicked this answer back to me—
"*I am thy Farad, staunch and true,*
Charged to a Volt with love for thee."

$\frac{dp}{dt}$

[NOTE BY THE EDITOR—

Ohm = Standard of resistance.
Weber = Electric current.
Volt = Electromotive force.
Farad = Capacity (of a condenser).

$$\text{Velocity of Puck, } \frac{\text{Once round the Earth}}{\text{40 minutes.}}$$

$$\text{,, of Ohm, } \frac{\text{Quadrant of meridian of Paris}}{\text{1 second.}}$$

$$\therefore \text{1 Ohm} = \text{600 Pucks.}]$$

NATURAL SCIENCE AT OXFORD

IT has been resolved in Convocation that the Curators of the University Chest be authorised to pay to the credit of the Museum Delegates the sum of 1,000*l*., to be employed at their discretion for the maintenance and improvement of the Collections in the Museum; a full statement of the expenditure for these purposes being prepared annually and reported to Convocation.

MAGDALEN COLLEGE.—*Demyships and Exhibition.*—There will be an election at this College in October next to not less than Six Demyships and One Exhibition. Of the Demyships, one at least will be Mathematical, one at least in Natural Science, and the rest Classical. The Exhibition will be in Mathematics, is of the value of 75*l*. per annum, inclusive of all allowances, and is tenable for five years.

EXETER COLLEGE.—There will be an Election to a Natural Science Fellowship in this College on Wednesday, June 10. The Examination will be in Biology. The Fellow elected will be required to reside and take part in the instruction of the College. The election will take place under the conditions of the following special ordinance of the College:—"Any Fellow who shall be elected previous to June 1874, with the declared purpose of taking part as Tutor or Lecturer in the College, shall *ipso facto* vacate his Fellowship on ceasing to reside. Provided also that if the said Fellow shall have taken part as Tutor or Lecturer in the College for seven academic years, consecutively or not, or for part of the time in one office and part in the other, he shall retain his Fellowship, subject only to the other causes of avoidance of Fellowship. Any fellow so elected shall hold himself bound, on pain of the loss of his Fellowship, to take part, if required, as Tutor or Lecturer in the College. If any such Fellow be incapacitated through ill health for educational work in the College, it shall be competent for two-thirds of the Governing Body, with the sanction of the Visitor, to dispense with the required residence during the continuance of such ill health." The Fellow elected under the ordinance will be subject in all other respects to the Statutes of the College. The Examination will begin on Tuesday, June 11, and no person can be admitted as a Candidate who has not passed all the Examinations necessary for the degree of Bachelor of Arts in the University of Oxford, or been incorporated as a graduate in the University.

SECOND PUBLIC EXAMINATION.—*Pass School.*—In pursuance of the statute, the Board of Studies for directing the Examination of Candidates who do not seek Honours at the Second Public Examination, and also the Examination in the Rudiments of Faith and Religion, gives notice that the books and subjects which may be offered in the Easter and Trinity Terms 1874, and until further notice, are—

In Group C.—(1) The Elements of Plane Geometry, including the doctrine of similar triangles. This includes the portion of Geometry treated of in Euclid Books I.-IV., with the definitions of Book V., and such parts of Book VI. as treat of similar triangles. These subjects may be read in any other treatise. The Elements of Trigonometry, including the trigonometrical ratios of the sum of two angles, the solution of plane triangles, the use of logarithms, and the mensuration of plane rectilinear figures. (2.) The Elements of the Mechanics of Solid and Fluid Bodies, including the composition and resolution of forces, centre of gravity, the simple machines and the application of virtual velocities to them, the laws of motion, the laws of falling bodies, the motion of projectiles, the pressure of fluids on surfaces, the equilibrium of floating bodies exclusive of the theory of stability, the methods of determining specific gravities, the laws of elastic fluids, simple hydrostatical and pneumatical machines. (3.) The Elements of Chemistry, with an elementary practical examination. Candidates who intend to offer this subject for examination are recommended to read that part of Roscoe's "Lessons in Elementary Chemistry" which treats of Inorganic Chemistry (pp. 1-268, new edition, 1869). The practical examination will be in the following subjects as treated of in Harcourt and Madan's "Exercises in Practical Chemistry":—1. The preparation and examination of gases (pp. 59-107); 2. The qualitative analysis of single substances (pp. 247-300; see also sections IV. and V., omitting that which relates to substances or properties of substances not referred to in the Analytical Course). (4.) The Elements of Physics. Candidates offering themselves for examination in this subject will be expected to show an acquaintance with Part I., together with any two of Parts II., III., IV. of the following treatise;—"Elementary Treatise on Natural Philosophy," by Deschanel. Translated and edited by Prof. Everett. Part I. Mechanics, Hydrostatics, and Pneumatics. Part II. Heat. Part III. Electricity and Magnetism, of which chapter 39 may be omitted. Part IV. Light and Sound (which will be published in a few weeks).

THE MURCHISON CHAIR OF GEOLOGY

UPWARDS of a year ago we duly chronicled the founding of a chair of geology and mineralogy in the University of Edinburgh by Sir Roderick Murchison, and we augured that the munificence of the founder would not be long in bearing fruit. It is pleasant to learn that the first session has been concluded successfully, and that the class has been greatly larger than the most sanguine friends of geology in Scotland had anticipated. In addition to the ordinary lectures of the class-room, there have been frequent afternoon excursions to the field, where the principles of the science have been learnt in a way in which they cannot be from mere lectures or books. Edinburgh is peculiarly favoured by nature for instruction of this practical kind. The crags and ravines which surround, or even stand in the midst of, the streets and gardens furnish admirable models of many of the more important and striking facts of physical geology. These advantages have been fully made use of during the past winter and spring. There has been, we are told, a brisk sale of geological hammers, and bands of hammerers have been seen on Saturday afternoons wandering over hill-side and quarry. At the close of the session Prof. Geikie and his students celebrated the termination of their labours together by a week's holiday in the island of Arran. For such an excursion good weather is the first grand essential, and in this respect the party appears to have been singularly lucky. The days were bright and bracing, so that from the highest hill tops the eye could wander over all the wide expanse of firth and fell which lies between the mountains of Jura and the far-off faintly-seen uplands of Galloway.

It was chiefly to the northern half of the island that the attention of the excursionists was devoted. They traced, of course, the well-known and often-described features—the granite mountains and veins, the schists, the trap-dykes, the carboniferous sandstones, conglomerates, limestones and tuffs, the raised beaches, &c. But they noted some points which deserve, perhaps, more special remark than has yet been accorded to them, and of these we have been furnished with the following jottings:—

1. Some interesting observations were made on the relation between the joints of the granite and the forms of surface into which that rock has been wasted. Everybody who has seen the Arran mountains remembers their sharp serrated ridges and deep corries. It was noticed in all the examples which were visited that each knife-edged crest coincided with the intersection of two sets of joints dipping in opposite directions, as the ridge of a roof coincides with the line along which the two opposite slopes meet. Where the one set of joints differed most in angle of inclination from the other, there was seen to be a corresponding difference in the slope on two sides of the crest, the highly inclined joints having a steep face, sometimes quite a precipice, on their side, while the less inclined joints had a gentler declivity on the other. From the summits it seemed as if the changes in the direction and inclination of the granite ridges were largely due to changes in the trend and slope of the systems of cross-joints. But there was not time to work out this problem.

2. Some mineralogical and petrographical facts of interest were gleaned. The passage of the Arran pitchstone was traced into a dull pearlstone which appeared to be closely connected with, if, indeed, it did not pass into one of the pale compact felstones or "compact felspars." The common association of pitchstone with the tertiary volcanic rocks of the west coast, and its entire absence from any of the abundant palæozoic volcanic masses of the mainland, raises the suspicion that perhaps the Arran pitchstones are likewise of tertiary date. The association of these pitchstones with some of the characteristic felstones or porphyries of that island also suggests as late an origin for the latter. Some facts, indeed, were noted, which, if properly worked out, might throw light on this question. It was observed, for example, that in the picturesque columnar ridge above Corriegills the columns are so arranged as to indicate that the mass of rock flowed along and consolidated in a trough or hollow. Was this hollow a valley carved out of the denuded surface of the carboniferous rocks, and did the porphyry flow into it as a *coulée?* A phenomenon of rare occurrence was noted in this Corriegills porphyry. Usually the quartz in such rocks exists merely as irregularly-shaped blebs or grains. In this rock, however, it is crystallised, and frequently appears in little doubly-terminated pyramids. Some of the party spent half an hour in gathering up perfect crystals from the weathered hollows of the rock. These crystals do not occur in amygdaloidal cavities but as essential constituents of the rock. In the Goatfell granite some cavities were found with well crystallised quartz, and one of the party was fortunate enough to light upon one cavity from which he obtained a handful of small cairngorms.

3. The moraines of the Glen Cloy afforded a pleasant afternoon. It was matter for surprise to some of the party that amid all that has been written about Arran these truly remarkable moraines have not received more notice. It is true, they do not lie among the group of the higher mountains of the island, and they have not the magnificent setting around them which they would have had if they had stood in Glen Rosa, or Glen Sannox, or Glen Iorsa. But in none of these glens, even though they plunge into the very heart of the central mass of granite, is there anything in the way of moraines at all to compare with the huge concentric mounds of rock-rubbish cumbered with blocks, which roughen the bottom and sides of the deep recess in which the upper part of Glen Cloy terminates. The plateau which served as the snow-field whence the Glen Cloy glacier was fed rises to an average height of only about 1,400 or 1,500 feet above the sea, while the neighbouring granite peaks are about twice as high. Yet the higher granite mountains have afforded comparatively few and small moraines. It gives a good notion of the severity of the climate during the glacial period to reflect that the little isolated patch of elevated ground, forming now the island of Arran, was large enough to nourish, even on its lower plateaux, snow-fields and glaciers.

4. Many striking lessons were learnt regarding some of the broad aspects of atmospheric denudation. Particularly were these lessons brought home to the mind among the wasted crags and corries of the granite mountains. Granite which, in the popular creed, is regarded as one of the most imperishable of rocks, was seen to be covered

with acre upon acre of its own wasted *débris*. On the crests of the heights the rock was found to be split by frosts along its joints; numberless blocks had fallen off, and the slopes below were thickly strewn with them. On smoother declivities, such as those that descend from the lonely and barren Beinn Bharrain, each casual torrent was seen to have ploughed out of the loosened and corroded granite a deep trench, which in time might get widened and deepened into a lateral valley. The waste of mountains could not be more eloquently revealed. As the party in long file threaded its way through these solitudes, a feeling which had been growing all day found at last expression, and as the scattered scouts who, in the exuberance of young life, had dispersed up and down hill on either side of the main body, gathered together into one merry company at the edge ot the dark and lonely Corrie au Lachan, it was unanimously agreed that had this band of stone-breakers seen nothing more than these proofs of how a mountain may be sculptured, the lesson was worth all the trouble and fatigue of the excursion.

NOË'S THERMO-ELECTRIC BATTERY

A SUPPLY of dynamic electricity is almost as requisite now for the lecture table as the supply of gas or water. The decomposition of water and various other liquids, the decomposition of certain gases in Hoffmann's U tube, with the aid of Rhumkorff's coil, and the physical test afforded by the passage of electricity through vacua containing traces of different gases, are most constant lecture experiments. And no wonder: the brilliant purple light afforded by the passage of electricity through a nitrogen vacuum, is, perhaps, the best and most reliable, if, indeed, it be not the only, test for nitrogen gas; while the decomposition of water gas, of ammonia, and of marsh gas, are experiments of the utmost importance in modern Chemistry. Hitherto the chief drawback has been the voltaic battery; the setting up of the battery before the lecture, the taking it to pieces afterwards, the constant amalgamation of the zinc plates, the consumption of zinc and acid, the fumes—in a word, the general inconvenience inseparable from any form of voltaic battery, but reduced to a minimum in Sir William Thomson's constant gravitation battery. The former of these inconveniences are more apparent, when, as is often the case, the battery is only required for five minutes during the whole lecture.

Hence, when a statement recently appeared in Poggendorff's "Annalen," to the effect that a thermo-electric battery of great power had recently been constructed in Vienna by Franz Noë, we were glad to take the first opportunity of trying to what extent it could replace the ordinary voltaic battery for the lecture table. It was stated that the battery could readily decompose water, work a Rhumkorff's coil, and powerfully excite electro-magnets. As we understand from the maker that the battery about to be described is the only one in use in this country, it may be of interest to give a short account of its capabilities.

The battery consists of eighty elements, which are heated by eighty small Bunsen burners, the cooling of the opposite extremities being effected by broad, blackened sheets of copper, which of course radiate freely. The negative metal is a silver-white alloy drawn into wire, and partially enclosed in small copper cylinders to protect it from the direct action of the flame. The positive metal is a dull grey alloy extremely crystalline and brittle, and is cast into cylinders abont 20 mm. long by 7 mm. diameter. The composition of both these alloys is kept secret. Alternate pairs are separated by small square plates of mica. The elements can with readiness be combined either for quantity or intensity. Thus the eighty elements may be used together in one series, or in two series of forty elements, or in four series of twenty elements.

The battery under consideration differs somewhat from that described in Poggendorff; it is larger; a sliding double groove keeps the burners in one position, exactly midway between the double rows of elements; and arrangements have been introduced in order to prevent radiation from the sides of the flames to the blackened copper radiators, that is, to the cool end of the elements.

According to the maker, the battery is equal to "8 grosse Daniell'sche Elemente." The electromotive force of one element, according to M. Von Waltenhofen, was found to be equal to 1·24 to 1·36 Jacobi-Siemens's unit, while a Daniell's cell (no dimensions given) is equal to twelve of these units. One Noë's elements is said to be equal to 11 of the ordinary bismuth-antimony elements.

On testing the battery of eighty elements, we found it to be somewhat weaker than we expected. It is, however, very difficult to know to what extent the elements may be safely heated. No definite directions are given on the subject by the maker, and as the composition of the alloys is kept secret, one is unable to ascertain their fusing point without sacrificing a pair of elements.

With large iron electrodes placed in a solution of very dilute potash, water was decomposed by the battery at the rate of 22 cub. centimetres per minute. With small platinum electrodes exposing less than one square centimetre of surface in water rendered acid by dilute sulphuric acid, 10 cc. of the mixed gases were evolved per minute. One of Gaiffe's induction coils, capable of giving a spark 15 mm. long, was connected with the battery. A copious stream of sparks 11 mm. long was produced. With a small 6-inch electro-magnet, surrounded by only two coils of wire, the battery produced a portative force of more than 20 lbs., when the gas was turned down so as to be nearly level with the orifice of the burners, and the flame was distant some 9 mm. from the surface of the elements. It is very possible that the battery might be heated to a higher temperature than that employed for decomposing water without detriment. In no case did the upper cylinders glow with even a faint red heat.

Here then we have an instrument which at a moment's notice can be set in action, which consumes a small amount of gas, is tolerably portable, and which is sufficient for all ordinary electro-magnetic experiments, for lecture demonstration of the decomposition of water, ammonia, &c., and for many purposes for which a voltaic battery has now to be used. Four or six of these batteries would be sufficient for all purposes save the electric light; they might be made more compact, and could with ease be placed beneath and at one end of the lecture table. The lighting of a row of gas jets would thus furnish us at any moment with an abundant current of electricity. The battery is, indeed, less strong than one could wish, but the production of it is a step in the right direction; and we look forward to the time when powerful and compact thermo-electric batteries will be found in every lecture-room, and when the lighting of a row of gas jets will, through the medium of such batteries, furnish us at any moment with a powerful electric light.

We are at the outset of invention in this direction; several improvements in Noë's battery seem to us to be both advisable and practicable. Let the elements be enlarged, the brittle cylinders of alloy protected, the radiation of heat from the sides of the row of gas jets to the cool ends of the elements entirely prevented, which is by no means the case now; and, if possible, let a current of cold water flow through pipes interspersed among the cooling plates. Again, let the pressure and amount of gas be indicated, and let the stop-cock admitting the gas be furnished with a projecting pin, moving on a graduated dial, so that any desired amount of gas (pre-determined) can at any time be caused to issue from the burners, and thus any desired strength of current (pre-determined in reference to the heating effect by ordinary electrometrical means) be obtained.

G. F. RODWELL

ON THE MEASUREMENT OF MUSICAL INTERVALS*

IN a series of communications to the *Académie des Sciences* (February 8 and 22, 1869, July 17, 1871, and January 29, 1872), M. Cornu and I have shown that musical impressions are based upon several systems of musical intervals. We were also able to announce, as a preliminary result of experiments not yet completed, the following propositions, which, while they show clearly the origin of discussions that have gone on for more than two thousand years, appear to be capable of putting an end to these discussions, by reconciling the two contrary opinions which have always been entertained upon this subject.

1. The musical intervals formed by the successive sounds of a melody without modulation, belong to the Pythagorean scale, the degrees of which are represented by the following ratios, containing only the factors 2 and 3†:—

$$\begin{array}{cccccccc} \text{do} & \text{re} & \text{mi} & \text{fa} & \text{sol} & \text{la} & \text{si} & \text{do} \\ 1 & \frac{3^2}{2^3} & \frac{3^4}{2^6} & \frac{2^2}{3} & \frac{3}{2} & \frac{3^3}{2^4} & \frac{3^5}{2^7} & 2. \end{array}$$

2. The intervals formed by the simultaneous sounds of the concords, which are the basis of harmony, belong to very different systems, depending upon the complexity of the cords. Those which form part of the simpler concords of two or three sounds, thirds, sixths, perfect concords, &c., may be included in the scale given in all treatises on physics, the degrees of which are represented by the following ratios formed by the factors 2, 3, and 5:

$$\begin{array}{cccccccc} \text{do} & \text{re} & \text{mi} & \text{fa} & \text{sol} & \text{la} & \text{si} & \text{do} \\ 1 & \frac{9}{8} & \frac{5}{4} & \frac{4}{3} & \frac{3}{2} & \frac{5}{3} & \frac{15}{8} & 2. \end{array}$$

To demonstrate these propositions several conditions require to be fulfilled.

In the first place, in the two scales above given, the three different intervals, viz., the major third *do—mi*, the sixth *do—la*, and the seventh *do—si*, differ from one another by the interval called a "comma," the value of which is $\frac{81}{80}$, as will be found on dividing one by the other the fractions which represent these intervals on the two scales. Now this value of the comma is very small, though very perceptible to the ear; to demonstrate it we must, therefore, seek the assistance of skilled musicians, and employ apparatus of considerable delicacy.

Secondly, to measure the intervals formed by successive sounds it is best to study these intervals not separately, but as they occur in the actual course of a melody. Consequently, if we employ as our means of measurement the process which consists in causing the sounding body to trace out its own vibrations (and in the present state of science no better method can be adopted), we must have the means of inscribing continuously the sounds which constitute fragments of melodies as they are executed upon an instrument.

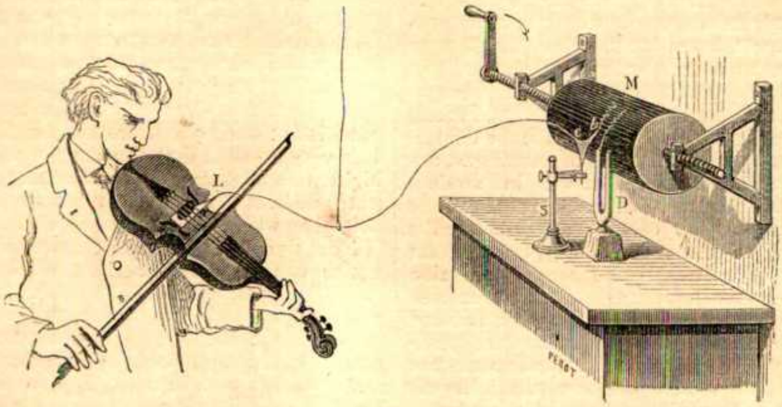

FIG. 1

Lastly, it is clearly necessary that the registration of the vibrations shall be automatic, and independent of the volition of the observers. The player must have nothing to do with it; he must not even see it going on, so that his attention may be entirely devoted to the music which he is playing.

After many trials we have succeeded in fulfilling these conditions. The apparatus which we use is very simple, the elements of it being found in every physical cabinet. It will, therefore, be useful to describe it.

Experiment shows that a metallic wire of steel, copper, brass, &c., without tension, and merely supported in such a manner that its vibrations may be executed freely, transmits to one of its extremities, by transverse vibrations, the sounds emitted by a sonorous body fixed to the other extremity. To show this it is sufficient to take two tuning-forks having mirrors attached to them, and tuned exactly in unison. Fix the end of a wire to one of them, and attach to the other end a feather carrying a shining point placed in front of the mirror of the second tuning-fork. On setting one of the forks in vibration, and properly adjusting the feather, the shining point is seen to describe an ellipse characteristic of the unison, and varying in form when a weight however small (a little piece of wax for example) is attached to the tuning-fork fixed to the wire.

A wire five, six, eight, ten, &c., metres long, suspended by narrow strips of caoutchouc, is soldered at one end to a small plate of brass, L, placed between the sounding-board of a stringed instrument and the foot of the bridge, the other end being slightly clasped to a heavy stand S. Near the fixed point a small piece of tinsel (*c*) is soldered on, and to this is attached a feather (*b*), by means of a little soft wax (by this arrangement a greater amplitude of vibration is attained than if the feather were directly attached to the wire). The musician stands in such a position that the wire may not impede the movements of his bow, and plays fragments of simple melodies in slow time (each note lasting at least a second). The vibrations of the strings are transmitted to the bridge, the metal plate, the wire, and, lastly, to the feather, which vibrates synchronously. It only remains to trace these vibrations.

The registering instrument is composed of a metal cylinder, M, the axis of which is furnished with a screw moving a double nut, firmly fixed to a table or to the wall. This cylinder is covered with a sheet of paper, which is blackened by making it revolve over the smoky flame of an oil-lamp. A tuning-fork, D, making from 300 to 500 double vibrations per second, and carrying a strip of tinsel to serve as an index, is firmly fixed in a vice or in the wall, and arranged so that its index may vibrate in the direction of the generating lines of the cylinder. These vibrations serve to mark the time, and the tuning-fork serves as a chronograph, obviating the necessity of giving to the cylinder a regular and uniform motion. Further, the feather is moved forward, so that its point may just touch the blackened paper, and that it may vibrate quite close to the index, and, like the latter, in the direction of the generating lines of the cylinder.

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* Translated from the *Journal de Physique*.

† See the same volume, p. 109, Sur l'Histoire de l'Acoustique Musicale.

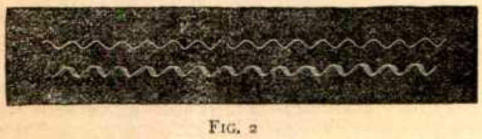

FIG. 2

These arrangements being made, the tuning-fork is set in vibration, either with a bow or by striking it with a stick covered with leather, and the musician plays, while the cylinder is turned at a suitable rate either by the hand or by any convenient motive power.

In this manner a tracing is obtained like that of which a fragment is shown in Fig. 2, each note of the melody being represented by a form of vibration peculiar to itself. The number of vibrations for each note, corresponding to 100 vibrations, for example, of the tuning-forks, is counted, and the ratio of the numbers thus obtained gives the values of the intervals. The vibrations are sometimes complicated with harmonics (Fig. 3), but they are almost always octaves, rarely fifths, very rarely thirds; moreover, it is not possible to make a mistake on this point.

To preserve the tracing after it is detached from the cylinder, it is split longitudinally, dipped for an instant into a 4 per cent. solution of shellac in alcohol, whereby it becomes covered with a very thin layer of unalterable varnish.

If, instead of measuring intervals of melody, we wish to measure the harmonic intervals of two sounds, two strings of the instrument are tuned simultaneously (in the ordinary way), to the third, fifth, sixth, &c., till beats are no longer perceptible, and the ear is perfectly satisfied; the sounds of the two strings thus tuned are then separately traced.

We have made numerous experiments with several persons, in particular melody experiments with M. Léonard, the Belgian violinist, and M. Séligmann, the violoncellist. The mean values of the results obtained with the assistance of these eminent artists are given in the following table; other experiments gave octaves equal to 2.

	Do.	Re.	Mi.	Fa.	Sol.	La.	Si.	Do.
Mean of the results.	1·000	1·128	1·265	1·330	1·500	1·686	1·917	...
Pythagorean Scale.	1·000	1·125	1·266	1·333	1·500	1·687	1·898	2·000
Ordinary Scale.	1·000	1·125	1·250	1·333	1·500	1·666	1·875	2·000
Values of the Comma.*	0·013	0·014	0·016	0·017	0·019	0·021	0·024	0·025

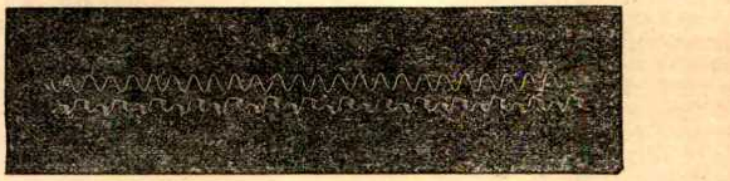

FIG. 3

It is necessary to add that the mean deviations of the experiments rarely exceed the third of a comma. As to the differences between the mean values of the results and the values of the intervals of the Pythagorean Scale, it is a mere fraction of the comma, insensible to the ear. For the seventh alone the difference amounts to five-sixths of the comma; but this result is remarkable, inasmuch as it exhibits a fact well known to musicians, namely, that in the case where the sensitive note Si is resolved upon the tonic Do (which is precisely what occurs in the four cases in which we obtained sevenths), it is perceptibly higher than in the inverse movement.

E. MERCADIER

DISCOVERY OF A LARGE BONE CAVE IN BAVARIA

DURING the cutting of the railway from Nuremberg to Regensburg by the Bavarian Eastern Railway Co., it was necessary to cut directly through a piece of mountain chain in Schelmengraben near Regensburg. It was owing to this that this bone cave was discovered, and its miscellaneous contents were able to be examined and arranged. Since the railway cut right across the middle of the cave, it allowed it to be very thoroughly examined, and under the most favourable circumstances and in daylight, as has been the case in very few other instances. The railway company have given every facility in their power that the cave should be thoroughly examined, and under the direction of Profs. Fraas and Zittel, a gang of men were actively employed for many days, and the objects so obtained were carefully preserved. From the local German papers the following particulars have been obtained, which, allowing for a little local colouring and exaggeration, show the find to have been a most important one, and one that may well come under the notice of the International Congress of Archæology and Anthropology at their meeting this year, where the whole question of bone caves and their contents is to form a prominent subject for discussion.

The cave in question was originally, when first discovered about two years ago, 28 metres (about 91 ft.) long, and was simply a fissure in the Jura limestone which had been enlarged by running water. Its opening was visible half way up the mountain side, partly hidden in dense woods. It stretched from North to South, with a slight turn towards the West of about 15°. The new line of railway cut deeply into the hill side, and during the course of this year has already cut away one half of the cave, but unfortunately the contents were employed on the line. On this account, only the part not touched was able to be excavated and examined, and this was 11 metres (36 ft.) long, 2 metres (6½ ft.) wide, and in the middle 3 metres (9½ ft.) deep. Wood ashes and pieces of coal, together with pieces of pottery, had accumulated to about the height

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* These numbers are the *differences* between the numerical value of each interval I, and the mean of this same interval raised by a comma, that is to say,

$$\left(\frac{81}{80}-1\right)\times I = I \times 0{\cdot}015$$

of three feet, in the midst of which were sharp splinters of flint, and a thick mass of broken and split bones, and the shattered skulls and jaw bones of a heterogeneous mass of animals of all kinds. In the lowest layer no trace of men, either by their remains or by their handiwork, could be found, all the remains consisted of bones of animals, chiefly the cave bear, hyæna, and lion. These cave-dwelling animals appear to have been the first and earliest possessors of the cave. But soon after this men must have discovered the cave and inhabited it, for from this layer up to the newest layer of all the presence of man is clearly shown, and the remains of their feasts and of their daily life are mingled with those of the previously-named animals. The most numerous remains consist of flints of which many thousand were found, but these do not appear to have been used as implements, but come rather under the category of flint-flakes, the chippings from knives, saws, lances, &c. The most perfect one found is three inches long, and half-an-inch wide, and is toothed like a saw, and was probably used as such to saw off the ends of the deer's horns, of which quantities were found.

In order to judge the age in which men began to inhabit this cave, we must examine the remains of the bones and skeletons of the animals which they hunted, and whose flesh was eaten in the cave. The most conspicuous amongst these is the cave bear, and although it might at first sight appear very difficult to recognise in the broken and burnt bits of bone that they really do belong to the cave bear, nevertheless, careful comparison with specimens in museums has proved that this is the case. Every care seems to have been made to utilise to the utmost all parts of this animal, which was apparently the most important game in the surrounding forests, and which no doubt required much labour and time to capture. At the same time, together with the bones of the cave bear are found bones of the elephant and of the rhinoceros, but not many in comparison. These remains, however, show conclusively, by the way in which they have been split up and broken, that man hunted these animals at the time he first appears on the scene. Remains of horses, oxen, cats, and wolves were also met with, and in proof that the early inhabitants were not unmindful of fish, there are the bones and scales of large pike and carp. The smaller bones of mice and frogs do not appear to owe their origin so much to man as to the owls which seem to have held possession of the cave as well.

Great interest attaches to the fragments of pottery which were found in the cave, and which rival the flint flakes in quantity. It appears to have been all hand made, but although rough, shows considerable beauty of shape and form. It is possible to put together from the fragments one or two more or less complete vessels, which, however, show great diversity as to size, &c., some being between 10 and 20 centimetres in diameter. The material of which they are made appears to be clay mixed with sand, but few, if any, seem to have been regularly burnt. Much of the pottery is ornamented with lines or rows of dots, which run in zigzag lines over the wider parts. The internal smoothness would appear to be due to the river mussel, Unio, obtained from the River Naab which flows close by, and of which many well rubbed and polished specimens were found in the cave. A block of granite with one side rubbed smooth, and by long usage appearing quite polished, can hardly be anything else than a well-worn millstone, and this is rendered more probable by two holes having been bored into the upper side as if for the purpose of affixing a handle. The presence of this millstone would indicate the cultivation of land in the immediate neighbourhood, which is confirmed by the finding of several spindles made of clay.

The different objects found in this cave are of great interest, as they apparently run counter to the somewhat hard and fast lines which have been drawn as to different well marked periods in the early history of man.

THE PARIS SIEGE BALLOONS

THE lessons learnt at Paris in regard to balloon navigation will be of great value in any future employment of aërial machines, and the statistics which have now been collected and published are well worthy of a brief notice. As many as sixty-four balloons, it appears, actually started from Paris in good order, with a *personnel* of 161, and with something like three million letters. The first ascent was made on the 23rd of September, 1870, by M. Duruof with safety, and the fifth balloon carried in it Gambetta, who arrived without accident at Amiens after a voyage of four hours. M. Janssen, whom, it will be remembered, was desirous of watching the approaching eclipse in the south of Europe, left Paris with all his instruments complete in the balloon Volta, on the 2nd of December, landing at Savenay (Loire Inférieure) after a journey of five hours and a half. One of the later voyages was made with two cases of dynamite, to be dropped and exploded at a seasonable moment; but fortunately for the enemy no such opportunity presented itself. The last balloon left Paris on the 28th of January, 1871.

Of these sixty-four balloons only seven were unsuccessful in fulfilling their purpose, two of the machines being utterly lost at sea; while five were captured by the enemy. As many as sixteen actually fell within the hostile lines; but the aëronauts were in most cases too quick for their pursuers, and managed to escape. Indeed, of the five actually taken only three were really captured by the enemy's forces, the other two falling in fact upon German soil, namely, in Prussia and Bavaria. The most interesting voyage was certainly that of M. Rollier, who travelled safely from Paris to Christiania in fourteen hours, after a journey across the North Sea of nearly twelve hours. Of the two lost at sea, one was observed to go down by some sailors at Rochelle; while of the other nothing certain is known.

The regularity with which the balloon service was conducted during the winter of 1870 under grave disadvantages will be remembered by all who studied the daily newspapers at that period, the news from the French Capital never being interrupted for more than three or four days together. Most of the aërial machines contained 2,000 cubic metres of gas, and one of them consisted of twin spheres tethered together; they were usually started from the Orleans or North railway stations at nightfall, so that they might escape the vigilance of the German troops posted round the city. Besides a freight of letters the majority carried baskets of pigeons, and in five cases dogs, destined to return with news to the beleagured city; how well the pigeon-post itself was organised may be gathered from the fact that fifty thousand messages were sent into Paris by its means alone.

Some attempts were made by MM. Tissandier Frères to return to Paris by means of aërial machines impelled by favourable winds; but two successive essays made from Rouen on the 8th and 9th of November were quite fruitless. M. Jules Godard, the aëronaut, and M. Nadar were the principal agents in organising the balloon service.

NOTES

AT the Anniversary Meeting of the Royal Geographical Society, held on Monday last, the Royal medals for the encouragement of geographical science and discovery were presented. The Founder's Medal was given to Sir W. Baker for Colonel Henry Yule, C.B., in recognition of the eminent services he has rendered to geography in the publication of his three great works—"A Mission to the Court of Ava," "Cathay, and the Way Thither," and "Marco Polo." The Patron's or Victoria Medal was personally presented to Mr. Robert Berkeley Shaw, for his journeys in Eastern Turkestan, and for his extensive series of astronomical and hypsometrical observations, which have enabled us to fix the longitude of Yarkand, and have given us, for the first time, the basis of a new delineation of the countries between Leh and Kashgar. A gold watch was also awarded to Lieut. G. C. Musters, R.N. (now travelling in America, and represented at the meeting by his brother), for his adventurous journey in Patagonia, through 960 miles of latitude, of which 780 were previously unknown to Europeans; and the sum of 25*l.* to Karl Mauch, in acknowledgment of the zeal and ability with which he has devoted himself for a series of years to the exploration of South-Eastern Africa. Mr. Shaw, who was addressed by the gallant president as "the hero of the hour," was loudly cheered by the meeting when he briefly acknowledged the honour paid to him. The annual geographical medals offered by the Society to the chief public schools were presented to the following successful competitors:—Physical Geography: Gold medal, S. E. Spring-Rice, Eton College; bronze medal, A. S. Butler, Liverpool College. Political Geography: Gold Medal, W. G. Collingwood, Liverpool College; bronze medal, W. C. Graham, Eton College. The president, Sir Henry Rawlinson, K.C.B., then delivered his anniversary address, which was chiefly occupied by tributes to distinguished members who have died during the year, and to a statement of the most recent information respecting the Livingstone Search Expedition.

THE President of the Society of Telegraph Engineers has issued invitations for a *conversazione*, to be held in Lord Lindsay's Laboratory on June 6, at 9 P.M.

AT the meeting of the French Academy of Sciences on May 20, M. Tresca was elected a member of the section of Mechanics in the room of M. Combes, deceased.

THE Dutch Society of Sciences in Haarlem has awarded the great gold Boerhaave Medal to Mr. H. C. Sorby, F.R.S., and elected him a foreign member. This medal, of the gold value of 500 gulden (about forty guineas), has been established three years, and is to be given away every two years to those who during the last twenty years have made themselves particularly meritorious in different departments of Science, according to a fixed rotation, and this year was given for the branch of mineralogy and geology.

AT the last meeting of the Royal Society of Edinburgh, Prof. Turner was presented with the Neill prize and gold medal for the triennial period ending 1871, for his papers on the "Great Finner Whale," and on the "Gravid Uterus and the Arrangement of the Fœtal Membrane in the Cetacea." The Keith prize, for the biennial period ending May 1871, was awarded to Prof. Jas. Clerk Maxwell, F.R.S., for his paper "On Figures, Frames, and Diagrams of Forces"

THE *Medical Times and Gazette* states that M. Jules Simon, the Minister of Public Instruction in France, has accepted in principle the creation of a Faculty of Medicine at Bordeaux to replace that of Strasburg, and that a commission has been appointed to report upon the project in question. It is also in contemplation to establish a School of Medicine and a School of Pharmacy at Lyons.

THE *Revue Scientifique* of May 18th gives an account of the inauguration of the German University of Strasburg, with an interesting sketch of the history of the university under its original German rule, and subsequently to its incorporation into the French Empire by Napoleon I.

MR FLOWER, the Professor of Anatomy at the Royal College of Surgeons, London, is anxious to collect and exhibit in the Museum of the College a complete set of skulls of all the varieties of the dog. The collection will be of great value as bearing on the question of the variability of the skeleton in domesticated varieties of the same species.

THE following excursions have been arranged by the Geologists' Association to take place in June:—Excursion to Guildford, Saturday, June 1, Directors, Prof. T. Rupert Jones, and Mr. C. J. A. Meyer. Upon arrival at Guildford the members will proceed to inspect the very instructive exposure of the Chalk and Lower Greensand in the neighbourhood of Guildford. The physiography of the district is also extremely interesting, and is well seen from several elevations which will be visited. Excursion to Bromley and Chiselhurst, Saturday, June 15, Director, Mr. J. W. Ilott. Leave Charing Cross for Shortlands Station. Visit the waterworks at Shortlands, and inspect section of well. Walk along railway to Bromley, and examine five sections of the Woolwich and Reading Series in the Palace Park and at the Brick Works. Walk through Sundridge Park, and inspect Sections of Shell Beds. Subsequently visit the Chalk Caves of Camden Park, and return from Chislehurst Station. Excursion to Hendon and Finchley, Saturday, June 22, Director, Dr. Henry Hicks. On arrival at Hendon Station proceed, under the guidance of Dr. Hicks, to inspect the Sections of the Glacial Drift in the neighbourhood of Hendon and Finchley. Return from Finchley Station. The long excursion of the session will be to Ludlow and the Longmynds in July.

DR. STIMPSON, the eminent director of the Chicago Academy of Sciences, has been engaged during the past winter in prosecuting deep-sea explorations in Florida. He first accompanied the United States Coast Survey steamer *Bibb*, when making soundings between Cuba and Yucatan for a submarine cable, but found the sea bottom very poor in animal life. We have previously mentioned that the bottom temperature in the deepest water was about 39·5° F., which may possibly account for the scanty fauna. The bottom consisted of sand and Globigerinæ mixed, in which scarcely anything occurred but shells, mostly dead. Some of the species were identical with those obtained by Mr. Gwyn Jeffreys at a similar depth off the European coast. On their way back from the cable work, the expedition made one haul of the dredge off the Cuban coast, near Havana, in 250 fathoms water, and obtained a superb specimen of the very rare *Pentacrinus Caput Medusæ.* the first ever obtained so near the American coast, and perhaps hardly represented as yet in any of the museums After returning to Key West the doctor took charge of the dredging on board the Coast Survey steamer *Bache*, but ill-health prevented his prosecuting this to any extent.

Harper's Weekly states that Prof. J. D. Whitney, the accomplished State Geologist of California, has undertaken to collect the facts in regard to the late earthquake, and has proceeded, with this object to Inyo County, the centre of its most active manifestation. As. Prof. Whitney has made a specialty of the study of earthquakes and the accompanying and resultant phenomena, we have no doubt that much light will be thrown upon this interesting topic.

WITH reference to the connection between electricity and earthquakes, the *Pall Mall Gazette* quotes from a Californian paper, the *Inyo Independent*, the following curious statements respecting the prevalence of electrical phenomena at the time of the recent earthquake in that State:—"A few days after the big shock, so called, at Cerro Gardo, very loud thunder was heard during a violent snowstorm. With the exception of the snow, the same thing occurred here, and perhaps at other places in the valley. This is remarkable, because almost unprecedented. Immediately following the great shock, men whose judgment and veracity are beyond question, while sitting on the ground near the Eclipse Mines, saw sheets of flame on the rocky sides of the Inyo Mountains, but half a mile distant. These flames, observed in several places, waved to and fro, apparently clear of the ground, like vast torches. They continued for only a few minutes. In this office, one day last week, while one of the proprietors was running a large number of sheets of flat-cap paper through a job press, these sheets, after leaving the press, were affected by the movements of the operator's hand, as a strong magnet would affect iron filings. When his hand was near them, the whole pile, or at least a hundred of them from the top, seemed to float in the air, like tissue paper in a slight breeze The top sheet would rise at each end up to the hand when held four inches above it, and thus by attraction be moved entirely away from the others. At times during the night sparks of fire were repeatedly emitted from a woollen shawl on being touched by the hand. At the Kearsarge Mill, located at an altitude of nearly 80,000 feet above the sea, the following occurrence was noted by Harry Clawson and P. J. Joslyn:—The former, while sitting with his knees within three inches of a cast-iron stove, felt a peculiar numbing sensation, and, supposing his limbs were 'asleep,' essayed to rub them with his hand. As soon as his hand touched his knee he felt a shock, and immediately after, and for a couple of seconds, a stream of fire ran between both knees and the stove. We will here state, on the authority of a man who had an opportunity of knowing, that the item going the rounds to the effect that no movement of the earth was observable 300 feet underground in the mines is not correct. At Cerro Gardo, and also at the Eclipse Mine, the rocking motion was distinctly observed, especially in the timbering. Small particles of rock were detached, and in both places the miners went to the surface in alarm; but at Cerro Gardo they soon resumed work as before."

TELEGRAMS just received from New York speak of a terrible disaster to the seal fishing fleet on the coasts of Labrador and Newfoundland. Four steamers and nearly forty sailing vessels are reported to have been wrecked among icebergs and the ice fields by a hurricane. The whole of the crews, which averaged ninety men, perished. Later accounts, however, state that the reports of the disaster to the fishing fleet were exaggerated. Only twelve vessels were lost.

A TERRIFIC hurricane visited Madras on May 2. It is described as being the most violent that has occurred there for many years, the devastation occasioned among the shipping being of a terrible character. Nine English vessels are spoken of as having become total wrecks, and the destruction of life is said to have been very great.

NATAL papers describe the great brilliance of the Aurora Australis witnessed both in that colony and in Cape Colony and the Free States on Feb. 4. The *Natal Colonist* of March 5 speaks of the southern aurora being not unfrequently visible there in broad daylight.

A REMARKABLE story from Newfoundland is detailed in a letter to the *New York Times* of April 15, to the effect that a Danish brig just arrived, which had left Disco on March 1, brought information that the *Polaris*, under Captain Hall, had been there for two days undergoing repairs and procuring a fresh supply of provisions. The account goes on to say that on the evening of February 8 the *Polaris* encountered extremely heavy weather, and while lying to, owing to the shallowness of the water, ran among ice snags, which caused a leak in the vessel, and made it necessary to proceed to Disco for repairs. Mysterious intimations were given of wonderful discoveries which had already been made by the *Polaris*, indicating the existence of a genial atmosphere and open seas in the extreme north. Plants indigenous to southern climes were detected in the ice, while a floating stick of wood proved to be northern birch. Throughout the whole of the month of February very little ice was seen, and the skies were literally alive with meteors of the most gorgeous description. On Christmas-day the ship was hemmed in by a heavy field of ice, but the weather was as pleasant as an Italian spring day. Such was the reluctance of Captain Hall to have the further discoveries which he is expecting to make shared by rival expeditions, that, according to the writer, he did not send word of his return to the Secretary of the Navy. The entire story bears little evidence of credibility, and will at least require further confirmation before it can be accepted.

CAPTAIN THOMAS LONG, so well known as the discoverer, in 1867, of Wrangell's Land, situated about seventy to one hundred miles north of Cape Yakan, in Siberia, has written a letter in reference to the plan of exploration by Mr. Octave Pavy, to which we have already referred. While indorsing the idea presented by Mr. Pavy, he takes occasion to claim it as his own, having, as he states, urged this route as long ago as 1867, the time of his first discovery. He does not think that Mr. Pavy will be able to pass through the channels between Spitzbergen and Greenland, or between Nova Zembla and Spitzbergen, as those passages have always been found blocked with ice, and it would be impossible to winter in the ice in such a raft as he has constructed. He thinks it possible that the North Pole may be reached from Wrangell's Land, but that it would be necessary for him to return for winter quarters; but to endeavour to return into the Atlantic with such a craft would be the height of folly. He believes that a vessel, properly fitted for the purpose, could make the passage from Behring Strait to the Atlantic in one year from the time of passing Behring Strait.

PROF. O. C. MARSH describes in the *American Journal of Science* for May four new species of fossil birds, three of them belonging to the genus *Graculavus*, probably closely allied to the cormorants of the present day, and occurring in the cretaceous deposits of New Jersey and of Kansas. The fourth is a species of *Palæotringa*, from the cretaceous greensands of New Jersey. The same paper contains a more elaborate description of the very remarkable new fossil bird named by him in January last *Hesperornis regalis*. This has numerous peculiarities, although it seems to resemble most closely the common loon of the United States. It was, however, much larger, as its complete skeleton would measure nearly six feet from the tip of the bill to the end of the toes. It occurs as a fossil in the gray shale of the upper cretaceous formations near Smoky Hill Fork, in Western Kansas.

WE learn from Rockhampton, Queensland, that on January 31 a fisherman named W. C. Easton discovered an alligator's nest on Eighteen-mile Island, eighteen miles above Rockhampton in the Fitzroy River. The mother was in the nest when Easton made the discovery, but she ran off, "bellowing like a cow after her calf," as Easton fired a shot from his double-barrelled gun into the river. She was about nine feet long. As Easton went up to the nest, a large carpet snake was about to enter it, but the snake, too, fled before his approach. On examining the nest, Easton discovered sixty-seven eggs, which he took away. The eggs were rather larger than a goose's egg, measuring 6½ inches in circumference one way, by 8½ inches the other; 3¼ inches in length, and nearly white, and in shape almost a true ellipse, but rather too long for their breadth. Mr. Easton placed four of the eggs under a hen, and twelve in straw, in the hope of rearing and domesticating some young alligators. The Fitzroy is the most southern river in which the alligator is found on the East Coast of Australia, and is just within the tropics.

ON OPTICAL PHENOMENA PRODUCED BY CRYSTALS SUBMITTED TO CIRCULARLY POLARISED LIGHT*

ON a former occasion I exhibited some phenomena depending upon circular, or, as it was then also called, successive polarisation, and in particular I adopted and explained a method for producing circularly polarised light devised by Sir Charles Wheatstone. I propose on the present occasion to pursue the subject into some of its ulterior consequences. In terms of the wave theory, light is said to be circularly polarised when the vibrations are circular, as distinguished from plane polarisation, when they are rectilinear. And further, it is known from mechanical principles that a circular vibration may always be produced by the combination of two rectilinear vibrations, the amplitudes or extents of which are equal, and whereof one is in advance or in rear of the other by one or by any odd number of quarter-wave lengths. In the former of these cases the circular motion will take place in one direction, say right-handed, in the latter in the opposite, say left-handed. The contrivance used for producing circular polarisation this evening is known by the name of a "quarter undulation plate," and consists of a plate of mica split to such a thickness that one of the two rays into which plane polarised light is divided on entering it is retarded by an odd number of quarter-wave lengths behind the other.

The optical phenomena produced by crystals when submitted to polarised light are usually divided into two classes, viz (1) those arising from the use of parallel light, and consisting of broad sheets of colour; and (2) those due to convergent light, and consisting of the rings and brushes, the general character of which is well known. I propose to take a few specimens from each class, and to examine the modifications which the known phenomena undergo when the light is both polarised and analysed circularly, *i.e.*, when one quarter-undulation plate is interposed between the polariser (Nicol's prism) and the crystal to be examined, and the second between the crystal and the analyser (Nicol's prism).

In the first place, it is known that if a plate of selenite be placed in an ordinary apparatus when the polariser and analyser are either parallel or crossed, there are four positions at 90° apart in which the plate will produce colour; and further, that if the analyser be turned through 90° the same result will be obtained, except that the colour will be complementary to that first seen. The intensity of the light at any given point is then given by the formula:

$$\cos.^2 s - \sin.\ 2i \sin.\ 2(i - s) \sin.^2 \frac{\theta}{2}$$

where i and s are the angles made with the original plane of polarisation by the principal sections of the crystal and of the analyser respectively, and θ is the retardation.

If, however, the two quarter-undulation plates (say the plates A and B) be introduced, the light undergoes the following processes:—First, it is plane polarised by the polariser; secondly, the plate A being placed so that its axis is inclined at ± 45° to the original plane of polarisation, the light undergoes right or left-handed circular polarisation, and in that condition falls upon the crystal; thirdly, in their passage through the crystal C the rays are each divided into two, whose vibrations are at right angles to one another, and whereof one is retarded in proportion to the thickness of C; fourthly, the plate B being placed so that its axis is parallel or perpendicular to that of A, each of these sets of rays is circularly polarised, one set right-handed, and the other set left-handed; fifthly, these two oppositely circularly polarised sets of rays combine, according to known mechanical laws, on emerging from B into plane rays, in which the planes of polarisation of the different colours of the spectrum are turned through different angles. Hence finally by turning the analyser round we shall cross these various planes in turn and successively extinguish the different colours, leaving the complementary colours visible. The system of plates A C B consequently acts in this respect like quartz. It is, however, to be observed that if the plate B be turned from one of the two proposed positions to the other, the directions of motion in the two emergent circularly polarised rays, and consequently the planes of polarisation of the different colours, will be reversed; in other words, with the plate B in one position we shall imitate a right-handed, with the plate B in the other a left-handed, quartz The intensity of the light at any point is then given by the formula: —

$$\sin.^2 \frac{\theta}{2} \text{ for one position,}$$

$$\cos.^2 \frac{\theta}{2} \text{ for the other.}$$

Again, if the plates A B retaining either of the positions before indicated, the crystal C be turned round in its own plane; then, since the light emerging from A and B is circularly polarised, it has lost all trace of direction with reference to the positions of the polariser and analyser, and consequently no change of tint will be observed. The same is abundantly clear from the formula written above, because the only term it contains depends upon the retardation within the crystal C. This experiment was made by Airy.

If the plates A and B have their axes directed 45° on either side of the axis of C, and the three plates be turned round as one piece, the colour will remain unchanged; while, if the analyser be turned, we have the colours shown in the regular order. If the plates A and B have their axes directed at 45° on the same side of the axis of C, and the pieces be turned round bodily as before, the colours change in the same order as above, and go through their cycle once in every 90° of rotation; and if the analyser be turned in the same direction the colours change, but in the reverse order. The explanation of this is to be found in the fact that when the plates A and B are crossed, the retardation due by A is compensated by that due to B; so that the only effective retardation is that due to the crystal C. But upon this depends the rotation of the plane of polarisation; if, therefore, the polariser and analyser remain fixed, the colour will remain unaltered. When the plates A and B have their axes parallel, there is no compensation, and the colour will consequently change. This experiment was made by Fresnel. The mathematical expressions for the intensity of the light in the two cases respectively are:—

$$\cos.^2\left(j + i + \frac{\theta}{2}\right), \text{ and } \cos.^2\left(j - i - \frac{\theta}{2}\right),$$

where i is the angle made by the principal sections of A with that of the polariser, and j that of the principal section of B with that of the analyser. The first expression is obviously unchanged when the angle between the polariser and analyser, viz. $\frac{\pi}{2} + i + j$, is unchanged.

It should be added that the rotation of the plane of polarisation, and consequently also the sequence of tints, does not follow exactly the same law in the above cases as in quartz.

We now come to the case of convergent light, that is, to the phenomena of crystal rings. And let us examine the effects produced by the same arrangement as before, viz., two quarter-undulation plates, A, B, one in front and one behind the crystal C. To quote from Mr. Airy:—"The first thing that strikes us in this combination is that there is nothing, except in the crystal, that has any respect to sides. For the only incident light is circularly polarised; the only light allowed to emerge is circularly polarised. The appearance, therefore, of the coloured rings, &c., must be such as conveys no trace of any plane of polarisation, and must not vary as the crystal is turned round. In the common exhibition of the coloured rings the principal trace of the planes of polarisation is in the uncoloured brushes. In uniaxal crystals they form an eight-rayed star, composed of two square crosses, inclined at an angle equal to that between the planes of polarisation, every ray of which separates complementary rings. In biaxal crystals they compose two pairs of rectangular hyperbolas, the angle between whose asymptotes is the same as that between the planes of polarisation, and whose branches divide complementary rings. The two crosses or two sets of hyperbolas unite when the planes of polarisation are parallel or perpendicular. But in the case under consideration the rings exhibited by crystals will not be traversed by any brushes. Uniaxal crystals will exhibit circular rings without a cross; and biaxal crystals will exhibit complete lemniscates, without any interruption from curved brushes." And it is further to be noticed, as the formula given above indicates, that the centres of the rings will be bright or dark according as the analyser stands at 0° or 90°.

To pursue this matter further. Suppose that, the arrangements remaining otherwise as before, the analyser be turned round; then in any position intermediate to 0° and 90° the rings

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[*] Lecture delivered at the Royal Institution of Great Britain, May 3, 1872, by W. Spottiswoode, Treas. R.S.

will be contracted and extended in opposite quadrants until at 45° they are divided by two diagonals, on each side of which the colours are complementary. Beyond 45° the rings begin to coalesce, until at 90° the four quadrants coincide again. During this movement the centre has changed from bright to dark. If the motion of the analyser be reversed, the quadrants which before contracted now expand, and *vice versâ*. Again, if the crystal (say positive) be replaced by another (say negative), the effect on the quadrants of the rings will be reversed. This method of examination therefore affords a test of the character, positive or negative, of a crystal.

A similar process applies to biaxal crystals; but in this case the diagonals interrupting the rings are replaced by a pair of rectangular hyperbolas, on either side of which the rings expand or contract, and the effect is reversed either by reversing the motion of the analyser, or by replacing a positive by a negative crystal, or *vice versâ*. The experiment may then be made in biaxal crystals, by turning the analyser slightly to the right or to the left, and observing whether the rings advance towards or recede from one another in the centre of the field. In particular, if, polariser and analyser being parallel, the plate A have its axis in a N.E. direction to a person looking through the analyser, the plate B its axis in a N.W. direction, and the crystal be so placed that the line joining the optic axes be N.S., then on turning the analyser to the right the rings will advance to one another if the crystal be negative, and recede if it be positive. The mathematical expression for the intensity of the light at any point P is in this case

$$\tfrac{1}{2}\,(1 + \text{sin. } 2\,j \text{ cos. } \theta + \text{sin. } 2\,b \text{ cos. } 2\,j \text{ sin. } \theta),$$

where *b* is the angle between the principal section of C through P and the principal section of B, and *j* the angle between the principal sections of B and the analyser. This shows that when the polariser and analyser are parallel or crossed at 0° or 90°, and consequently $j = 45°$ or 135°, the expression is independent of *b*, *i.e.*, the intensity is the same throughout circles about the centre, but that when the polariser and analyser are crossed we have an expression of the form

$$\tfrac{1}{2}\,(1 \pm \text{sin. } 2\,b \text{ sin. } \theta),$$

the sign of the second term depending upon the direction in which the analyser has been turned, and also upon the sign of θ, that is, upon the character (positive or negative) of the crystal.

The dispersion of the planes of polarisation effected by the passage of plane polarised light through a plate of quartz cut perpendicular to the axis may be rendered visible by interposing such a plate of quartz between the polariser and a uniaxal or biaxal crystal, when the analyser is at 90°, *i.e.*, when dark brushes are formed. In this case the brushes cease to be black, and are tinged throughout with colour. The analyser must, however, be turned back or forward, according as the quartz be right-handed or left-handed, in order that it may cross in succession the planes of polarisation of the different coloured rays, and so produce the most vivid effects. The dispersion of the brushes by a plate of quartz may, however, be studied by employing an additional polariser and quartz plate between the source of light and the whole system previously used. By turning this polariser round we extinguish each ray of the spectrum in turn, and tint the whole field with the complementary colour. The brushes will then appear to revolve about their centres as the tints vary continuously from one end of the spectrum to the other. If the polariser be turned still farther round, the tints which had changed continuously from red to violet, or *vice versâ*, change suddenly from violet to red, or *vice versâ*, and the brushes jump suddenly back to their original position.

This last optical arrangement may be employed to examine the more important phenomena of the dispersion of the optic axes produced, not by a quartz plate between the usual polariser and crystal, but by certain biaxal crystals themselves.

BOTANY

The Leaves of Drosera

In a recent note to the Paris Academy of Sciences, M. Ziegler writes as follows:—

The hairs on the leaves of Droseras exude at their extremity a small drop of glue, by which insects are caught. Whenever an insect becomes attached, the exterior threads bend over it, covering it like the fingers of a hand, and do not straighten again till some days after, when a fresh drop exudes for a fresh prey.

In studying these remarkable plants, I noticed that all the albuminoid animal substances, if held for a moment between the fingers, acquired the property of making the hairs of the Drosera contract. I also observed that such substances, when they had not been in contact with a living animal, had no visible action on the hairs. This shows that the simple contact of the fingers communicates to inert animal substances a property which they did not possess before.

These same animal substances thus prepared lost this singular property when they were moistened several times with distilled water, and dried each time in a water-bath. This is a convenient mode of preparing the substances for experiment.

The contraction of the hairs is not caused by animal heat, which the fingers may have communicated to the animal substances, for the hairs contracted equally when the substance had been cooled before placing it on the leaf.

The perspiration of the fingers cannot affect the phenomenon, for the property can be communicated to animal substances across fine waxed paper. And the result is not affected if the substances are first covered with a coating of wax, thus preventing any chemical action of soluble matters which the animal substances may contain.

A living animal thus communicating by simple contact new physical properties to an inert body, it was important to know whether, by increasing the amount of transmission, we should observe any change in the vital state of the animal. Some rabbits were enclosed in light wooden cages. These were of such a size that their sides were always in contact with the hairs of the animal at one part or other. To the outside of the cage were attached bags of cloth or paper, containing (for each cage) two kilogrammes of dried serum (albumen from blood). Other rabbits were placed in exactly similar cages, but without the albumen. Their food consisted of 25 grammes of hulled oats every twenty-four hours, with cabbage leaves at discretion.

At the end of some days, the rabbits that had been in contact with the albumen became diabetic in a high degree (though without saccharine matter); the urine was given in normal quantity, but the loss in ammoniaco-magnesian phosphate was very great, and these rabbits deteriorated and lost weight. The other rabbits, which had not been in contact with albumen, remained in their normal state, and even gained weight.

It was interesting to ascertain if the avidity of the Drosera for insects was insatiable, and to find what would be the effect on it of increasing the contact of a living animal. Some dozens were accordingly placed, with a small clod of earth and sufficient water, in light platinum capsules. These capsules were each placed in a sheath containing blood albumen, which had previously been held for half an hour in the hand. At the end of twenty-four hours all these Droseras had become quite insensible to insects and to organic animal bodies modified by living contact. The properties of these plants were reversed, and strange to say, their hairs were found to contract under the influence of organic matters which had previously been in contact with paper packets (of double or triple envelope) containing sulphate of quinine. Organic matters influenced in this purely physical manner by sulphate of quinine have no contractile action on the hairs of the Drosera in their normal state. The plants whose physical properties have been reversed by the influence of albumen in the above way, could be restored to their normal state by placing them for twenty-four hours with the platinum capsule on a packet of sulphate of quinine. This method may be adopted whenever, from any cause, the leaves have become insensible to insects. In every case the contraction of the hairs is always slow; it commences visibly in about a quarter of an hour, and is often not complete till after several hours.

Among vegetable matters seeds only are impressible in the way referred to, and the experiments mentioned (which were made with albuminoid animal substances) may be repeated with these.

Nature of Diatoms

In a recent essay by Prof. Adolf Weiss, of Lemburg ("Zum Baue und der Natur der Diatomaceen"), it appears to be demonstrated that the siliceous investment of these little plants has cellulose for its base. The silex is infiltrated to a variable extent in the different families, and the mode of its deposition can to a certain extent be ascertained by examination with polarised light. In opposition to the opinion hitherto generally admitted, Prof. Weiss shows that the siliceous coal is capable of polarising light; and he has found also that it contains a certain amount of iron-oxide compounds, which are for the most part in an insoluble condition. He strongly objects to the view that the *Diatomaceæ* are one-celled organisms, but contends that each frustule is composed of numerous very minute but perfectly individualised cells. The different markings on the frustules—areolæ, ribs, crests, &c.—are in no way caused by the contour lines of the several cells of which they are composed. The size of the cells is very variable. In *Triceratium favus* they are as large as 0·008 of a millimetre, whilst in *Hyalosira delicatula* they do not exceed 0·00025 of a millimetre. Each cell is arched, and, as a rule, prolonged into a papilliform process at its centre. The papillæ are the cause of the moniliform or pearl necklace-like markings of diatoms when examined with high powers, and which appear as striæ with low powers. The large cavity between the two frustules is, he thinks, comparable to the embryo-sac of higher plants; and Weiss has succeeded in observing the development of new individuals in it. The product of this new individual indicates the alternation of generations in the *Diatomaceæ*.

SCIENTIFIC SERIALS

THE first number of *Zeitschrift fur Ethnologie* for the current year (1872) opens with a paper by A. Bastian on "The Position of the Kaukasus in relation to the history of the migration of nations," in which the author points out the importance of studying the hydrography and orography of a country before we attempt to trace the origin of its inhabitants. Mountains and streams afford more stable evidence in regard to ethnological centres of origin than the ever-fluctuating combinations of language. Thus, for the history of our own Continent we can have no more important standpoint than the Kaukasian range, which forms the boundary line between Europe and Asia, from which rivers open the way into the Caspian and Black Seas. Herr Bastian next traces the various directions taken by successive waves of population after they reached the Steppes between the Don and the Dnieper, which long formed the meeting-place of the Scythio-Sarmatian races, and often witnessed the fierce encounter of rival hordes, whose defeat or success on that great battle-field of nations decided the fate of future races. The relation of the nomadic races of Asia to the Persian Empire is of special interest to us, since the latter by its control over the destinies of the western half of the Asiatic continent has exerted the most important influence on the ethnological history of Europe. In Asia the course of civilisation has followed the line of the Steppes; and the nomadic tribes who possessed horses have spread themselves through every pastoral district, amalgamating at times with the earlier settlers, but more generally organising themselves into hostile bands, whose leaders became the founders of equestrian dynasties, and raised thrones for themselves in Central and Western Asia. The author follows at length the progress of Parthian and Persian conquests and migrations, and, after considering the anatomical features and cranial dimensions and forms of the various races, which have given conquerors to the world, discusses the probable bastard or mixed origin of those inferior subjected races, who from time to time have risen against their masters, and asserted their right to freedom, as in the case of some of the Servian tribes against their Pannonian lords, and various Mestizoes or Creoles in Africa and America.—The remaining papers in this number are below the usual standard of the *Zeitschrift für Ethnologie*. We have a paper by Dr. E. v. Martens "On the Different Uses of the Conchilia," originally read to the Anthropological Society of Berlin, which is little more than a *résumé* of what G. E. Rumph, P. Bonanni, Johnston, and Mr. Woodward have given in their semi-popular works on subjects of conchological interest. Dr. Martens also contributes a translation of a paper on the geography, history, and statistics of the Island of Puerto Rico, by S. Bello, of Espinosa. We learn that while sugar and coffee constitute the riches of the island, all the tropical fruits abound, and the excessive annual rainfall maintains a vigorous and verdant vegetation. The hot moist climate is unhealthy, and dysentery, yellow fever (vomits), and remittent fevers of various kinds prevail. The population has, however, gone on steadily increasing during the last forty years, notwithstanding the diminution in the numbers of the slaves, amongst whom the deaths have of late years exceeded the births in the ratio of from 5 to 10 per cent. In 1839 the population was 319,000, in 1870 it had risen to 646,360; in the latter year the number of the slaves had fallen to 32,000, after being 42,227 in 1866, thus giving a diminution of 25 per cent. in four years.—M. de Quatrefages' history of Prussian aggrandisement, which first appeared in the *Revue des Deux Mondes* (1871), under the title "*La Race Prussienne*." has called forth an impassioned and indignant rejoinder in this number of the *Zeitschrift fur Ethnologie*. We should be more disposed to concur in the line of argument adopted by the writer in refutation of M. de Quatrefages' too sweeping assertion that Prussians are Finno-Slaves with only a slight admixture of French and German blood in the higher classes, if he had not allowed personal rancour and national hate to over-weigh every consideration of courtesy, justice, and reason. We think an ethnological journal is not the place for international warfare.

Annalen der Chemie und Pharmacie, viii. Supplementband, 2 Heft.—The first 100 pages of this number are occupied by an important theoretical paper "on a periodical law of the chemical elements," by Dr. Mendelejeff; the author has arranged the elements into eight groups and into twelve series; there seems to be a most curious regular progression, both in the atomic weights, the atomicities, and in the chemical proportion of these groups. To take for example the third series of elements, starting from group 1 to 7, we find the following:—Sodium$^{\text{I}}$ 23, Magnesium$^{\text{II}}$ 24, Aluminium$^{\text{III}}$ 27·3, Silicon$^{\text{IV}}$ 28, Phosphorus$^{\text{V}}$ 31, Sulphur$^{\text{VI}}$ 32, Chlorine 35·5, it will be seen that the first named is a very positive element, and that the positive character gradually changes through the groups until in the seventh we have a powerfully negative body; the atomic weights and atomicities of the elements also increase in a regular manner. In the other series the same kind of relation seems to exist; the author has left spaces in his table for elements not yet discovered, but for which he gives hypothetical atomic weights. The next paper is by Gorup Besanez "on the dolomite springs of the Jura," and is followed by another "on a new class of platinum compounds," by Schutzenberger; by the action of carbonic oxide on platinous chloride at high temperatures three distinct compounds have been obtained, the first containing one equivalent of carbonic oxide to one equivalent of platinous chloride, the second two equivalents of carbonic oxide, and the third one and a half equivalents of carbonic oxide to one of platinous chloride. Linnemann and Zotta have found that by heating glycerine with calcic chloride, small quantities of phenol are formed, and at the same time there is produced glyceric ether. Phenol is also obtained from glycerine by the action of zincic chloride or potassic bisulphate.

IN the *Journal of the Franklin Institute* for April we have the continuation of several papers already commenced, viz.:—Mr. Joseph Harrison's article on the locomotive engine, and Philadelphia's share in its early improvement; of Mr. J. S. Smith's account of the Keokuk and Hamilton Bridge; of Mr. J. F. Henry's paper on the flow of water in rivers and canals; and of Mr. J. Richard's article on wood-working machinery. The only new article of any length is by Lieut. Dutton on the principles of gun construction. and there are the usual paragraphs of Items and Novelties.

THE *American Journal of Science and Arts* for April commences with Prof. Marsh's account of the discovery of additional remains of Pterosauria, with descriptions of several new species, *Pterodactylus occidentalis*, *P. velox*, and *P. ingens*, of which full measurements are given, the last probably measuring nearly 22 feet between the tips of the fully expanded wings.—Prof. A. E. Dolbear describes a new method of measuring the velocity of rotation; and Prof. Dana continues his history of Green Mountain geology, dealing this month with the quartzite.—From Mr. F. B. Meek we have descriptions of two new starfishes and a crinoid from the Cincinnati Group of Ohio and Indiana, which he proposes to name *Palæaster* (?) *Dyeri*, *Stenaster grandis*, and *Glyptocrinus Baeri*.—Prof. Abbe gives an account of his observations on the total eclipse of the sun in 1869; and Prof. Twining of various observations on the aurora of Feb. 4.—Mr. Verrill's series of papers include this month recent additions to the molluscan fauna of New England and adjacent waters, with plate.

In the May number is a valuable epitome of recent geographical work in the United States, deduced from the report of the Corps of Engineers, U.S. Army, the route of the Northern Pacific Railroad, and the map of transportation routes in Minnesota and Dakota.—Prof. W. A. Norton contributes a paper on molecular and cosmical physics, in which he propounds several new theorems: the subject is to be continued.—As the commencement of a series of papers entitled "Contributions from the Physical Laboratory of Harvard College," Prof. Trowbridge has a paper on the electro-motive action of liquids separated by membranes.—Prof. Marsh describes, under the name of *Hesperornis regalis*, his exceedingly interesting gigantic fossil swimming bird discovered in the cretaceous strata, which he considers to belong to the Palmipedes, and to be most nearly allied to the Columbidæ, but differing widely in many respects from that group and from all other known birds, recent and extinct. Both in this and the previous number are the usual interesting paragraphs of information arranged under the various natural sciences.

SOCIETIES AND ACADEMIES

London

Zoological Society, May 21.—Mr. R. Hudson, F.R.S., vice-president, in the chair. The Secretary read a report on the additions that had been made to the Society's menagerie during the month of April, amongst which was a young female Baird's tapir (*Tapirus bairdi*) from Nicaragua, and a red-billed flying squirrel (*Pteromys magnificus*) from the Himalayas.—A letter was read from Dr. G. Bennett, of Sydney, N.S.W., giving particulars of the habits of a pair of *Didunculus strigirostris*, and of other birds living in the Botanic Gardens at Sydney. Dr. Bennett also mentioned that a pair of the red-billed curassow (*Crax carunculata*) had built a nest in one of the trees in the same gardens, and had hatched out two young birds, which at the time he wrote were doing well.—Sir Victor Brooke, Bart, read a paper on the royal antelope and allied species of the genus *Nanotragus*.—Mr. A. H. Garrod read some notes on the anatomy of the Huia bird (*Heteralocha gouldi*) as observed in a specimen that had lately died in the Society's gardens, and showed that this form must be referred to the family *Sturnidæ*.—A communication was read from the Rev. J. E. Semper, containing observations on the birds of St. Lucia, to which were added some notes on the species by Mr. P. L. Sclater.—A communication was read from Dr. J. E. Gray on the sea bear of New Zealand (*Arctocephalus cinereus*) and the North Australian sea bear (*Gypsiphoca tropicalis*).—A communication was read from Dr. A. Gunther, F.R.S., containing a note on *Hyla punctata* and *Hyla rhodoporus*.—Mr. P. L. Sclater read a paper on the species of Quadrumana collected by Mr. Buckley in Ecuador, amongst which was a specimen of *Ateles fusciceps* Gray, from the western valleys of the Andes.—Dr. Murie read a paper on the osteology of the Tody (*Todus viridis*). He showed that this form comes under the group of *Coccygomorphæ* of Huxley, and does not belong to the Passeres (*Coracomorphæ*). Its nearest allies are the mot-mots and kingfishers, but it must stand as a group of itself (*Todidæ*), notwithstanding which it shows some osteological and other points of resemblance to fly-catchers (*Muscicapidæ*).

Linnean Society, May 24.—Anniversary meeting.—The following were elected Officers and Council of the Society for the ensuing year:—President, Mr. G. Bentham, F.R.S.; Treasurer, Mr. W. W. Saunders, F.R.S.; Secretaries, Mr. F. Currey, F.R.S., and Mr. H. T. Stainton, F.R.S.; Council, Mr. A. W. Bennett, Mr. R. Braithwaite, M.D., Mr. G. Busk, F.R.S., Mr. J. Gwyn Jeffreys, F.R.S., Dr. J. D. Hooker, F.R.S., Mr. M. A. Lawson, Mr. H. Lee, Mr. R. McLachlan, Mr. J. Miers, F.R.S., Mr. D. Oliver, F.R.S., and Rev. Thos. Wiltshire.

Photographic Society, May 14.—Mr. James Glaisher, F.R.S., president, in the chair.—A paper "On Photographic Pictures" was read by John Hubbard, in which the manner of elaborating his photographic studies was gone into at some length. His entire method of operating was described, which, however, differed little from that in ordinary use.—Lord Lindsay exhibited a series of transparent pictures of the last eclipse, five positives from every negative being shown, so as to afford an exceedingly clear representation of the phenomenon.—Major Tennant, R.E., also forwarded a series of eclipse pictures for exhibition to the members.

Bristol

Observing Astronomical Society.—*Sun.*—Mr. T. W. Backhouse writes that "there was a fine group of solar spots in the sun's northern hemisphere last month. On the 24th at 5^h there was a largish spot at the preceding end of the group, which on the 23rd, at 3^h, either was small or did not exist at all. On the 26th, about 4^h, its penumbra was 51,500 miles long, and its umbra 28,000, but it never became such a conspicuous umbra as the one which had all along been the largest in the group. On the 28th, at 4^h 15^m, the penumbræ of the two were united, and 88,500 miles long, while at 20^h I found the penumbra to be 92,000 miles in length. It was then so close to the limb that I could not measure it accurately, the height being extremely foreshortened."—*Jupiter.*—Mr. H. W. Hollis, of Newcastle, Staffordshire, reports that on January 14, 9^h, the disc of the planet appeared very sharp, and he counted twenty-two different bands of colour. "Those visible in the equatorial parts of a beautiful, delicate, pinky brown. I am certain that the belts are visible up to the very edges of the disc, but there is an apparent increase of brightness for a considerable distance round the edge of the planet—probably an effect of contrast—which obliterates the extremities of the belts, unless carefully looked for. Several well-marked and beautifully-defined irregularities in the belts showed the rotation most clearly even in half an hour's watching. Jan. 23, 8^h 15^m.—Satellite I. just entered on disc of Jupiter, and appears as an intensely white spot. 9^h 20^m.—Shadow of I. on centre of disc, black, and sharply circular; the satellite itself cannot be seen." Mr. T. W. Backhouse, of Sunderland, observed the transit of Satellite I. on Jan. 14. At 13^h 54^m it "appeared as a faint white spot." On Feb. 3, 6^h 7^m, he examined Satellite III., and its shadow when in transit. The satellite itself was, at the time mentioned, nearly half across Jupiter, on a darkish belt. "It is much darker than the darkest part of the planet." At 7^h 30^m it was "still very plain, but only the same shade as the darkest part of Jupiter. It was smaller than its shadow, which was very black." *T. Coronæ Borealis.*—Mr. T. W. Backhouse says:—"A change has taken place in this star. On its fading the second time it became stationary in brightness about the middle of the year 1867, since which time, up the beginning of this year, it continued the same, but with frequent slight fluctuations, which however ceased, so far as I could judge, at the end of 1869. I have suspected fluctuations since 1869, but they were doubtful. On January 14 this year I looked at the star and found it about its usual brightness, or perhaps a little fainter, but certainly not fainter than it had been at times previously. I did not look at it again till March 5, when I found it much fainter than I ever saw it before, perhaps half a magnitude less than usual, and it was the same on the following day." *Nebula in the Pleiades.*—Mr. H. W. Hollis has looked for this nebula with his 8 in. achromatic, but cannot find it. He says:—"There is something peculiar about all the brighter stars of this group, which for months past have appeared to me as if surrounded with nebulous light. Can the nebula have been distributed amongst them?" *Meteors.*—The Rev. S. J. Johnson, of Crediton, witnessed the appearance of "a splendid meteor at 7^h 37^m April 6. Its course was in a straight line downwards from about 15° above the N.W. horizon to about 5°. Colour, white with a greenish tinge. Duration, about 5″. Seen against a dark sky, this meteor would have equalled, if not exceeded, the brightness of Venus or Jupiter. I was looking for Mercury at the time." On April 19, 11^h 10^m, Mr. William F. Denning, of Bristol, saw a brilliant meteor. It passed slowly down the N.N.E. sky. It was starlike in appearance, and left no train of light. *Mercury.*—The Rev. S J. Johnson observed Mercury both with the naked eye and telescope on the evenings of March 25 and April 5. A power of 100 on a small telescope brought out the phase.

Cambridge

Philosophical Society, April 29.—Mr. Paley, "On certain effects of Light on Portland Stone." The author said that he doubted from the mode in which this occurred whether the blackness of stone seen in towns was due simply to smoke; the black scraped from the stone was unaffected by soap or solution of soda, and presented under the microscope an appearance quite different from that of ordinary soot.—By Prof. Miller, "On Faye's method of comparing Mètres à Traits, and an improvement of it suggested by Prof. Miller."—By Mr. Bonney, "On certain lithodomous Burrows in the Carboniferous Limestone of Derbyshire." The author said that doubts having been thrown upon the accuracy of his statement of the occurrence of these burrows in Miller's Dale, he had again visited the spot, had found his description correct, and had discovered a large number of these burrows in Miller's Dale and in Tideswell Dale. From the positions in which he found them, he was more than ever convinced they were the work of *Helices*.

May 13—"On a Method proposed by M. Fizeau for comparing a *mètre à bouts* with a *mètre à traits*," by Prof. Miller.—"On the Section exposed at Roslyn Hill Pit, Ely," by Mr. Bonney. The author stated that there were two hypotheses which accounted for the singular collocation of boulder clay, cretaceous rocks, and Kimmeridge clay in this pit; the one attributing it to a fault, the other to a boulder-like mass of cretaceous beds which had been dropped there in the Boulder clay time. He exhibited plans and sections, and pointed out that the faulting would be of such a singular and exceptional kind that this hypothesis appeared to him in the highest degree improbable. The choice remained between regarding the cretaceous beds as brought on by an ice-raft, or the result of a slip from beds subsequently removed by denudation, and on the whole he preferred the former of these.

EDINBURGH

Royal Society of Edinburgh, May 20.—Prof. Sir Robert Christison, Bart., president, in the chair.—The Keith Prize for the Biennial period ending May 1871 having been awarded by the Council to Prof. James Clerk Maxwell for his paper "On Figures, Frames, and Diagrams of Forces," which has been published in the Transactions, the medal was delivered to him by the President at the commencement of the meeting.—The Neill Prize for the Triennial period ending 1871 has been awarded by the Council to Prof. Turner for his papers "On the Great Finner Whale," and "On the Gravid Uterus and the Arrangement of the Fœtal Membranes in the Cetacea," which have been published in the Transactions.—The following communications were read:—"Some Helps to the Study of Scoto-Celtic Philology," by the Hon. Lord Neaves, V.P.—"Some Observations on the Dentition of the Narwhal (*Monodon monoceros*)," by Prof. Turner.—"On the Occurrence of *Ziphius cavirostris*, got from Hillswick, Shetland, in the Shetland Seas, and a comparison of its cranium with that of *Mesoplodon Sowerbyi*," by Prof. Turner.—"On the Maternal Sinus System of the Human Placenta," by Prof. Turner.

HALIFAX, NOVA SCOTIA

Institute of Natural Science, December 11, 1871.—The vice-president, Dr. Gilpin, in the chair. The vice-president read the concluding paper of a series on the mammals of Nova Scotia, including the moose deer. Dr. Gilpin described its peculiar form, differing from all mammals by the length of its cannon bones (metacarpal and metatarsal), whilst in the shortness of its neck, its great height, its prehensile lip, it had a singular analogy with the equatorial form of the elephant, the giraffe, and the tapir, yet it more resembled certain large wading birds. It might be called a wading mammal, in summer resorting to the swamps and shallow lakes, in winter its long cannon bones allowing it to walk in the deep snow. It thus becomes straddling and weak-footed. In comparing its hind leg with that of the greyhound and hare, the swiftest animals known, its form would be found exactly opposite. He described its nuptial suit in September (its rutting season) of glossy black, and golden tan legs, and its wintery livery of shaggy grey hair. Its identity with the elk of Sweden was discussed, and from observations in the R. C. Surgeons' Museum, Sir John Richardson, and especially from Captain Hardy, R. A., who compared his sketches, notes, and measurements, of the moose of Nova Scotia, personally, with a pair of young elks from Sweden at Sandringham, Dr. Gilpin concluded them to be identical. Allowing them to be identical, then, as their fossil bones have been found in the Upper Tertiary formations in America, and not yet in the Old World, the moose must be held as the primitive type. Dr. Gilpin thought that, with the carriboo, its form must have existed contemporarily with many forms now extinct; that, perhaps, it was one of the earliest existing fauna that succeeded the glacial epoch in Nova Scotia, and that from some cause now existing this earliest fauna may be destined to be the last. From the almost entire identity of the boreal marine fauna, the marine birds, and the fish, the more we study the arctic forms, the more we are impressed with the conviction that we must look to the north for the common type of many of our temperate and equatorial forms. The shaggy elephant of Lorna, and the rough musk ox of the Pole, and the hairy-coated carriboo, may each have been the primal type of the naked-skinned elephants and buffalos of Asia and the satin-skinned African deer, whilst the coatis may equally have been the type of all foxes and dogs to the hairless race in Turkey. In a conversation that ensued it was maintained that two varieties existed in the province, but Dr. Gilpin considered them not permanent varieties. To show the numbers still extant, the game book of a gentleman present gave, from the year 1863, twenty-seven, whose death he had been in at, and ninety-seven which he had seen altogether.

CALIFORNIA

Academy of Sciences, San Francisco, November 6, 1871.—Dr. T. Blake, president, in the chair.—Mr. Harford stated that he had examined some Indian graves on San Miguel island, from two of which he had obtained a number of relics. The pits were about 25 ft. long, 5 ft. deep, and situated at an elevation of about 80 ft. above the sea. In one of the pits there were from 75 to 100 skeletons, most of the skulls showing marks of violence. No order had been observed in the burial, children and adults, male and female, all lying together. The other pit seemed to be of a much older date, as the bones were in a more advanced state of decay, and only stone beads or trinkets were found there, whereas in the other principally glass and shell beads were found. Large shell heaps were of common occurrence on the island, showing that at one time they must have been inhabited, although no Indians had been known to live on the island since the settlement of the country by the Spaniards.—Prof. Davidson, of the United States Coast Survey, remarked that as a rule in the entrances to the harbours and rivers on our coast the channels all tended to the N.W., the northern headlands showing bold rocky bluffs, the southern points, on the other hand, forming long low sandy beaches. He said his own observations had been confirmed by information received from Lower California, thus showing that there is a strong inshore northerly current along the entire coast.

December 4.—The president, Dr. Blake, in the chair. Prof. Whitney exhibited a collection of fossils made by Mr. J. E. Clayton in Nevada, near the 116th meridian, and not far from the mining settlement of Eureka. These fossils are very interesting as representing the Primordial or Potsdam period of the Silurian, and exhibiting the same combination of genera and species of the *Lingulidæ* family of the brachiopods, and the *Paradoxidæ* family of trilobites, which is so characteristic of this group farther east. Indeed there are no families represented in these specimens but these two. The trilobites are very imperfect, much broken, and crowded together in great numbers in the rocks. The same *Agraulos* (*Arionellus* of Barrande, and *Crepicephalus* of D. Owen, Meek, and Hayden), which occurs in the Big Horn Mountains, about longitude 167°, is found in this lot from the 116th meridian. There is also a *Conocoryphe* (Conocephalites); but the *Agraulos* is much the most abundant. The brachiopods appear to be represented by at least two genera, *Lingulepis* (Lingula) and *Obolella*. The lithological character of the rock in which these fossils occur is of importance, as it is not a sandstone, but a limestone. The Primordial or "Potsdam sandstone" fossils have not, previously to this discovery, been found to the west of the Big Horn mountains, so far as appears from any published documents. The discovery is therefore an interesting one, and will furnish a valuable datum-point for working out the geology of the Great Basin.—Mr. Montrange read a paper on White Island on the coast of New Zealand. Whakari, as the Maoris call it, is White Island, is situated in the Bay of Plenty, on the east coast of New Zealand (North Island). Hours before reaching it, one sees the large crest of vapour which crowns its summit. It is of very difficult access, and very few even of the oldest settlers have ever visited it. The whole of the island is one perpetually active crater, which, like the mollusc that secretes its little shell, has built up its huge cone, three and a half miles in circumference at the base, 860 feet high, of scoriæ and indurated ashes. The walls of the cone are straight, cut at intervals with deep longitudinal furrows; the crater is inside, on the eastern side.—Prof. Marsh, of Yale College, who was present at the request of the President, made some remarks on the results of his trip from Nebraska and Dacota across the continent to Eastern Oregon. He stated that the extensive fresh water deposits that had been found in Nebraska and Dacota were again met with in Eastern Oregon, extending, in fact, across the continent. The Oregon beds were as rich in new and interesting fossil remains as those on the eastern sides of the Rocky Mountains. During his trip across the continent, he had selected a large number of vertebrates, amongst which were thirty or forty species, which he considered entirely new. The family *Equidæ* were represented by several new species, furnishing important material for tracing the development of the group through its intermediate forms. Another interesting fact in connection with these fresh water deposits is, that whilst the fossil fauna of Eastern Oregon abounds in species identical or analogous with those of Nebraska and Dacota, yet extensive fresh water deposits were met with in Wyoming and the centre of the continent in which the fossil fauna was of an entirely distinct character, although belonging to the same geological epoch, the Miocene.

PARIS

Academy of Sciences, May 13.—M. Chasles presented a further series of theorems relating to the theory of the obliques of a curve—M. L. Cailletet communicated a note on the influence of pressure upon the bands of the spectrum, in which he describes the increased resistance offered by compressed gases to the passage of the electric spark and its influence on the luminous phenomena produced, and stated that whilst the luminous intensity of the bands of the spectrum is increased by pressure, when the latter is extreme they disappear entirely, the spectrum becoming continuous.—M. Melsens forwarded a memoir on lightning conductors with multiple conductors; and M. Decharme a note on the spontaneous ascensional movement of liquids in capillary tubes, compared with the flow of the same liquids in the same tubes under a constant artificial pressure.—A note was read by M. Arnould Thenard on the decomposition of carbonic acid under the influence of electricity.—M. Balard presented a note by M. Amagat on the dilatation of moist gases.—M. Lamy commented on a recent note by M. Personne on the presence of selenium in sulphuric acid of French manufacture, and indicated that its existence had been known for the last ten years.—M. Wurtz also presented a note by M. Scheurer-Kestner upon the same subject.—A note was read by M. J. Boussingault on the determination of carbon combined with meteoric iron.—M. C. Robin communicated a note by M. H. Byasson on the hydrosulphate of chloral (sulphuretted chloral).—M. Bouchut presented some investigations on the action of the bases and alkaloids obtained from opium.—M. Robin presented a note by M. J. P. Megnin, on the development of the unarmed cestoid worms, in which he described his observations on an undetermined species allied to *Tænia perfoliata* (Goeze), *T. plicata* (Rud.), and *T. mamillaria* (Mehlis), discovered by M. Baillet and himself in the horse and mule. He seems to think that all the stages of development of this parasite are passed in the same animal.—M. Clos presented a note upon a portion of the leaf in certain plants, to which he gives the name of *prelimb*.—The Minister of Foreign Affairs communicated a report received from the French Consul-General at San Francisco, relating to an earthquake which occurred in the county of Inyo on March 30.—A note on the silicified plants of Autun, with observations on the structure of *Dictyoxylon*, by M. B. Rénault, was presented by M. Brongniart.—A letter from M. Palmieri on the late eruption of Mount Vesuvius, dated May 5, was read.

VIENNA

Academy of Sciences, January 4.—Prof. L. Gegenbauer, of Krems, forwarded a second memoir on the evaluation of definite integrals.—Dr. F. C. Schneider noticed the production of a detonating iodine-compound by treating oxyiodide of mercury with solution of iodide of potassium containing iodine. The detonating compound was formed as a crust over the residue of oxyiodide and upon the sides of the glass in which the mixture had remained for a fortnight; its violently explosive qualities were discovered on an attempt being made to remove it by means of a glass rod. —M. J. Schlesinger deposited a sealed note on the formula for the rapidity of outflow of water from tubes.—Prof. von Oppolzer announced the re-discovery on December 20, 1871, of the lost planet Ægina (91).—Dr. Sigmund Exner presented a memoir entitled "Further Investigations on the Structure of the Olfactory Mucous Membrane in the Vertebrata," in which he showed that the branches of the olfactory nerve in birds, mammals, and in man, terminate in the same way as was previously described by him in the frog. The author regarded the glands of the olfactory region as tubular, and not acinose.—Dr. A. Boué communicated a reply to M. Blaek's remarks on his catalogue of northern and southern lights, and M. H. Fritz forwarded a note relating to the same subject.

January 11.—Dr. L. J. Fitzinger communicated a memoir on the natural family of the Pangolins (*Manes*), and M. S. Adler some mathematical demonstrations connected with the game of dominoes.

January 18.—A memoir by D. A. Seydler on the path of Dione (106) was read.—Dr. F. O. Sofka communicated six short papers on various mathematical and physical subjects.

BOOKS RECEIVED

ENGLISH.—Introduction to the Study of Palæontological Botany: J. H. Balfour (A. and C. Black).—Fruit Trees, 2nd edition: W Wardle (Lockwoods).—Nature: A. Walker (Longmans).—The Fallacies of Darwinism: C. R. Bree, M.D (Longmans.)

AMERICAN.—Annual Record of Science and Industry for 1871, edited by S. F. Baird.—The Lens, edited by S. A. Briggs, vol. i., No 2.

DIARY

THURSDAY, MAY 30.

ROYAL SOCIETY, at 8.30.—The Bakerian Lecture: On the Structure and Development of the Skull of the Salmon: W. K. Parker, F.R S.—On Ammonia in the Urine in Health and Disease: Dr. Tidy and Dr. Woodman.—The Structure and Functions of the Rods of the Cochlea: Dr Pritchard.—Examination of the Gases occluded in Meteoric Iron from Virginia: Dr. J. W. Mallet.

SOCIETY OF ANTIQUARIES, at 8.30.—Ballot for the Election of Fellows.

FRIDAY, MAY 31.

ROYAL INSTITUTION, at 3.—Old and New Art: E. J. Poynter.

SATURDAY, JUNE 1.

ROYAL INSTITUTION, at 3.—On the Chemical Action of Light: Prof. Roscoe, F.R.S.

GOVERNMENT SCHOOL OF MINES, at 8.—On Geology: Dr. Cobbold, F.R.S.

MONDAY, JUNE 3.

ROYAL INSTITUTION, at 2.

ENTOMOLOGICAL SOCIETY, at 7.

ANTHROPOLOGICAL SOCIETY, at 8.—On the Artificial Enlargement of the Earlobe in the East: J. Park Harrison, M.A.—On the Westerly Drifting of Nomads—the Fins: H. H. Howorth, M.A.—On Tumuli at Sapolia, Russia: Baron de Bognschefoky.

VICTORIA INSTITUTE, at 8, Anniversary Meeting.

TUESDAY, JUNE 4.

ROYAL INSTITUTION, at 3.—On Development of Belief and Custom E. B. Tylor, F.R S.

ZOOLOGICAL SOCIETY, at 9.—On Dinornis (Part XIX.) containing a description of a Femur, indicative of a new genus of large wingless bird (*Dromornis australis*, Owen) from a post-tertiary deposit in Queensland, Australia: Prof. Owen, F.R.S.—On the Anatomy of the Two-spotted Paradoxure (*Nandinia binotata*): Prof. Flower.

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.—On the Political Condition of Egypt before the Reign of Rameses III.: Dr. August Eisenlohr.—Some Mathematical Observations on the Dimensions of the Base of the Great Pyramid, and the Royal Coffer: Solomon M. Drach.—The XXXVII. Aamu in the Tomb of Chnum-Hotep, at Beni Hassan, identified with the Family of Israel: Daniel H. Haigh.

WEDNESDAY, JUNE 5.

GEOLOGICAL SOCIETY, at 8.—Notes on Sand-pits, Mud Volcanoes, and Brine-pits, met with during the Yarkand Expedition of 1870: Dr. G. Henderson.—On the Cervidæ of the Forest-bed of Norfolk and Suffolk: W. Boyd Dawkins, F.R.S.—The Classification of the Pleistocene Strata of Britain and the Continent by means of the Mammalia: W. Boyd Dawkins, F.R S

MICROSCOPICAL SOCIETY, at 8.—Remarks on the Homological Position of the members constituting the Thecated Section of the Rotatoria: Chas. Cubitt.—On a Micro-pantograph: Isaac Roberts.

THURSDAY, JUNE 6.

ROYAL INSTITUTION, at 3.—On Heat and Light: Prof. Tyndall, F.R S.

SOCIETY OF ANTIQUARIES, at 8.30.

LINNEAN SOCIETY, at 8.

CHEMICAL SOCIETY, at 8.

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THURSDAY, JUNE 6, 1872

BAD GREEK OR GOOD GERMAN?

AN event occurred on Thursday last at Cambridge, not in itself, perhaps, of imposing magnitude, but yet fraught with very important consequences. For this long while back an agitation has been going on with the purpose of making Greek no longer absolutely essential to the Previous Examination (or "Little Go," as it is popularly called), but of allowing French or German, or both, to be substituted for it at the option of the candidate. As any long-headed man might have foreseen, the genuine scholarship and liberal intelligence of the University are in favour of such a change; but the opposition has been neither feeble nor silent. Discussion has abounded more and more, and "fly-sheets" have fallen like the latter rain. The advocates of the change seem to have been more or less governed by a dislike to many words, and to have had large faith in the merits of their cause; their opponents, on the other hand, appear to have believed in the efficacy of much speaking, and in the effects of arguments drawn from all quarters, and looking all ways; their papers and speeches, all put together, form as pretty a piece of incoherence as may be found in a literary day's march, and would have been a perfect godsend to the great Skepsius when he wrote his famous tract *An hominibus mens absit.* The reasons indeed for making the change were so clear and cogent that there seemed hardly any hope of its being accomplished. Yet by one of those freaks of fortune which are met with even in the Universities, wisdom prevailed; and by the vote of the Senate on Thursday last, which will, in all probability, be speedily ratified at a second meeting, the student who desires to go out in an "honours" examination henceforth need not at his Little Go scratch up a smattering of bad Greek, if he satisfies his examiner that he possesses a real knowledge of French or German.

We trust that the scientific workers at Cambridge will take heart at this happy issue of the struggle, and gird up their loins for the heavy task of introducing order and system into the chaos in which the natural science studies at Cambridge are now lost. Let them set to work at once, and no longer wait for that *Deus ex machina* of the Royal Commission, who at present sit aloft, like the gods in Tennyson's "Lotos Eaters," and of whom it might be said, "Though their wheels are grinding finely, yet they grind exceeding slow."

The graduates of the University of London too might do well to ponder over this result. It is one of the marks of good tone at Cambridge to be very imperfectly acquainted with the Metropolitan University, except so far as its scholarships and examinerships are concerned; and accordingly it was stated more than once in the course of the discussion, and used as an argument against the proposed change, that the University of London had recently refused to make Greek optional at its Matriculation Examination. Our better informed readers are probably aware that the Senate, the real governing body of the University, have the matter at this very moment under their consideration, and, without wishing to forestall the future, we may presume to say that beyond doubt a change will soon be made. It is perfectly true that Convocation, in spite of the Report of its Annual Committee, hesitated to recommend the change; and this seems to have led to the mistake of the Cambridge advocates of bad Greek; but it is well known that, as indeed a sound knowledge of human nature would lead one to expect would be the case, there is among the body of graduates of the radical University, a mass of partly rabid and partly stupid conservatism, which, if it had its own way, would soon bring the University to ruin. Happily the executive Senate, being for the most part selected by the Crown, is wise and liberal, and is especially animated by the feeling that the University, if it is to fulfil its function, must grow with the growth of time, and change with changing things.

It is not a little to the credit of the older University of Cambridge that she should have been actually the first to remove one more of the old-fashioned swaddling clothes, which have been checking the development of youthful science, and we trust it is an earnest of still greater changes which she means to take in hand. Science has been too long at that old University a sort of blind Samson, bound with many cords, and serving chiefly to make sport for mocking Philistines of the classical and mathematical tribes. It is time his cords were loosed, and his strength made use of for the general advancement of the University.

OUR NATIONAL INDUSTRIES

IT is believed by many scientific men that research is all but dead in England. Whether we confess it or not, England, so far as the advancement of knowledge goes, is but a third or fourth-rate power. It is not our present purpose to inquire into the causes of all this; whether, as some say, it is because our professors are so rich, or whether, as others affirm, because all arrangements for the increase of knowledge are so poor, but rather to call attention to the certain influence of this on the wealth—let us put it in the most sordid manner—of the nation in the future.

In this inquiry we find to our hand, in a recent number of the *Birmingham Morning News*, an article on the future extension of Birmingham industries, by Mr. George Gore, whose important researches are well known. We know no one better qualified than Mr. Gore to discuss the subject, and no town where it is more important that the subject should be ventilated, for Birmingham has received much from and has given nothing to original scientific research; but the conclusions to be drawn from the article are in no way limited to Birmingham.

In this article Mr. Gore first considers by what general means the chief trades of Birmingham were first originated and improved; and then discusses whether we can by similar means, applied in a more effectual manner, lay the foundation of other new trades and improvements. Mr. Gore writes:—

"Let us consider German-silver and its manufacture. That substance is an alloy of copper, zinc, and nickel; it owes its peculiar whiteness or 'silver-like' appearance to the latter metal, and cannot be made without it; it is certain, therefore, that by whatever means that metal or the alloy was discovered, the discovery was the origin of the German-silver manufacture, and was essential to all manufactures, processes, or appliances, in which German-silver, nickel, or any of its compounds are used. Nickel was discovered by Cronstedt during the year 1751, and its compounds were chiefly investigated by English and foreign chemists. Cronstedt found it as a peculiar metal in the mineral called kupfernickel, whilst chemically examining the properties of that substance. The general method by which he discovered it was careful experiment, observation, and study of the properties of matter. I believe it is a fact that the Chinese and other nations made alloys of nickel long before nickel itself was known to be a separate metal; they had found, by experiment, that when ores of copper and zinc were mixed with a particular kind of mineral and smelted, a white alloy was obtained; but this also proves the general statement already made, that the German-silver manufacture was originated by means of experiment and observation. It was by a more skilful, but similar mode of procedure, that Cronstedt discovered the metal itself, and thus laid the basis of improvements in the manufacture of its alloys. I need not here enlarge upon the multitude of uses to which nickel has already been applied in Birmingham manufactures, nor speak of the large sums of money which have been and still are made by means of it and its compounds. . . .

"The manufactures of iron-wire and copper-wire for telegraphs are two other modern trades of great magnitude in this town, and were originated in the following manner:—In 1799, Volta, an Italian philosopher, was experimenting, observing, and studying the electric properties of metals in liquids, and discovered the Voltaic battery. In 1815, Prof. Oersted, of Copenhagen, was experimenting on the relation of electric currents to magnets, and observed that when a magnet was suspended near and parallel to a horizontal copper-wire, through which an electric current was passing, the magnet moved spontaneously, and placed itself at right angles to the wire. From these two small experiments, made by putting matter and its forces under new conditions, observing and studying the results, all our telegraphs and the immense manufactures of iron and copper telegraph wire have arisen.

"There is a saying, that 'all great things have had small beginnings,' and this is true, not only of electric telegraphs, but also of the great trade of electro-plating, and of the magneto-electric machine, which is now largely used instead of the Voltaic battery. After Volta had made his small and apparently unimportant experiments on the electricity produced by metals and liquids, various persons tried the effect of that electricity upon metallic solutions. Brugnatelli, in 1805, found that two silver medals became gilded in a solution of gold by passing the electricity through them. Mr. Henry Bessemer, in 1834, coated various lead ornaments with copper by using a solution of copper in a similar manner. And in 1836 Mr. De la Rue found that copies might be taken in copper of engraved copper plates by the electro-depositing process. Faraday discovered magneto-electricity in the year 1831, by rotating a disc of copper between the poles of a magnet, and he has stated that the first successful result he obtained was so small that he could hardly detect it. This simple experiment was the origin of the magneto-electric machine, and many of those machines are now used by Messrs. Elkington for depositing copper, silver, and gold, instead of the Voltaic battery.

"Another large manufacture of this district is that of phosphorus. The origin of it is due to the man, whoever he was, who first isolated that element. Histories of chemistry tell us that it was discovered by Brandt, a merchant of Hamburgh, in 1669; but evidence exists that it had been obtained in the separate state very many years before by the early Arabian chemists. Brandt obtained it by distilling a mixture of dried residue of urine and charcoal. His discovery was also made by careful experiments, and observation of the properties of matter, and had it not been made there would have been no manufactures of phosphorus or phosphorus matches in this district.

"Priestley made many experiments on the absorption of gases by water, and proposed such liquids as beverages, and those apparently trifling experiments have since expanded into the large manufactures of aerated waters."

After having given these instances out of many, the manner in which these practical results have been obtained is stated:—

"Persons inexperienced in scientific matters are apt to think that discoveries are generally made by accident. The reverse is, however, the case; nearly all our great modern discoveries were effected by men who were constantly making careful experiments upon the properties of matter and its forces, by subjecting them to new and definite conditions. Nearly all persons look upon such discoveries as fortunate ideas, which, when once found, are quickly developed, instead of which they are in most cases, slowly developed results of most difficult mental labour. Discoveries in science are occasionally made, not by original scientific investigators, but by practical men engaged in manufacturing or technical employments. The hydro-electric machine originated in this way: a man at Newcastle was attending to a steam-boiler, and found that he received electric shocks when he touched the boiler. This circumstance was investigated by his employer, Mr., now Sir William, Armstrong, and led him to construct the hydro-electric machine. The accumulation of electricity in submarine telegraph cables was also first observed at the Gutta-Percha Company's works, London. It was noticed on testing the cable by means of a voltaic battery (the cable being submerged in water) that discharges of electricity flowed from the cable after the battery was removed; this circumstance was investigated by Faraday, and led to improvements in submarine telegraphy. In these instances also the same general method was employed, viz., new experiments were made (though not intentionally) by putting matter and its forces under new conditions, and new results were observed. . .

"Scientific discovery, therefore, by developing new facts and laws relating to matter and its forces, constitutes not only the basis of new manufactures, but largely, also, of the improvements in trades made by inventors and practical men; and if discoveries are not made, the means by which improvements are effected by such men will become exhausted. The great value of new scientific knowledge to such men is proved by the fact that when new scientific discoveries are published there are numerous inventors and practical men who immediately endeavour to apply them to useful purposes. Since the first application of coal-tar to the production of dyes, every discovery in that branch of chemistry has been closely watched for a similar purpose.

"According to all our experience, scientific discovery provides the knowledge necessary for making inventions, and practical inventions lead to increase of trade. It might easily be shown that in this way scientific research has already resulted in the employment of whole armies of workmen, and in the expenditure and investment of a fabulous amount of money in railways, telegraphs, machinery, gasworks, chemical works, electro-plating, photography, &c., &c., in this country; and Birmingham has received a large share of the benefit."

We now come to the point that we are anxious to enforce:—

"The future success of this town and district is dependent upon original scientific research to a degree of which persons in general can form but little conception. Hundreds of millions of pounds are being expended in covering the earth with telegraphs, and thousands of millions in covering it with railways, gasworks, waterworks, &c., and Birmingham and its district has its share in supplying the rails, the wire, and the machinery. In this country alone more than 550,000,000 of pounds have been already expended upon railways only. *Original scientific research is the great fountain-head of industry*, and its capability of developing increased trade is practically unlimited: it is at present quite in its infancy, and we are only on the very threshold of a knowledge of the forces of nature, and of the constitution of material substances; and if such enormous results are being produced by the beginnings of unaided science, what may be expected from its future developments, especially if scientific research is assisted in an effectual manner?

"Numerous important subjects of investigation, capable of yielding valuable results bearing upon the trades of this town, exist in all directions. Researches in electricity and in inorganic chemistry, particularly the metals and their compounds, would probably lead, as they have done before, to the establishment of new trades, and to improvements in local manufactures, and thus lay the foundation of future commercial prosperity. Discoveries in science, however, are best made, not by trying to obtain some valuable commercial or technical result (that object belongs to an inventor), but by making new, reliable, and systematic investigations. By investigating the chemical action of electricity upon saline bodies, Sir Humphry Davy isolated sodium and magesium, which has led to the recent establishment in Manchester of the manufactures of those metals. By the abstract researches of Hofmann and others upon coal-tar, the immensely profitable manufacture of the splendid coal-tar dyes was originated.

"Scientific discovery is the most valuable in its ultimate practical results when it is pursued from a love of truth as the ruling motive, and any attempt to make it more directly and quickly remunerative, by trying to direct it into practical channels, will decrease the importance of its results, diminish the spirit of inquiry, and sooner or later reduce it to the character of invention. The greatest practical realities of this age had their origin, not in invention or a search for utilities, but in a search after important new truths, entirely irrespective of what utilities they might lead to.

"I do not intend by these remarks to imply that any new trades or improvements in manufactures have been or can be effected without the labours of inventors and practical men; but that there should be a more judicious division of labour, one man to discover new truths, another to put them into the form of practical inventions, and the practical business man to work them; because it is proved by experience that in nearly all cases these different kinds of labour require men of widely different habits of mind, and that the faculties of discovery, invention, and practical manufacture, are very rarely united in one man.

"Our large manufacturers and men of business have accepted and employed the advantages of science in an endless number of ways in their occupations, and have thereby acquired great wealth; but, notwithstanding this, and that the greatest trades of this district were originated and improved largely by means of scientific investigation, scarcely any of the wealthy manufacturers or landholders of the locality, who have derived such great benefits from the increase of trades, give the least assistance to scientific research; that which is the duty of all has been attended to by none. The probable explanation is, original scientific research is a subject quite outside the experience and knowledge of persons in general. It may be objected that such research is not aided, because it sometimes takes a long time to acquire a practical shape and make it pay. We do not omit to plant an acorn because it requires many years to become an oak; we do not neglect to rear a child because he may not live to become a man; but we leave scientific discovery to take care of itself."

England's present and special weakness is then referred to:—

"Our practice with regard to science is very different from the plan carried out in Germany. Within the last few years great laboratories have been erected in Berlin, Leipzig, Aix la Chapelle, Bonn, Carlsruhe, Stuttgardt, Griefswald, and other places, at the expense of the State, and special provision has been made in them for original scientific research. A glance at the frequently published list of scientific investigations made in different countries will show us that the Germans are making a far greater number of discoveries in science than ourselves. If we are to maintain our position as a manufacturing nation, we also must adopt special means to promote scientific research; for how can we expect to obtain new arts and manufactures, or improvements in old ones, if we do not make new discoveries in the properties of matter and its forces? I need not multiply instances of the essential dependence of our present commercial success upon abstract scientific research, but may safely affirm that nearly all our great manufactures have been originated by means of experiment, observation, and study of matter and its forces; and that the great bulk of the improvements made in manufactures by practical men could not have been effected had not scientific investigators discovered, and made known in books, the properties of bodies. The inference from these conclusions is obvious: by adopting similar means, but in a more effectual way, we shall obtain similar but more successful results."

And this being so, what is the actual condition of things? According to Mr. Gore, "at present, original scientific researches are generally made by teachers of science, who spend a portion of their scanty incomes in making experiments, and lead lives of great self-denial in the labour. There is absolutely no provision in this country for the support of scientific investigators, and thus the great source of new trades and improvements in manufactures remains undeveloped."

Surely if scientific men are convinced, first, that the future of our national industries depends upon research, and secondly, that there is no research, the time has arrived when action of some sort is incumbent upon them if they are ever to take action in any subject whatever; for it is perfectly obvious that any bettering of such a state of things can only proceed from the action of the scientific men themselves.

We have let Mr. Gore speak for himself thus at length, as in his article there is ample endorsement of much that has already appeared in this journal, but he is by no means the only witness that we can appeal to. Even the President of the Chemical Society is compelled to acknowledge that the original researches brought before that Society have fallen nearly to zero. Commenting on this, the *British Medical Journal* points out that the institutions which were formerly considered the homes of research, are now silent. If these things be true, then if those who hold that research is a national necessity are right, our future position is not far to seek.

THE HIGHLANDS OF CENTRAL INDIA

The Highlands of Central India. By Captain J. Forsyth, Bengal Staff Corps. (London: Chapman and Hall.)

THIS is a book descriptive of that great tract of hill and forest country which is situated in the very centre of the Indian Peninsula, and whose drainage forms the headwaters of the Narbadá, the Tapti, the Sône, and affluents of the Màhànadi.

The author, who, in the course of his professional duties, acquired a close and intimate acquaintance with the country and its inhabitants, commences by giving a sketch of the probable history of the various races found in this wild and, until lately, almost unexplored country—races whose ethnological and religious history possess peculiar interest; because here the Aboriginal tribes, the races of mixed Aryan and Aboriginal blood, and the Hindoos proper, can be found living side by side; and the effects of the colonisation of the district by the Aryans of Hindustan can be traced out with some degree of clearness.

A considerable portion of the book is devoted to the author's sporting experiences. These he relates in a lively and spirited style; but as the reminiscences of Indian sportsmen have a strong family likeness, this part of the book does not call for much notice, except that the author appears to make rather too light of the danger of bison-shooting, and makes some statements concerning this game which are scarcely correct, one of these statements being that the animal is always known under the name of *bhìnsa*, or more correctly *bhainsa* (a word resembling in sound the English word bison), and never as *gaur*. Now in the very district of Northern Belàspùr which the author describes in the latter portion of the book, there is a high and prominent hill much frequented by these animals, and called in consequence Gaur-duàri, or the pass of the bison. The author also appears to confound the bison of Central India with the similar but not identical animal, known as the Mithan or Mithna, found on the hill ranges of Eastern and North-Eastern Bengal.

From the description given of some of the shrines and pilgrimages met with in these hill districts, an idea may be formed of the way in which the Hinduism of Hindustan proper has been localised; and how the scenes of some of the principal events of Hindu mythology have been transferred from the Ganges and Jumna to the Narbada and its adjacent heights.

The latter portion of the volume is devoted in a great measure to the country lying under and east of the Mykul range; that is the range which, according to the author's definition, forms the eastern boundary of the Highlands of Central India. This tract of country is remarkable, because it presents, perhaps, the greatest sweep of unbroken jungle and forest to be found in India south of the Ganges and Brahmaputra. It is neither plain nor hill, but rocky with gentle undulations, and covered with forest. Only a few miserable villages are found throughout the whole country; just enough to make its wildness and desolation more striking. Such a country, unfitted by nature for cultivation, and exhaling a deadly malaria, must long remain the home of the elephant, the tiger, and of the animals on which the latter preys; nor are there found any remains or traditions which would indicate that the place had ever been more thickly populated than it is at present. Here man is the inferior animal, and the wild elephant is the lord of the country. He roams at will from place to place, and, just when the unhappy villager looks to gather in his scanty rice crop, wrung with difficulty out of an unwilling soil, he invades the fields and browses at leisure on the ripening grain, utterly scorning the feeble efforts of the hapless owner to drive him away. Probably no European has braved the deadly malaria of the country at the time of the ripening of the rice crop, or that of the drying up of the rains, but the deep and enormous foot-prints imprinted in almost every field by this jungle ravager remain visible throughout the dry season, and tell the tale of spoliation only too plainly.

Throughout this district, and indeed with most of the tribes who inhabit the wilder and more remote parts of Central India, such religion as the people possess is "devil" worship and fetishism, with just so much gloss of Hinduism as may be imparted by a rare visit to a Hindu shrine, or the presence of a brahmin whenever anything is to be got—which is seldom.

A Gond legend, taken down from the lips of one of their most celebrated bards, and translated by the author into blank verse, in imitation of Longfellow's "Song of Hiawatha," is given at length; it is interesting because it brings in here and there some curious little bits of savage life, and also from the very strong resemblance of the whole legend to that on which Longfellow's poem is based. Moreover, as the author points out, a still more curious analogy will suggest itself to the careful reader.

The whole work is written in a very lively and readable style. It will be found amusing by those fond of sporting anecdotes; and though the subjects may be thought to have an interest too local for the general reader, yet the spectacle here presented of races of men just emerging from utter barbarism, and acquiring the rudiments of civilisation, gives to the country and its inhabitants an interest which it would not otherwise possess.

M. T. SALE

OUR BOOK SHELF

Lecture Notes for Chemical Students.—Vol. II. Organic Chemistry. By E. Frankland, D.C.L., F.R.S. (London: John Van Voorst.)

IN this, the second part of "Lecture Notes for Chemical Students," Dr. Frankland develops very fully his own peculiar notation. The use of thick letters, as $\mathbf{C}H_4$; of contracted symbols for many organic radicles, as Ayo for $C_5H_{11}O$; of a peculiar way of writing the formulæ of well-known substances, so as at first sight to make them appear as if they were new compounds, as $\mathbf{N}PhO_2$, representing nitrobenzol; these and a few other remarkable characteristics will make this book, we should think, seem rather startling to the ordinary student. By combining attendance on lectures on organic chemistry with careful reading and a good deal of patient work, the student will find these "Lecture Notes" very useful, containing as they do, in small space, the results of the latest investigations as these bear upon the molecular constitution of organic compounds. Very large use is made of graphic formulæ, and also of the theory of types. The types used are somewhat different from those with which we are familiar in the text-books, and certainly the names applied to them are derived more from an anatomical than a chemical vocabulary; thus, the marsh gas type is termed the "Monadelphic," the methyl type the "Diadelphic," and so on. The very useful and scientific method of writing the formulæ of all organic acids, as containing the group COHO; all alcohols, as containing CH_2HO; and all aldehydes, as containing COH, is adopted throughout. The relations of alcohols, aldehydes, and acids; the passage from one class of alcohols, &c., to another; and very many other points of great scientific value, too commonly overlooked in the text-books, are here all carefully noted. As we said, the book requires attentive study, but this it will certainly repay.

M.M.P.M.

Thermal Paths to the Pole. An Address delivered before the St. Louis Mercantile Library Association in January 1872. By Silas Bent.

IN this pamphlet the author repeats the substance of a lecture delivered in 1868, the object of which was to show that the continuations of the warm Gulf Stream of the Atlantic, and of the Japan current in the Pacific, afford the only practicable avenues by which ships can enter an assumed open sea round the North Pole; and points out how the more recent Arctic explorations have confirmed the views then advanced.

The author's opinions should derive weight from the fact that he was one of the leading scientific observers in the American expedition of 1852, during which the Japan current was mapped out, and from his twenty-five years of observations at sea.

So far as the warm drift continuing the Gulf Stream into the Arctic region between Spitzbergen and Nova Zemlia is concerned, the theory of its influence (which, however, can hardly be called "original," since it has been current among Arctic authorities for many years) has indeed been remarkably confirmed in the past year by Payer and Weyprecht's voyage in open water to 79° N. lat. But it remains to be shown that the summer current from the Pacific through the narrow and shallow passage of Behring Straits has any considerable influence on the condition of the Arctic basin. In the circumpolar chart which accompanies the pamphlet, Behring Straits has been carefully widened to admit the Kuro-Siwo in a breadth quite equivalent to that of the Gulf Stream drift.

A considerable portion of the address is devoted to the description of a method by which "were it not for the inhumanity of exercising such a power," the whole of Europe might be placed at the mercy of America. Europe derives its mild climate from the Gulf Stream, and to divert this stream from its present direction would be to make "Europe a frozen wilderness." This grand result, the author believes, could be accomplished "by the possession of the Isthmus of Panama and the expenditure of half the cost of the recent war between France and Germany, in the excavation of a sufficient width and depth of the rock only that intervenes between the Caribbean Sea and the Pacific." Mr. Bent has himself, however, thrown some doubt on the entire practicability of his design by quoting, in a previous paragraph, the belief expressed by Professor Maury that the great mass of the Gulf Stream is formed by that part of the equatorial current of the Atlantic which passes to northward of the Antilles, and which "must be a hundredfold greater than that which returns to the east from the Gulf of Mexico;" he has also omitted to notice that the force of the drift in the Caribbean Sea is not directed in any degree against the narrower portion of the isthmus, and we presume that even Mr. Bent would not attack the plateaus of Guatemala or Mexico.

K. J.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Spectroscopic Nomenclature

THE letter of Captain Herschel upon "Spectroscopic Nomenclature," which appeared in NATURE of April 25 contains many criticisms that are eminently just and timely, but there are one or two points in respect to which I should like to express dissent.

Thus as regards the name D_3 (to which he objects) for the yellow line of the chromosphere spectrum, it is not easy to see what other designation would better convey to the mind an idea of its position in the spectrum and its importance, without involving any assumption, or hypothesis even, as to the material causing it.

To say nothing of the fact that the whole Greek alphabet would not suffice to name one in three of the bright lines which have been observed in the chromosphere spectrum, there is this further difficulty, that if the letters are to be applied to lines in the order of discovery, ω is as likely to fall between α and β as anywhere else, but if according to position in the spectrum, then every discovery of new lines involves a revision of the nomenclature.

It seems doubtful therefore whether any better system is possible than to designate lines by reference to some standard map of the spectrum, as stars are catalogued by their right ascension and declination.

An accurate chart of the solar spectrum on which the lines should be mapped according to "inverse wave-length," proposed by Captain Herschel himself I believe, as well as by Mr. Stoney and others, would sufficiently resemble the spectrum seen in a spectroscope to be equally convenient in the observatory with that of Kirchhoff, and would be free from the reproach of arbitrariness and irregularity in its scale. Such a chart would be most gladly welcomed by all spectroscopists, and would immediately supersede those of Kirchhoff and Angstrom.

With reference to the green corona line, he writes "and now we have '1474.' No one knows what the true position of that line is. The line 1474 K is an iron line, and it is to the last degree improbable that the corona line is identical with it." I am not quite sure what is meant by the second clause. If only, that the position of this line may *possibly* (not by any means *probably*) be doubtful to the extent of ¼ of one of Kirchhoff's scale divisions, that is about ¼ the distance between the two E lines, I have nothing to object.

But if the sentence is intended, as one would naturally suppose, to convey the idea that the position of the line is not very accurately determined, and may be considered uncertain to the extent of several scale divisions, it is certainly wrong I *know* of what I affirm, and perhaps may be allowed to refer to an article in this journal for Feb. 2, 1871, in which the evidence is stated as it was at that time, and it has received confirmation since.

Indeed as this bright line is almost always visible in the chromosphere to an instrument of sufficient power, I think I may confidently appeal to Mr. Lockyer or Dr. Huggins to bear me out in the statement that the bright scarlet line of the chromosphere appears to coincide no more perfectly with the dark C, than does this green corona line with the dark line at 1474 K.

I confess I am almost sorry that the spectrum of iron shows a bright line coincident with 1474, for all things considered, I cannot think that iron vapour has anything to do with this line in the spectrum of the corona, and the coincidence has probably only served to mislead.

But there are in the spectrum many cases of lines belonging to the spectra of different metals coinciding, if not absolutely, yet so closely that no existing spectroscope can separate them, and I am disposed to believe that this close coincidence is not accidental, but probably points to some physical relationship, some similarity of molecular constitution perhaps, between the metals concerned.

So in the case of the green coronal matter, is it not likely that, though not iron, it may turn out to bear some important relation to that metal? And yet I for one should be very glad if the application of higher dispersive power should show the apparent coincidence to be merely a very close juxtaposition.

C. A. YOUNG

Dartmouth College, U.S.A., May 16

Historical Note on the Method of Least Squares

THIS excellent method for the discussion of observations was published and first practically applied by Gauss in his Theoria Motûs, 1808. In the *American Journal of Science* for June 1871, Mr. Cleveland Abbe has shown that Prof. Robert Adrain, of New Brunswick, New Jersey, U.S., independently discovered the same method in 1808. I wish to call attention to what seems to me a singular oversight in the history of this subject, viz., to the fact that in 1770—1773, Lagrange published an elaborate memoir at Turin under the title "Mémoire sur l'utilité de la Méthode de prendre le Milieu entre les resultats de plusieurs Observations," &c. *Vide* "Œuvres de Lagrange," edited by J. A. Serret, vol. 2.

I have never seen any notice of this memoir except a translation of a part of it into German by Encke, published in the *Berliner Jahrbuch* for 1853. Thus in the abstract of a memoir by Mr. J. W. L. Glaisher, given in the notices of the Royal Astronomical Society for April 1872, the name of Lagrange does not occur.

I think that the English mathematician, Thomas Simpson, busied himself with this problem about 1750, but I am not able to refer to his works. ASAPH HALL

Washington, May 22

The Volcanoes of Central France

AN unlucky error, perhaps mine, in the letter on the "Volcanoes of Central France," p. 80, will quite prevent any reader finding the paper I mentioned of May 1865, which, instead of being in the *Gentleman's Magazine*, was in the *Englishman's Magazine*, a short-lived periodical, begun and ended, I think, with that year. As your two correspondents, Prof. Corfield and the Rev. Mr. Webb, like the writer of that paper, repeat the late Dr. Daubeny's most marvellous "conclusion" that there might have been nothing more eruptive in the phenomena than "bursting out of flames" from earthquake fissures, and even that the fires mentioned by Sidonius and Avitus might be "domestic conflagrations," may I briefly indicate the grounds that make these suppositions to me incredible? These fires, as named in the portions of each document that I have translated—quite distinct from the conflagration of some public building on the Easter festival of a previous year, which both writers afterwards relate at greater length as an earlier and less known case of successful prayer by Mamertus, the memory of which had encouraged him under these "prodigies" and "portents," the *ignes* (not *incendia*) that both writers make a chief or the chief part of the "terrors"—(Sidonius, indeed, names the earthquakes before them, but Avitus twice over puts the fires first)—these were *crebri* and *assidui*, continual for two or three years, yet not a word of what they fed on or what valuables they destroyed, and they were only *sæpe flammati*. Their being so sometimes is plainly named by Sidonius as an unusual and greater portent. Now, I never heard of any "domestic" fires that were *not* "*flammati*," whereas volcanic eruptions, even severe, seldom if ever involve flame truly so called, though their strongly illuminated smoke may often by night be mistaken for flames, and has led them to be called in extreme cases, as Sidonius here said, *sæpe flammati*. He adds that when thus "*flammati*" they did, or rather threatened to do, the only mischief named as even apprehended from them at the capital, the endangering frail roofs by a load of ashes thrown over, *superjecto favillarum monte*. Now, surely this is not an effect of any ædile conflagrations however often repeated (a repetition that would anywhere have been regarded rather as suspicious of incendiarism than as "prodigious" and preternatural). Nor would any such accidents lead Avitus to ask in his sermon to those who remembered all, "Who would not dread the Sodomitic showers?" Again, Mr. Webb conceives that earthquakes might not only drive the wealthier part of the population out of the city, "but, as it would seem, the beasts into it!" I never heard of shocks producing so singular an effect as driving any living thing into cities or buildings, and cannot conceive what natural event could so drive them, unless what is here by both witnesses implied, "Sodomitic showers" of hot or cumbering *favillæ*. Such showers, which we know to be often carried, from eruptions involving no lava, scores or even hundreds of miles, in the direction of the prevailing winds, would be carried from any of the well-known cones of the Forèz or Vivarais, towards, or even far beyond, Vienne; and wild animals, fleeing north-eastward, would have no refuge but under roofs; and if private house doors were habitually shut (as now in England) might crowd into the colonnades (*fori latera*) of that capital city. This incursion of the wild deer, bears, and wolves into towns was so well remembered as to become, in the later chroniclers, Gregory of Tours, &c., dwelt upon among the main "prodigies" of the time, along with the earthquakes and burnings of buildings, though any other fires cease to be implied; and the reason of this is obvious on comparing their accounts. They all copy one another, and the earliest, whose sole authorities were those two pompous and involved writers, mis-read them exactly as our moderns (except Sir F. Palgrave) appear to have done, confusing together the fires of the "prodigies," that led to the Rogation fasts with the earlier ædile conflagration at some Easter, said to have been prayed out by Mamertus, which occupies both the writer and preacher immediately after, and at greater length than these well-known "terrors" remembered by those they addressed personally.

The whole strikingly shows, as Sir F. Palgrave said, the fallacy of geological inferences from the "silence of history" (or what may be deemed silence) in times and places practically prehistoric, or at least preter-historic. He showed that, but for Pliny and a mere accident, we should probably have been as ignorant of even the Pompeii and Herculaneum eruption as of these equally attested ones. Again, the Spaniards would have preserved us no memory of the rise of Jorullo, in the very last century; and yet probably no part of Gaul in the generation when the Romans lost it was really more settled and populous than Mexico in its third century of Spanish rule. The only important colonies within moderate shower-range of the eastern volcanoes were Vienne and Lyons, the latter farther off, and not at that time a capital, indeed but little heard of in those early middle ages. And fires, not called damaging, only "prodigious" and terrifying to Vienne, and causing "Sodomitic showers" there, need not have been within a few miles, but far in the wilds, then hardly trodden, of its mountainous south-western horizon.

E. L. GARBETT

7, Mornington Road, N.W., June 1

Temperature of the Deep Sea

WILL you allow me to ask, through your pages, if there be any rule for ascertaining the temperature of the sea at given depths below the surface? To practical electricians such a rule would be very valuable.

I will state a case. There is a submarine cable connecting two stations, A and B, 150 miles distant. The temperature at A is 75° Fah.; that at B, 68° Fah.; and the average depth at which the cable lies 120 fathoms: what is the average temperature of the cable?

If you could refer me to any work in which this point is treated I shall be obliged. F.

ENDOWMENT OF PROFESSORSHIPS

THE following correspondence between Professors H. E. Roscoe and B. Stewart, of Owens College, Manchester, and the Chancellor of the Exchequer, is published in the *Times* of Monday last:—

"TO THE RIGHT HON. ROBERT LOWE, CHANCELLOR OF THE EXCHEQUER.

"Owens College, Manchester, May 21, 1862.

"SIR,—In the *Times* of May 17 you are reported, at the presentation for Degrees at the University of London, to have pointed out 'how the endowment of Professorships naturally tended to make teaching inefficient (seeing that the revenues come in independently of the results of teaching), suggesting that those who had any money to spare for the advancement of education should rather make it available in the forms of Scholarships and Exhibitions.'

"While we gratefully acknowledge the many services which, as Chancellor of the Exchequer, you have rendered to the cause of knowledge, we yet feel most strongly that the above expressions are calculated to mislead, and that were your suggestions to be carried out, the result would be fatal to the higher education of this country.

"We therefore request permission to lay before you our own views on this most important subject. Writing from the very house once inhabited by Cobden, we feel proud to be connected with a city which was the birth-place of Free Trade; yet we feel equally privileged to form part of a very useful institution which never could have existed except in apparent contradiction to the principles of Free Trade.

"That the foundation by the late John Owens of Professorships of Arts and Sciences in the midst of this great city was not thought by Cobden to be subversive of his principles is proved by the fact that he himself was one of the original trustees, yet this conclusion does not appear equally clear to all of his disciples.

"We are, in sober truth, utterly at a loss to conceive how the higher education of the country can be efficiently carried on without a moderate endowment of its Professorships. The necessity for such an education you yourself admit.

"A single example from our own staff, which, more or less, applies to other places and subjects, will render our argument clear. It is evidently of very great importance that in a place like Manchester the citizens should be taught by a master mind the principles of political economy, and they have been fortunate in being able to avail themselves of the services of such a man as our colleague, Professor Jevons. But, although here both elements of pecuniary success might appear to be present in an intelligent public and a first-rate teacher, the fact remains that without the (misguided !) endowment of our founder the few who attend his lectures could not have benefited from the teaching of Professor Jevons unless the fees of attendance had been enormously increased. Indeed, we question whether the great apostle of Free Trade himself would have ultimately met with success had he not first of all received some sort of protection and support.

"We are naturally led by the instance we have quoted to remark that endowments really tend to diminish the expenses of education, and, looking around us, we see that in University College and King's College (London), where there are no endowments, they cannot afford to give their education at so low a figure as is possible at Owens College and in the Scotch Colleges, where endowments exist.

"In the German Universities, again, where all the important Chairs are well endowed, the expenses of education are almost nominal. In Scotland the education is in some branches of a very high standard, and in others great improvements have recently taken place, chiefly in the direction of relieving the head Professors from the duty of teaching junior classes which pay, and of enabling them to devote their energies to senior classes which do not pay. Such, in Scotland, have been the effects of endowments. Again, with regard to Germany, we have never heard any complaints made of the inefficiency of the German Professors.

"We must candidly own that we were much surprised by your statement as to the advisability of simply founding Scholarships and Exhibitions, coming, as it does, from a distinguished Oxford man well acquainted with the present state of feeling in the older Universities. Is it not true that this feeling is strongly against the extension of the already too numerous Scholarships, Fellowships, and other incitements to study, and in favour of the application of these funds to increase the paltry salaries of the Professors?

"The excessive endowment of Scholarships appears to us to be objectionable, as an instance of unnecessary protection, where, by means of a hotbed regimen, young men are induced to enter a profession for which there is no subsequent career.

"While we admit that in a perfect state of society (unhappily still far distant) the laws of supply and demand may perhaps be applicable to all knowledge, yet we must point out that the teachers of the higher branches have too often now to create a taste for the commodity which they supply, and hence we believe that the moderate endowment of Professorships, such as exists in our own case, is essential to the progress of civilisation in this country.

"In conclusion, Sir, we cannot understand why endowment naturally tends to make teaching inefficient in the case of a Professor of science or arts more than it does in that of a minister of religion or a statesman.

"Are they not all servants of the nation administering to its higher needs? The teacher of science or of the arts will, we venture to say, be no less conscientious and faithful to the true interests of a noble cause in teaching his class than the minister of religion in addressing his congregation, or the Minister of State in addressing his constituents.

"We are, Sir, your obedient servants,
"HENRY E. ROSCOE, B.A. (Lond.) F.R.S.
"BALFOUR STEWART, LL.D., F.R.S."

"11, Downing Street, Whitehall, May 23, 1872

"Gentlemen,—The speech which I made at the annual meeting of the London University occupied three-quarters of an hour, and was reported in a few lines. I never alluded to Professors, but spoke only of teachers, meaning those who do the drudgery or hard work of teaching, not those who are devoted to the investigation and inculcation of higher and more refined knowledge. I have the greatest respect for Mr. Jevons, and do not doubt that the endowment of his Chair is money well laid out.

"I also agree—indeed, I said—that the endowment both of Fellowships and Scholarships at Oxford and Cambridge is excessive; but I pointed out how hard the competition was for the London University, with strict examinations and hardly any endowment, against Oxford and Cambridge, with rich endowments and easy examinations. I added that in my judgment money was better spent in giving Exhibitions to young men, leaving them free to choose the place of their education, than in paying persons to teach them; since in the one case the inducement to the teacher to work was diminished, while in the other the student with money in his hand was sure to find the best teacher for himself.

"I am, Gentlemen, your obedient servant,
"ROBERT LOWE.

"I am an older Freetrader than Mr. Cobden, and am by no means prepared to assent to his views in all respects."

GLAISHER'S (HALL'S IMPROVED) RAIN GAUGE[*]

IN the first paragraph of my "Notes on the Rainfall of 1871," which recently appeared in NATURE (vol. v., p. 481) your readers will probably have noticed certain reference to the above.

The improvement to which I refer consists of an *inverted rim* (similar to the rim or flange in which the receiver stands) fixed to the outside cylinder of the receiver, and made sufficiently large to admit of its dropping over the rim or flange, sometimes called "channel," fixed to the lower cylinder, *id est*, the one just mentioned in parenthesis.

The *inverted rim* is shown by a thick line on the right of the accompanying half-sectional diagram.

The reason that I suggested this addition was, that on one occasion, while registering the daily rainfall at Twickenham, during the winter of 1869-70, I was unable to take the receiver off, owing to the already existing channel being partly filled with water, which had frozen hard during the night.[†]

It is intended that water should be collected in the flange (Glaisher's) I have spoken of, and thus close the gauge against evaporation, scarcely a good idea theoretically, *certainly not practically*, inasmuch as the water collected therein so soon evaporates, especially in hot weather; *vide* "Symon's British Rainfall, 1868;" "Rain Gauge Experiments at Strathfield Turgiss, Reading," by the Rev. C. H. Griffith, F.M.S., &c., p. 23, which further establishes my remarks. The absence of outlet for confined air here spoken of, might be remedied, if indeed needed, by drilling small air-holes in the bottom of the receiving cylinder and upright flange, *but not facing each other*. I have two Glaisher's gauges fitted with the inverted flange arrangement, both of which answer remarkably well. I believe the improvement which I have adopted is more effectual against loss by evaporation (during all weathers) than the present (Glaisher's) system.

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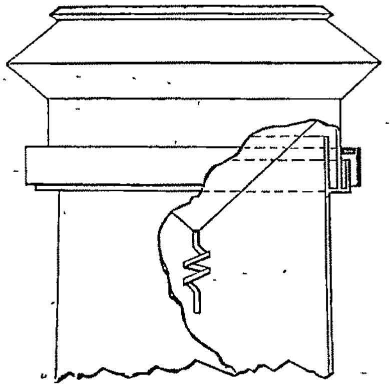
[*] Vide *Scientific Opinion*, vol. iii pp. 449, 450

[†] In order to avoid the interference of houses and trees, my gauge was supported at this time on a bracket carriage, running in vertical slides from a staircase window to a point a few feet above the roof of my residence.



One of my gauges has been further improved at my suggestion by being fitted with a spiral or *helical* pipe in the place of the J-shaped pipe, thereby presenting no direct opening for evaporation, at the same time offering little or no hindrance to the speedy descent of the rain-water; but this is a matter I hope to enlarge upon in a paper (as yet unpublished) which I hope to communicate to you shortly, "On a Proposed New Form of Rain Gauge (the Atmospileometer)," in which a similar, but more extensive, idea, is shown.

The particulars mentioned in paragraphs 1 and 3 of this letter were long since communicated to and approved by the Secretary of the Rainfall Committee of the British Association (G. J. Symons, Esq; F.M.S., &c.) one of the highest authorities in matters relating to rainfall.

JOHN JAMES HALL

WATER ANALYSIS

I.

IT is now upwards of twenty years since the inhabitants of this country, and especially of the metropolis, were awakened, by a succession of virulent attacks of epidemic cholera, from the profound indifference with which they had regarded all matters connected with the public health for the hundred and eighty years which had succeeded the Great Plague. During that interval builders had been allowed to cover land with hundreds of acres of dwellings built without regard to ventilation, drainage, or water supply. In all the towns and villages of the country the ground was honeycombed with cesspools and wells, the latter deriving their supply of water at least in part from the former, and in all riverside towns the river either received the town sewage at once or after it had passed through the cesspools. Attention once being drawn to the matter, it became the duty of the chemist to detect the various polluting matters introduced by the sewage into the different sources of water-supply, and to discover, if he could, waters that were free from this pollution; and a still greater field was opened up, for the two first inquiries naturally led to the allied questions—How can sewage be rendered harmless? and Can slightly polluted water be rendered safely drinkable by the removal from it of the contaminating matter introduced by sewage? We propose in this article to look only at the first of these questions, the one on the successful solution of which depend all the others—Can organic contamination be detected and estimated with accuracy? As soon as the question was approached it was found to be one of extraordinary difficulty, and in 1856 Hofmann and Blyth drew attention to the inaccuracy of the then existing processes, especially of the one known as "loss on ignition" obtained by igniting the solid residue on the evaporation of the water. This loss, then generally looked on as affording a measure of the organic matter present in the water, they proved to consist of a loss of carbonic anhydride, nitric acid, ammonia, and moisture, &c., and they proposed to render the determination more accurate by the addition of a known weight of sodic carbonate, which, while it drove off the ammonia (usually a very small fraction of the loss), retained the acids and prevented the aqueous magnesic chloride from losing hydrochloric acid. The same chemists pointed out the necessity of determining the amount of nitrogen present, but were unable to recommend any process for its estimation. The methods for estimating the ammonia were also very unsatisfactory, for we find Dr. Dundas Thomson in 1855 distilling as much as fifty gallons of the metropolitan water-supplies in order to estimate the ammonia, which was done by titration with standard acid; and this when some of the metropolitan supply was taken from the Thames at Vauxhall, and "Fibrin from Fæces" could be distinctly recognised in the Southwark Company's water. Another process, devised by Forchhammer, was also in use for the determination of the organic matter, which consisted in adding a standard solution of potassic permanganate until no further loss of colour occurred. This process had been improved from time to time, and was and is largely used. The only other test was that for hardness, invented by Dr. Clark, and which is still in use, with but slight modification from the original method.[1]

If these processes are considered but shortly, the defects they possess are at once apparent. Take, for instance, Hofmann and Blyth's improved solid residue process. On ignition there was great danger of decrepitation and consequent loss, notwithstanding the high temperature (120° to 130° C.) to which the residue had been exposed. Frankland and Armstrong have shown that portions of the nitrogenous matter were liable to remain fixed in the ignited residue as cyanogen compounds. Again, in the case of some artificial residues prepared by treating dilute solutions of urea as in the above process, from 44 to 59 per cent. of the urea used was found to have been lost during the preliminary evaporation, the sodic carbonate having expelled it as ammonic carbonate. And on the treatment of similar residues by ignition, from 58 to 85 per cent. of the organic matter was left in the residue. It was usual also to restore the lost carbonic anhydride to the ignited residue by evaporating a solution of that gas on it and weighing until a fresh treatment did not increase the weight; but to still further increase the difficulty of this unhappy process, it was shown that some residues seemed to have the power of taking up such quantities of carbonic anhydride, that they weighed more after this treatment than they did before ignition. The estimation of ammonia by titration with acid needs no argument against it; the enormous quantities of water necessary for the determination sufficiently condemn it; and it has been long superseded by the admirable quantitative form of the Nessler process invented by the late Mr. Hadow, of King's College. The permanganate process, however, being an easy one to perform, still survives in the laboratories of many analysts. Indeed, not content with giving the results of this determination as "oxygen required to oxidise the organic matter present," the lively imagination of some led them to the remarkable conclusion that every grain of oxygen oxidised eight grains of "organic matter."

Whether this test is to be trusted may be judged from the following facts. Potassic permanganate is deoxidised by ferrous salts, nitrites, sulphites, &c., much more rapidly than by organic substances, so that a water absolutely free from any organic matter, but containing one of these compounds, would be set down as requiring so much "oxygen to oxidise organic matter." Secondly, in the case of water to which known weights of various organic compounds were added, Frankland and Armstrong found that in no instance was the oxidation complete, even after the lapse of six hours. In fact, even after that time the amount of oxygen actually absorbed was in every case a mere fraction of the quantity actually required to completely oxidise the organic substance. The test, though thus shown to be valueless as quantitative, is of some value qualitatively, as it can be easily and quickly applied; and it may be said that, though it might induce a person to abandon a good water, it would not often lead him to use a bad one.

All the above processes were in use up to 1868, when Messrs. Chapman, Wanklyn, and Smith proposed to determine the organic matter in water from the amount of ammonia evolved when the water was treated with a strongly alkaline solution of potassic permanganate and then distilled, the ammonia being determined in the distillate by Hadow's modification of the Nessler test.

That albumin is decomposed and the nitrogen thus evolved, they had shown in a paper presented to the Chemical Society in the preceding year. The way in which this process was applied to the water may be briefly stated as follows:—A measured quantity of the water was rendered alkaline with freshly-ignited sodic carbonate, and the ammonia distilled off and estimated by Hadow's modified Nessler process. As soon as all the ammonia thus obtainable had been expelled, the alkaline permanganate solution (50 cubic centimetres of a solution containing 200 grammes of potassic hydrate and 8 grammes potassic permanganate per litre) was run in. The distillation was then resumed, and the ammonia estimated as before. This last was set down as "albuminoid ammonia," and as the average evolution of ammonia from the following substances, gelatine, caseine, dry albumin, uric acid, creatine, theine, dried fish flesh, amounted to 10 per cent., it was suggested the albuminoid ammonia multiplied by 10 gave a fair estimation of amount of organic matter.

It had at first been stated that albumin gave up the whole of its nitrogen when treated with alkaline permanganate, but the statement was subsequently modified to "It appears to be two-thirds, being at any rate a constant quantity." Now this process would indeed be a valuable one if the 10 per cent. average could be depended on, or if the albumin evolved a certain quantity, and the above substances were alone found in water. Unfortunately, none of these suppositions are true. With regard to the last, the authors themselves recognised the difficulty, and accordingly examined a number of other nitrogenous organic bodies; which examination led to the publication of two lists of bodies that evolve the whole of their nitrogen as ammonia, and bodies that yield various fractions. Frankland and Armstrong also made some experiments on this subject. With regard to the list of bodies yielding half their ammonia, the numbers given by the authors vary from 44 per cent. in the case of papaverine to 58 per cent. in the case of sulphate of cinchonine; and whilst narcotine appears in Wanklyn, Chapman, and Smith's list as evolving all its nitrogen, Frankland and Armstrong give it as evolving about 46 per cent. Strychnine, given by the former authors in their list as evolving 53 per cent., is given by the latter as evolving 31½ per cent., and sulphate of quinine also in the list with 50 per cent., appears again with Frankland to have evolved nearly 57 per cent. No other examples will be necessary to show the extreme uncertainty of the process. If the authors had enabled us to ascertain the absolute error on the quantity taken instead of the per-centage error, by giving us the quantities from which the results were taken, it would no doubt be much more apparent; the results given above in the case of those from Frankland and Armstrong's paper are absolute errors.

But it may be urged in defence of the method that none of these bodies are found, or are likely to be found, in natural waters. Let this be granted, and the process must be defended on the albumin and other bodies mentioned, and on the list of bodies giving up all their ammonia. The average from this is 11·82 per cent.; from the albumin list, 9·92; or, taking the two, 10·87. So far the lists hold good. But it must be borne in mind that we have three different statements about the ammonia evolved from albumin—first, that all is evolved; secondly, "two thirds, or, at any rate, a constant quantity;" thirdly,[*] that 100 parts of albumin give 10 parts of ammonia.[†] The inconsistency of these statements needs but this comment, that they can only be caused by the extreme uncertainty of the process; in fact, the amount of nitrogen converted into ammonia will be influenced by the nature of its previous combination, the degree of concentration of the solution, and the amount of heat applied to the retort, and consequent rate of distillation and time to which the solution is exposed to the action of the alkaline permanganate. That this is the case is proved by the fact that water which has been distilled from alkaline permanganate, and gives no trace of reaction with Nessler's test, will evolve ammonia if again boiled with the permanganate. Lastly, let any one take a water which has been largely contaminated with sewage and then filtered, such as the effluent water from a sewage farm. Such water, as a rule, contains much nitric and nitrous acid, and comparatively small quantities of organic nitrogen. A water of this character continues to evolve albuminoid ammonia till boiled nearly to dryness, and not unfrequently the retort requires to be filled up with pure water, and the operation carried on. The process is thus not only rendered tedious, but the necessity of repeatedly taking samples of the distillate and estimating the ammonia in them introduces an amount of experimental error which becomes serious when calculated out in milligrams per litre, though its actual amount on each cylinder of distillate may be very small.

When it is added that Mr. E. T. Chapman has included in the second edition of Wanklyn's "Water Analysis" a process for the estimation of volatile organic matter, founded on the fact that water largely contaminated with sewage evolves volatile bases when boiled with potassic hydrate, the question is still further complicated; for it cannot be doubted that some portion of these bases would be driven off by the action of the alkaline permanganate before it had time to act on and destroy them.[‡] Whether this is the case ought at once to be determined by those who use the process.

In a second article we shall consider Frankland and Armstrong's process for the analysis of potable water, and also those determinations, such as nitrous and nitric acids and chlorine, which are of great value as enabling us to trace back the history of a water, and to tell from whence it is derived and what it has received in the way of animal contamination before it came into our hands.

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[*] "Water Analysis," 2nd edition, p. 66.

[†] If Lieberkuhn's formula for albumin $H\,Na\,C_{72}\,H_{110}\,N_{18}\,S\,O_{22},\ H_2O$ be taken as the true one, 10 parts of ammonia (NH_3) from 100 parts of albumin will be equal to 8·23 parts, or little more than half the total nitrogen, which is for that formula 15·25 per cent. If the sodium in the above formula be replaced by hydrogen, the discrepancy is still greater.

[‡] An action of this nature apparently occurs when sewage is treated with alkaline permanganate, as nearly the whole of the ammonia comes off in the first 100 cub. cent. of the distillate, and after that the evolution soon stops. Waters that evolve small quantities of albuminoid ammonia seem almos always to require a long time for its liberation. Is this because the organic bases are so diluted that they cannot be driven out, and so slowly decompose

RADIATION AT DIFFERENT TEMPERATURES

BALFOUR STEWART states in his "Elementary Treatise on Heat" that "Newton was the first to enunciate his views on the cooling of bodies. He supposed that a heated body exposed to a certain cooling cause would lose at each instant a quantity of heat proportionate to the excess of its temperature above that of the surrounding air." In order to prove the fallacy of Newton's supposition, Prof. Stewart presents the following extract from the work of MM. Dulong and Petit:—

Excess of temperature of the thermometer.	Velocity of cooling.
°C	°C
240	10·69
220	8·81
200	7·40
180	6·10
160	4·89
140	3·88
120	3·02
100	2·30
80	1·74

"We see at once from this table," says Prof. Stewart, "that the law of Newton does not hold, for according to it the velocity of cooling for an excess of 200° should be precisely double of that for an excess of 100°; now we find that it is more than three times as much." The author of the Elementary Treatise on Heat thus assumes that the velocity of cooling established by Dulong and Petit represents the radiant energy or quantity of heat transmitted by the radiator. Consequently, the amount of energy at 200° is assumed to be $\frac{10·69}{1·74} = 6·14$ times greater than at 80°; while, agreeably to Newton's law, the increase of radiant energy should be proportional to the differential temperature, viz., $\frac{240}{80} = 3$ times that of the tabulated temperature of 80°. Modern research having established that radiant heat is energy amenable to the laws of dynamics, it may be demonstrated that the deviation from the Newtonian doctrine assumed by Dulong and Petit is groundless; but, before considering the theory, let us examine the practical result of recent elaborate experiments conducted with an apparatus containing the spherical radiator adverted to in the preceding article on Solar Temperature (vol. v. pp. 505-507). The accompanying illustration (Fig. 1) represents a vertical section of the said apparatus, *a* being a spherical vessel 5 inches in diameter, suspended within an exterior casing *b*, filled with water. A spherical radiator, *c*, 2·75 inches in diameter, composed of very thin copper, charged with water and coated with lamp-black, is sustained in the centre of the sphere *a* by means of tubes applied above and below. The upper tube is large enough to admit the bulb of a thermometer, the lower one being only sufficiently large to accommodate a small axle, to which is attached a paddle-wheel, provided with curved paddles, arranged in such a manner that the bulb of the thermometer may be inserted considerably beyond the centre of the sphere, as shown in the illustration. The external casing *b* is provided with nozzles, *g* and *d*, to which tubes are attached for circulating cold water through the annular space during experiments. The air is exhausted from the spherical enclosure through the tube *k*, which passes across the annular space. It will be evident that the centrifugal action of the paddles of the wheel applied within the radiating sphere will produce a continuous current from the centre towards the circumference, the fluid successively passing over and coming in contact with the inside of the thin shell, then returning to the centre to be again thrown off by the centrifugal action. The rotary motion of the water, kept up without intermission round the cylindrical bulb of the thermometer, will evidently render its indication prompt and reliable. It is hardly necessary to observe that the rapid presentation of fresh particles of water promoted by the action of the paddles, will effectually prevent the reduction of temperature to proceed faster at the circumference than at the centre, the radiation at the surface, in virtue of the continuous interchange of particles, affecting almost simultaneously every molecule within the sphere. Consequently the total energy of radiation will be rendered available in reducing the temperature of the contents of the radiator; while the central thermometer will indicate at every instant the precise degree of temperature of the entire mass.[*]

The mode of conducting the experiment will be seen by the following statement:—A wooden cistern containing 16 gallons, charged with water and crushed ice, is connected by flexible tubes to the nozzles *g* and *d* on opposite sides of the annular space, a pump being applied between the cistern and the said nozzles, by means of which the cold water is forced through the apparatus and then returned to the cistern.

In view of the great importance of the question at issue, the investigation has been conducted with the utmost care, four operators having invariably been employed during the experiments, the labour being thus divided: 1st operator regulates the temperature of the water in the cistern by continual agitation and supply of crushed ice from time to time; 2nd operator works the pump at a uniform rate; 3rd operator turns the paddle-wheel, and reads the thermometer under a magnifying glass, calling time for each degree at the instant when the top of the mercurial column is covered by half the thickness of the line on the scale. Lastly, the 4th operator, provided with a Casella chronograph, records the time called. It will be seen presently that, notwithstanding this procedure, there is a slight discrepancy in the ratio of temperature and time, viz., the increment of time for each degree is not regular. Obviously the most practised eye cannot determine exactly at what moment the top of the falling column is half covered by the line on the thermometric scale. Again a perfectly graduated thermometer cannot be obtained. But the discrepancy referred to in reality only disfigures the record, since the computations are based on *mean* time. Referring to the accompanying table, it will be seen that the rate at which the spherical radiator cools has been recorded separately for each degree of differential temperature from 100° to 10°, the enclosure being maintained at a constant temperature of 33°. Regarding the construction of the table, it will suffice to state that the time entered in the fourth column is that shown by the chronograph. It will be evident on reflection that the increment of time for each successive degree of differential temperature expresses very nearly the rate of cooling; but, the recorded times being irregular, from causes already pointed out, the true increment cannot be determined without ascertaining the mean time recorded by the chronograph. This mean time will be found in the fifth column, the true increment, viz., the number of seconds during which the temperature of the radiator falls one degree, being entered in the last column.

Let us now examine the accompanying diagram, Fig. 2, in which the ordinates of the curve *a b* represent the observed time for each degree of differential temperature, while the ordinates of the curve *a c* represent the corrected time. The diagram having been constructed with the utmost exactness, in accordance with the temperature and time in the table, mere inspection will show that the observed and corrected times have produced curves nearly identical. Agreeably to Newton's law the rate of cooling is proportional to the excess of temperature of a body above that of the surrounding medium. Hence the increment of time for each degree, in other words, the number of seconds occupied in reducing the temperature of the radiator 1° (inserted in the last column of the table) should be proportional to the differential temperature inserted in the third column. For instance the rate of cooling at a differential temperature of 490° being 39·80 seconds for 1°, it should be $\frac{49 \times 39·80}{31} = 62·90$ seconds for an equal thermometric interval at a differential temperature of 31°. Referring to the table, it will be found that the rate thus computed agrees exactly with the increment of time inserted in the last column opposite the differential temperature 31°.

Applying a similar test to the other differential temperatures and rates of cooling contained in the table, the same exact agreement will be found to exist. Consequently, our table and diagram prove that the rate of cooling is proportional to the differential temperature, thus establishing the correctness of the Newtonian law. Regarding the discrepancy indicated by the slight irregularity of the curve *ab*, the writer attributes the same to the difference of emissive power of the radiator at different temperatures. It was stated in the preceding article, (vol. v. pp. 505-507), that the radiant power of one square foot of cast-iron develops 0·080 thermal unit per minute for each degree of differential temperature at 65°, and 0·337 unit at 3,000°; hence that the emissive power is increased $\frac{0\cdot337}{0\cdot080} = 4\cdot21$ times for an increment of 3,000 − 65 = 2,935°. Experiments conducted in the mean time show that the radiant power of one square foot of cast iron maintained at a differential temperature of 1,800° is 335 units per minute, hence that the emissive power at this stage of incandescence amounts to $\frac{335}{1,800} = 0\cdot186$ unit for each degree of differential temperature. Our investigations have thus proved that at 65° the emissive power is 0·080 thermal unit, at 1,800° 0·186 unit, and at 3,000° 0·337 for each degree of differential temperature. We have accordingly established the fact that the emissive power increases nearly in the same ratio as the intensities, being fully quadrupled between the differential tempera-

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[*] It might be supposed that the motion of the water within the radiating sphere, produced by the action of the paddle wheel, will occasion an elevation of temperature tending to render the indication of the central thermometer inaccurate. The requisite speed of the wheel being 30 turns per minute, experiments have been made to ascertain if at that rate heat is produced; but no elevation of temperature has been observed. The diameter of the wheel being 2·37in., the maximum speed of the particles of water produced by the rotation is scarcely 3·8ins. per second, a velocity too small to generate appreciable heat.

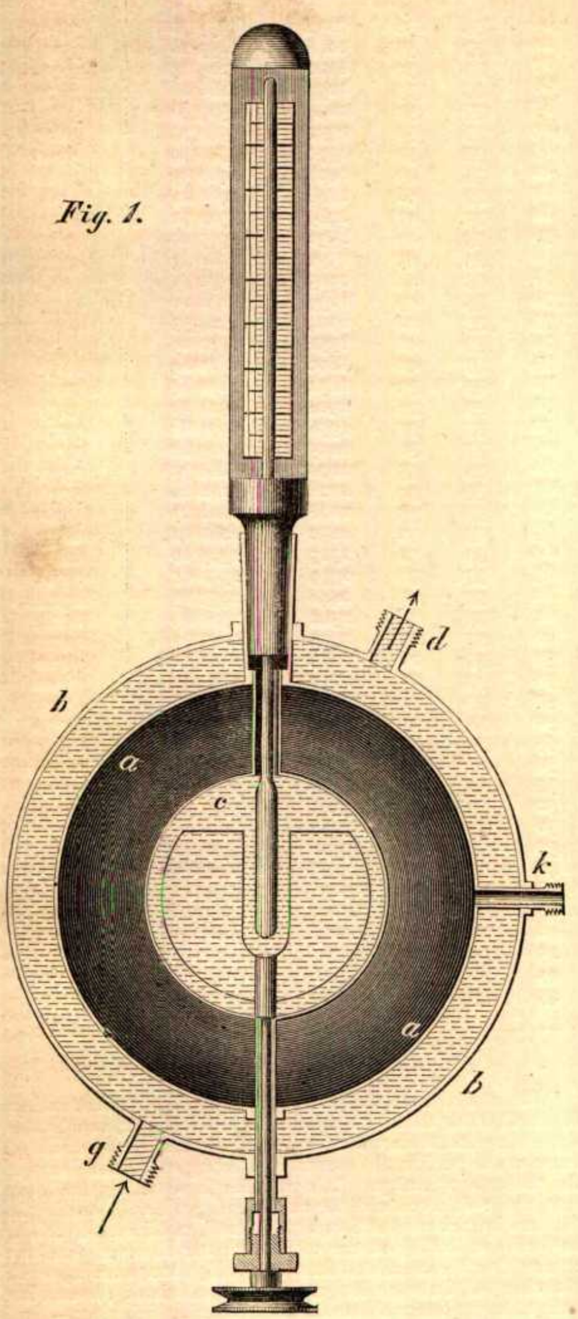

Fig. 1.

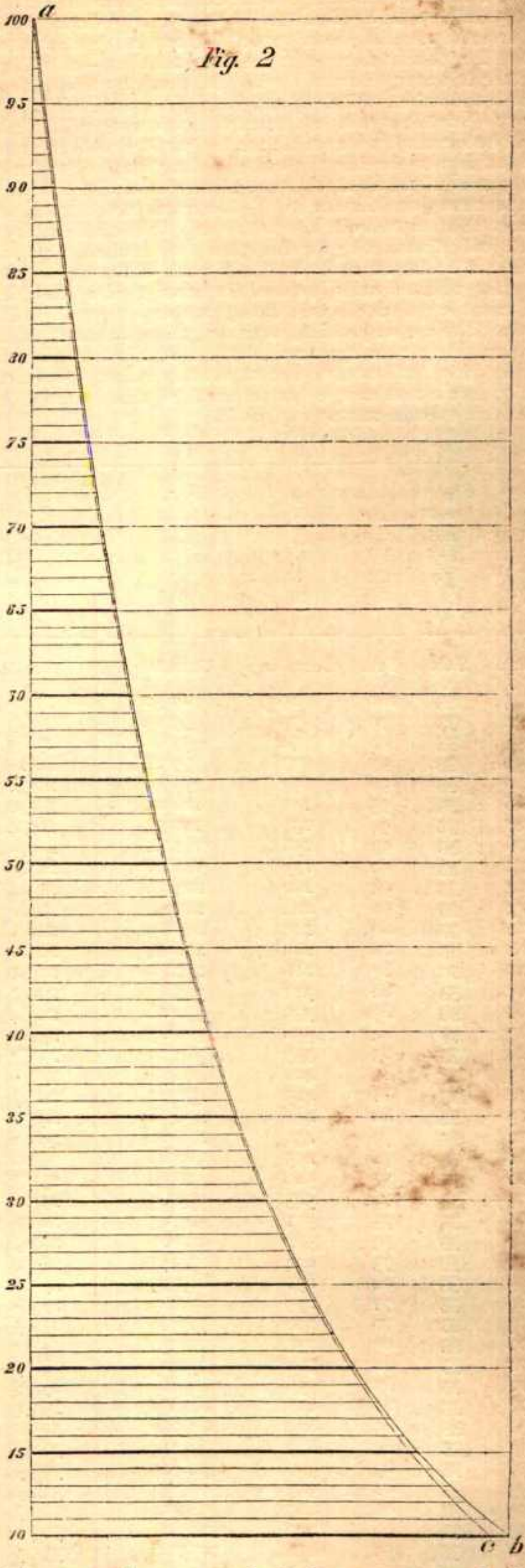

Fig. 2

ture of 65° and 3,000°. Let us be careful not to confound this increase of emissive power with the increase of radiant energy resulting from mere augmentation of temperature. It is, no doubt, owing to the change of the molecular constitution of the body during heating that the dynamic energy developed at a differential temperature of 3,000° is 4·21 times greater than it should be in accordance with the Newtonian law—a trifling increase, however, compared with that resulting from adopting the computations of Dulong and Petit, whose formula shows that for the stated range of temperature the ratio of radiant energy will be increased more than 4,000 times. It would be premature to attempt to explain the cause of the change of the radiant properties of metals at different temperatures disclosed by our experiments, until further investigations shall have established the exact relation between the actual and theoretical energy developed. Considering the difference of molecular motion within metallic bodies at white heat in a state of fusion, and at the freezing point of water, we need not be surprised at the variation of emissive power observed during our experimental investigation. Nor are we justified, in view of this variation of emissive power, in questioning the correctness of Sir Isaac Newton's assumption that heated bodies of definite radiant properties develop mechanical energies proportional to their excess of temperature over the surrounding media.

Temperature of the radiating sphere.	Temperature of enclosure.	Differential Temperature.	Observed Time.	Corrected Time.	Increment of time for each degree.
Fah.	Fah.	Fah.	Seconds.	Seconds.	Seconds.
133°	33°	100°	19·5	19·50	19·50
132	33	99	39	39·19	19·69
131	33	98	58	59·08	19·89
130	33	97	77	79·18	20·10
129	33	96	97	99·49	20·31
128	33	95	117	120·01	20·52
127	33	94	138	140·75	20·74
126	33	93	158	161·71	20·96
125	33	92	179	182·90	21·19
124	33	91	200	204·32	21·42
123	33	90	222	225·98	21·66
122	33	89	244	247·89	21·91
121	33	88	266	270·04	22·16
120	33	87	288	292·45	22·41
119	33	86	311	315·12	22 67
118	33	85	333	338·06	22·94
117	33	84	356	361·27	23·21
116	33	83	379	384·76	23·49
115	33	82	402	408·54	23·78
114	33	81	425	432 61	24·07
113	33	80	448	456·98	24·37
112	33	79	471	481·66	24·68
111	33	78	495	506·66	25·00
110	33	77	520	531·99	25·33
109	33	76	545	557·65	25·66
108	33	75	571	583·65	26·00
107	33	74	597	610·00	26·35
106	33	73	624	636·71	26·71
105	33	72	651	663·79	27·08
104	33	71	679	691·25	27·46
103	33	70	708	719·10	27·85
102	33	69	737	747·36	28·26
101	33	68	766	776·03	28·67
100	33	67	796	805·13	29·10
99	33	66	827	834·67	29·54
98	33	65	858	864·67	30·00
97	33	64	889	895·14	30·47
96	33	63	920	926·09	30·95
95	33	62	953	957·54	31·45
94	33	61	985	989·51	31·97
93	33	60	1017	1022·01	32·50
92	33	59	1050	1055·66	33·05
91	33	58	1084	1088·68	33·62
90	33	57	1118	1122·89	34·21
Fah.	Fah.	Fah.	Seconds.	Seconds.	Seconds.
89°	33°	56°	1152	1157·71	34·82
88	33	55	1188	1193·16	35·45
87	33	54	1220	1229·27	36·11
86	33	53	1257	1266·06	36·79
85	33	52	1294	1303·56	37·50
84	33	51	1331	1341·79	38·23
83	33	50	1371	1380·79	39·00
82	33	49	1408	1420·59	39·80
81	33	48	1448	1461·22	40·63
80	33	47	1489	1502·71	41·49
79	33	46	1530	1545·10	42·39
78	33	45	1572	1588·43	43·33
77	33	44	1615	1632·75	44·32
76	33	43	1659	1678·10	45·35
75	33	42	1704	1724·53	46·43
74	33	41	1751	1772·09	47·56
73	33	40	1802	1820·84	48·75
72	33	39	1852	1870·84	50·00
71	33	38	1904	1922·16	51·32
70	33	37	1958	1974·86	52·70
69	33	36	2015	2029·02	54·16
68	33	35	2070	2084·73	55·71
67	33	34	2128	2142·08	57·35
66	33	33	2188	2201·17	59·09
65	33	32	2250	2262·11	60·94
64	33	31	2313	2325·01	62·90
63	33	30	2379	2390·01	65·00
62	33	29	2448	2457·25	67·24
61	33	28	2520	2526·89	69·64
60	33	27	2595	2599·11	72·22
59	33	26	2674	2674·11	75·00
58	33	25	2754	2752·11	78·00
57	33	24	2839	2833·36	81·25
56	33	23	2929	2918·14	84·78
55	33	22	3025	3006·78	88·64
54	33	21	3126	3099·64	92·86
53	33	20	3232	3197·14	97·50
52	33	19	3343	3299·77	102·63
51	33	18	3459	3408·10	108·33
50	33	17	3581	3522·80	114·70
49	33	16	3715	3644·67	121·87
48	33	15	3859	3774·67	130·00
47	33	14	4015	3913·95	139·28
46	33	13	4185	4063·95	150·00
45	33	12	4370	4226·45	162·50
44	33	11	4571	4398·73	177·28
43	33	10	4792	4593·73	195·00

J. ERICSSON

[WE have received a communication by cable from the author, to the effect that the engraver has indicated, in Fig. 2, a continuous space between the curves *a b* and *a c*, whereas the space should cease at one portion, the curves intersecting each other between the ordinates 25 and 30. The mistake is so trifling that it can scarcely be observed with the naked eye, yet it will no doubt be detected by such men as Stewart, Maxwell, and Everett. It is to be found, on looking at the table, that the observed times are shorter than the calculated times at the high temperatures, while the observed times are longer than the calculated times at low temperatures; hence the curves must intersect each other.—ED.]

NOTES

THE *Pall Mall Gazette* states that the Earl of Portsmouth, who is the collateral representative of Sir Isaac Newton, has offered to the University of Cambridge, through the Duke of Devonshire (Chancellor of the University), all the papers of Sir

Isaac relating to scientific subjects which his lordship has inherited. Lord Portsmouth's gift is prompted by the feeling that these papers will be more fitly deposited in the library of the University of which Sir Isaac was so distinguished an ornament than in his own muniment-room.

THE visitation of the Royal Observatory took place on Saturday last. We have not yet received the Astronomer Royal's report; but there is a rumour that it will be proposed to the Government by the Board of Visitors that photographic and spectroscopic observations of the sun be added to the routine work of the Observatory.

IT has been decided that the first Meeting of the French Association for the Advancement of Science shall be held at Bordeaux in the autumn of the present year.

THE Prince of Wales will open the Bethnal Green Museum on Monday, the 24th inst. The Princess of Wales will accompany him. We should be glad to hear of free Scientific Lectures in connection with this Museum.

WE are glad to learn that the Royal Geographical Society are taking steps to press upon Government the importance of an expedition to the North Pole by way of Smith's Sound.

IN another column will be found a correspondence between the Chancellor of the Exchequer and Profs. Roscoe and Balfour Stewart with reference to the remarks made by the former at the presentation to degrees at the University of London, to which we drew attention a fortnight ago. It is with great satisfaction that we note Mr. Lowe's own version of his speech, on which we may say a word, although the accuracy of Mr. Lowe's defence has been called in question by Prof. Sylvester. If he and the Government which he represents remain firm to their avowed declaration in favour of the endowment of the higher professorships, the cause of scientific education will have reason to be grateful, the Professor being distinguished from the teacher by the presence of *investigation*, as well as of inculcation. The *Pall Mall Gazette* called attention in a forcible manner last week to the fallacies of the line of argument which Mr. Lowe was represented to have employed. To encourage a plethora of scholarships at the expense of the endowment of professorships would be a more perfect illustration of the maxim "How not to do it," than we have witnessed this long time.

THE proposed alterations in the examinations held during the first two years of a student's course in the University of Cambridge were submitted on May 30 to the Senate with varying success. The vote was taken on the principle, and not on the details, of the various propositions. The most important of those accepted were—to receive a knowledge of French and German as a substitute for Greek in the first or "Previous" Examination; and to add Heat as a subject in the "General" or Second Examination for the Ordinary Degree. The effect of these changes, when finally ratified, will be that a degree in Honours, in either Mathematics, Law, Natural or Moral Science may be obtained by a student who does not know Greek; but that an ordinary "Poll" or Pass Degree cannot be obtained without a knowledge of Greek. Also, a candidate for a degree in Honours will not be required to study (necessarily) any branch of Physical Science; but a candidate for a "Poll" Degree must have an elementary knowledge of Heat. As, however, several persons, in the debate which preceded the voting upon these questions, expressed themselves favourable to making some branch of Natural or Physical Science a part of the Previous Examination, it is, we conceive, not improbable that the Syndicate will embody some proposition of this kind in their amended scheme.

THE following arrangements are now made for the forty-second meeting of the British Association for the Advancement of Science, to be held at Brighton, and to commence Wednesday, August 14, under the direction of the following officers:—President—Dr. William B. Carpenter, F.R.S. Vice-Presidents—The Earl of Chichester, the Duke of Norfolk, the Duke of Richmond, K.G., the Duke of Devonshire, K.G., F.R.S., Sir John Lubbock, Bart., M.P., F.R.S., Dr. Sharpey, LL.D., Sec. R.S., Mr. Joseph Prestwich, F.R.S. General Secretaries—Dr. Thomas Thomson, F.R.S., Capt. Douglas Galton, C.B., F.R.S. Assistant General Secretary—Mr. George Griffith. Local Secretaries for the Meeting at Brighton—Mr. Charles Carpenter, Rev. Dr. Griffith, Mr. Henry Willett, the Pavilion, Brighton. Local Treasurer for the Meeting at Brighton—Mr. William H. Hallett, F.L.S. General Treasurer—Mr. William Spottiswoode, F.R.S. The General Committee will meet on Wednesday, August 14, at 1 P.M., for the election of Sectional Officers, and the despatch of business usually brought before that body. On this occasion there will be presented the Report of the Council, embodying their proceedings during the past year. The General Committee will meet again on Monday, August 19, at 3 P.M., for the purpose of appointing Officers for 1873, and of deciding on the place of meeting in 1874. The concluding meeting of this Committee will be held on Wednesday, August 21, at 1 P.M., when the Report of the Committee of Recommendations will be received. The first general meeting will be held on Wednesday, August 14, at 8 P.M., when the President will deliver an address; the concluding meeting on Wednesday, August 21, at 2·30 P.M., when the Association will be adjourned to its next place of meeting. The different Sections will assemble in the rooms appointed for them, for the reading and discussion of Reports and other communications, on Thursday, August 15, Friday, August 16, Saturday, August 17, Monday, August 19, and Tuesday, August 20, at 11 A.M. precisely. Authors are reminded that, under an arrangement dated from 1871, the acceptance of Memoirs, and the days on which they are to be read, are now, as far as possible, determined by Organising Committees for the several Sections before the beginning of the meeting.

PROFESSOR RUTHERFORD, of King's College, London, has been appointed Fullerian Professor of Physiology to the Royal Institution, in the place of Dr. M. Foster.

THE *British Medical Journal* gives currency to a report that a baronetcy is about to be conferred on Dr. William Stokes, Regius Professor of Physic in Trinity College, Dublin, and Physician to the Queen in Ireland.

WE have to record the death of one of our veteran botanists, Dr. Robert Wight, F.R.S., who died May 26, at his residence, Grazeley Lodge, near Reading, at the age of 76. Dr. Wight was a native of East Lothian, and very early in life entered the medical service of the East India Company, and while in this employment devoted his energies to the investigation of the then almost unknown flora of the British possessions in India. In 1834 he published, in conjunction with the late Prof. Arnott, the first volume of the "Prodromus Floræ Indiæ Orientalis," a work which was never continued. Further contributions to Indian botany were contained in his "Illustrations of Indian Botany," "Icones Plantarum Indiæ Orientalis," and "Spicilegium Neilgherrense," and in innumerable contributions to magazines and to the proceedings of societies. His name will also always be associated with his exertions towards the introduction of the cultivation of cotton into India. Dr. Wight was one of a band of botanists, to which Sir W. Hooker, Lindley, and Arnott belonged, who have now almost entirely passed away.

MR. JOHN B. LAWES, of Rothamsted, Herts, has announced his intention of placing in trust his laboratory and experimental fields with the sum of 100,000*l*., the interest of which, after his death, is to be applied to the continuation of the investigations which have been carried on for so many years at Rothamsted. It is seldom that we have to record an act of so great munificence directed in a channel calculated to bring about such important results to the scientific department of agriculture.

PROF. WATSON, the indefatigable planet hunter of the Ann Arbor Observatory, reports the discovery, on May 12, 1872, of a new asteroid (No. 121) of the eleventh magnitude. The following observations are communicated by him to *Harper's Weekly* :—

Ann Arbor.	Mean Time.	Right Ascension.	Declination.
May 12	$14^h\ 13^m\ 42^s$	$16^h\ 20^m\ 37{\cdot}58^s$	$-18°\ 53'\ 9{\cdot}4''$
,, 13	$11^h\ 13^m\ 22^s$	$16^h\ 19^m\ 59{\cdot}35^s$	$-18°\ 52'\ 46{\cdot}2''$

Daily motion in right ascension, -43^s; in declination, $+26''$.

PROF. FLOWER'S lectures on the Comparative Anatomy of the Organs of Digestion of the Mammalia lately delivered at the Royal College of Surgeons of England are being published with illustrations in the *Medical Times and Gazette*.

THE last Swiney lecture of the present year will be delivered on Saturday next, when the general subject of Science in relation to Education will be considered. This will terminate Dr. Cobbold's tenure of the Chair; which is only open to medical graduates of the Edinburgh University. The collective attendances for this series of sixty discourses will, we understand, have registered a total of upwards of 15,000, a result which is gratifying to the friends of popular scientific instruction.

MR. CARRUTHERS has printed his Official Report for 1871 of the Department of Botany in the British Museum. In consequence of the extent of the recent additions to the general herbarium, additional cabinets have had to be incorporated, and the old ones rearranged. The exhibition rooms have also been rearranged, with a view of making them more instructive and attractive to visitors. Several natural orders in the General and British Herbaria have been rearranged. Among the more important additions to the General Herbarium are 17,000 species, chiefly from Central Europe, Alsace, the Jura, the Lower Rhine, Spain, Mexico, and Labrador, being the herbarium of Auerswald, of Leipzig; 1,000 from Yucatan, collected by Dr. A. Schott; upwards of 1,000 from Russia; and upwards of 1,500 from Scandinavia, collected by Ahlberg.

IN a letter from General Otto Struve, director of the Palkowa Observatory, and Astronomer Royal of Russia, to Prof. Newcomb, of the Washington Observatory, detailing the Russian preparations for observing the forthcoming transit of Venus, and printed in *Harper's Weekly*, he remarks that the inquiries into the meteorological conditions of the stations selected have given, on the whole, very satisfactory results, particularly for the station on the coast of the Pacific Ocean and in Eastern Siberia (85 per cent. of clear sky for December). In two only of the stations chosen, Taschkent and Astrabad, these conditions are not sufficiently satisfactory. For this reason the observers designed for Taschkent will probably go to a place about 100 miles west of that town; and instead of Astrabad it is proposed to take either the island of Aschuradeh, in the Caspian Sea, or, if possible, to cross the Elburz Mountains, and establish observers at Schahrech, in Persia (with nearly absolute certainty of clear sky). The total number of Russian stations will be twenty-four, each of them provided with only one instrument for the transit observation. These instruments are—three 4-inch heliometers, three photo-heliographs, four 6-inch equatorials, and four 4-inch equatorials, provided with filar micrometers and spectroscopic apparatus, and ten 4-inch telescopes, designed merely for contact observations. Each station will also be furnished with clocks, chronometers, and the instruments necessary for exact determination of time. The principal instruments have already been ordered. Most of them will be ready for use in the course of the present or beginning of next year. For these instruments the observers are also in a great part already selected. They will all visit Palkowa for a certain time in 1873 to exercise themselves in the observations. The geographical positions of the stations will not be determined by the transit observers; but all stations on which the transit has been successfully observed will be carefully determined afterwards by special expeditions of the general staff or the navy. For this purpose a principal line of telegraphic longitudes will probably be laid next year through all Siberia to Nicolajevsk, with which line the other stations of that part of Russia can easily be joined, either by telegraphic or chronometric operations. With regard to photographic observations, Prof. Struve states that two observers, one at Vilna, and Dr. Vogel at Bothkamp, in Holstein, have been perfectly successful in taking instantaneous observations with dry plates.

THE Cambridge Natural Science Club has just issued its first terminal report. The club was founded March 11, 1872, by some of the undergraduates and B.A. members of the university for (Rule 1) "The promotion of natural science by means of friendly intercourse and mutual instruction." The number of members was at first limited to nine, but at the third meeting the number increased to twelve. The number is limited in order that the meetings may be held in the rooms of the members. Meetings are held every Saturday evening during term, and during eight weeks of the long vacation, in the rooms of the members in rotation. The member in whose rooms the meeting is held is president for the evening, and (Rule 7) "brings some subject of scientific interest under the consideration of the members in the form of a short paper, with such practical illustrations as the subject admits of." The papers read during this term have been the following:—By Mr. J. C. Saunders, B.A. (Downing College), "On Conspicuous Movements in Plants;" Mr. C. T. Whitwell, B.A., B.Sc. (London), Trinity, "On Isothermals;" Mr. C. J. F. Yule (St. John's), "On the Anatomy of Pyrosoma;" Mr. H. M. Martin, M.B., B.Sc. (London) Christ's, "On the Modes of Reproduction of Animals and Plants;" Mr. A. Liversedge (Christ's) "On Supersaturated Saline Solutions;" Mr. J. E. H. Gordon (Caius), "On Submarine Telegraphy;" Mr. P. H. Carpenter (Trinity), "On the History of the Abbeville Jaw."

A BOTANICAL and geological excursion class has been commenced in connection with the St. Thomas's Church Schools Birmingham. President, Rev. T. D. Halstead, rector; secretary, Mr. Miller (brother of the late celebrated chemist); conductor, Mr. John Turner, F.M.S. The class consists of twenty-five members, most of whom have attended science classes during the winter, and recently competed in the examinations of the Science and Art Department. The class meets every Saturday afternoon.

WE learn from the *British Medical Journal* that a committee, consisting of Profs. Bamberger, Billroth, Brücke, Duchek, and Schroff, has been appointed to select candidates to be invited to fill the chair of Pathological Chemistry at the University of Vienna. The names of Hoppe-Seyler (lately appointed to the University of Strasburg), Kuhne (of Heidelberg), and Liebreich (of Berlin), are mentioned in connection with the post. The establishment of a professorship of Hygiene in the University is under discussion; and there appears to be a general agreement among the professors in the University as to the necessity of having such a chair instituted with as little delay as possible.

A FAIRLY perfect human skeleton has been discovered by Dr. Revière in the caverns of Baoussé-Roussé, near Menton, on March 26, and a short paper on this interesting find was read to the Academy of Sciences at a recent sitting.

MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY*

AS a general summary of the current zoological literature the "Zoological Record" maintains its high value. The volume for 1870 has lately appeared under the new editorship of Mr. Newton, and the arrangements now made for its further prosecution are very hopeful; yet I must again urge upon all our Fellows, who, as amateur zoologists or patrons of the science, have joined our ranks, to give their further support to the "Zoological Record" Association in order to secure the continuance of this annual summary for the sake of the working members, to whom it is so essential. I would also call attention to the sketch of the ornithological works recently published or in progress contained in the last number of the *Ibis*, an example which it were to be wished were regularly followed in all periodicals specially devoted to any branch of our sciences. The Reports on the contributions to the various branches of zoology inserted in Wiegmann's "Archiv" under the editorship of, and some of them compiled by, Troschel, replace in some measure the "Zoological Record" for the German public, and are kept up nearly to the same period, some of the reports for 1870 having already appeared; they are also much to be commended, although they may not have quite the method and completeness of the "Zoological Record." I have further to congratulate science in general on the near completion of the Royal Society's great Catalogue of Scientific Papers, the sixth and last volume of which is far advanced, and likely to be in our hands by the commencement of the next session of the Society.

In Botany, Pritzel's excellent and much-improved second edition of his "Thesaurus" is rapidly going through the press, and brings the repertory of separate botanical works down to the year 1871. Current botanical publications are also generally noticed in various botanical publications, especially the "Giornale Botanico Italiano," edited by Prof. Caruel; the "Flora" of Ratisbon; the "Botanische Zeitung," continued since the death of v. Mohl by A. de Bary; the "Bulletin de la Société Botanique de France," which comprises perhaps the fullest bibliographical review; and the *Journal of Botany*, which promises well under the new and active editorship of Dr. Trimen. But, with the exception of lichenography, the bibliography of which is brought down to the year 1870 in Kreppelhuber's detailed "History and Literature of Lichenology," we have no comprehensive references to Memoirs and Papers published since 1863, the term of the Royal Society's Catalogue, and we feel much the want of an annual summary corresponding to the "Zoological Record."

A work has recently appeared which has naturally attracted much of my attention as being intimately connected with a branch of the science which I have on several occasions taken as the subject of my annual Addresses, and as being the result of long and careful study of the great and varied mass of data collected by its laborious and distinguished author. I speak of Grisebach's "Vegetation of the Earth according to its climatological distribution." The general scope and plan of the work has been recently noticed in an article in NATURE,† and I shall on the present occasion confine myself to a few observations on his views with reference to some of those regions or districts to which I had intended to call your attention in my last year's Address.

One of the most interesting of these regions is the Japanese, or the greater part of Grisebach's Chino-Japanese regions, that is, the Japanese islands and opposite coasts of the Asiatic continent. The peculiarities of its flora have been accounted for, upon considerations depending chiefly on origin, in a well-known paper by Asa Gray (Mem. Amer. Acad. new ser. vol. vi. p. 424), whose views are fully coincided in by Maximowicz and others, but strongly objected to formerly by Miquel and now by Grisebach, who relies upon climatological and other physical considerations. It appears to me that this is a strong instance of the combined effects of the two agents as explained in my above-mentioned Address of 1869 (p. 15; Proc. Linn. Soc. 1868-69, p. lxxvii.) The main features of this flora are the mutual intergrafting of northern and tropical types, and the number of highly differentiated endemic or widely dissevered monotypic or almost monotypic races, the former due to physical, the latter to derivative causes.

With regard to the endemic or widely dissevered highly differentiated races (monotypic genera, sections, or very distinct species), Grisebach's views differ widely from those of Asa Gray and other modern naturalists who adopt more or less the theory of evolution. Grisebach, as already observed, entirely ignores community of origin of closely allied or representative species, and is but little disposed to take into consideration ancient dispersion under geological conditions different from the present ones. Each species he believes has arisen—he had formerly said been created, an expression he now abandons in order not to be supposed to prejudge a question which admits of no positive solution—each species has arisen in a particular spot (from what materials he thinks it vain to inquire), under the influence of physical and other external conditions, and has spread more or less in every direction from this birthplace or centre as far as those external conditions have prevailed, and in so far as their progress has been unopposed by insurmountable physical or climatological barriers. In conformity with these views he explains closely allied and representative species in a passage which I give at length for fear of misrepresenting him by an abstract. "The birthplace (*Entstehungsort*) of a plant species," he says, vol. i p. 515, "may be taken as the most perfect expression of the concordance between the physical life-conditions of the place and the organisation of the plant; for this suitability to given influences of inorganic nature gives the highest measure of the capability of preservation which life strives to attain. Upon these propositions is founded the conclusion, that the nearer the centres of different plants are placed geographically, and the less different are therefore their climatological conditions, the more similar must be their organisation, or, what amounts to the same thing, the more species will have arisen in the same genus. This phenomenon is exhibited in all places where we can compare endemic species whose dispersion is limited, but in islands which have a peculiar vegetation it is less pronounced than in continents. From any one point climate is gradually altered, like the radii of a circle which gradually diverge more and more from each other from the centre to the circumference. In a continent the whole area of the circle may be supposed to be suited to the production of changes in organisation; in an archipelago it is interrupted by the sea, and here, therefore, few similar species have arisen. Another consideration to be taken into account is that genera when compared with each other are unequally susceptible of change (*veranderungsfahig*); their species, therefore, to keep to the same metaphor, will be found arranged at greater or less distances from each other in the radii of the circle. If the area of the continuous land is small, monotypes will have more readily arisen—genera which, on the one hand, are very little or not at all susceptible of change, and on the other hand, can no longer subsist with a certain degree of climatological change. If in a more remote geographical distance the more important climatological conditions which these genera require are repeated, we may, perhaps, find in another part of the globe a second species; and this generally explains the origin of the species which have been termed representative (*vikarirende Arten*). A precisely similar climate, however (exactly the same complication of the very varied phenomenon towards which organisms bear themselves receptively), is never repeated in two distant points of the earth's surface; and this may be taken as the foundation of the absolute unity of centres of vegetation, that is to say, of the proposition that every species in its wanderings has issued from a single birthplace, which does not exclude the possibility of solitary exceptions which might be imagined in plants of less receptivity."

In all this it appears to me that if the writer refuses to admit of a descent from a common parent, we have a right to ask of him what is the previous organisation upon which he imagines climate to have worked to produce allied species in one region and representative species in distant regions? what are the previous genera which have changed? for upon that seems to hinge the whole of his argument in refutation of Asa Gray's hypothesis explanatory of the original connection between the East Asiatic and East American floras. That every species had arisen in one spot, whether by differentiation or by creation, appears now to be tacitly admitted by all. Asa Gray, in accordance with Darwinian theories, supposes widely spread species to have been, under the different conditions of distant lands, gradually modified in different directions, so as to have produced distinct varieties or representative species; Grisebach supposes these different conditions to have independently produced distinct but similar species, by acting on organisms which had not been one and the same species; but what else they may have been he seems to think beyond the reach of plausible conjecture.

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* Delivered Friday, May 24, and abridged.

† No. 128, April 11, 1872.

Leaving, however, these questions of origin aside, he strongly objects to the classing representative with identical species in considering geographical distribution; for the former appear in such absolutely dissevered distant regions that an interchange of species, even in early geological periods, seems impossible, as, for instance, in the case of several Ericas of the Cape and of Europe. It is on the contrary, he believes, almost always possible to deduce the actual progress of identical species from the form or physical accidents of their homes and from the means of dispersion at their command (p. 519). He therefore, in combating Asa Gray's conclusion, commences by eliminating from his calculations, after the example of Miquel ("Over de Verwantschap der Flora van Japan met Azie en Noord America, Versl. K. Akad." Amsterdam, ser. 2, ii.), all representative species, thus reducing Asa Gray's list of concordant races in Japan and Eastern North America from 226 to 81, from these Grisebach subtracts 41, which are also inhabitants of Western North America, and can still, he thinks, daily transmit their seeds across the Pacific Ocean; 17 more are, in his opinion, supported by that of other botanists, either certainly not identical or doubtful, and to be added to the already eliminated representative species. Of the remaining 23, he finds 21 which can bear a high northern climate and may yet be found in the Oregon or other imperfectly explored territories of North-West America; and the whole long list is thus reduced to two species only, whose problematical disseverance in Japan and Eastern North America remains unexplained—the one, *Elodea petiolata*, being a marsh plant, which as such possesses great migratory powers, the other, *Carex rostrata*, from the White Mountains, awaits further researches on its geographical distribution. Even admitting the possibility of the greater early dispersion of these species in former geological periods propounded by Asa Gray, Grisebach thinks that any such great antiquity of the Japanese flora is not established on so firm a ground as to supersede any attempts at finding other explanations limited to the results of forces still in activity in present times, and that accordingly the distribution of the species in question may be satisfactorily accounted for by the means of dispersion still available, if the data are viewed in the light he has placed them. I should doubt, however, whether his mode of cutting up a long array of ascertained facts further increased by subsequent researches, in order to make them agree with preconceived theories, will carry any stronger conviction into Asa Gray's mind than in my own, more especially as the presumed great antiquity of the Japanese flora is not deduced from these facts alone, but is derived also from other evidences, amongst which the peculiar character of the endemic monotypes bears a prominent part.

With regard to Grisebach's idea that representative and similar species are independently produced by similarity of climatological conditions, and that they afford no conclusive evidence of community of origin, for that they are to be found in widely dissevered localities between which it is impossible to conceive any continuity even in ancient geological periods, and with reference to the instance he adduces of the above-mentioned heaths of the Cape and of Western Europe, I would recall to your minds some remarks I made in my Address of 1869 (p. 25; "Proceedings," p. lxxxvii.) on the remarkable coincidence of several genera, and the near similarity of some species that exists between these two widely dissevered regions. I would now add that if it is difficult to imagine any ancient continuity which should readily explain this phenomenon, it seems equally difficult to account for it by any climatological similarity, if we consider how much Cape plants in general, accustomed to a prolonged summer's sun, suffer from its want in the dull damp seasons of Western Europe.

The Eastern Archipelago, the study of whose fauna, as connected with the history of the great changes it has undergone by successive submersions and upheavals, has been rendered so interesting by the well-known labours of Mr. A. R. Wallace, calls imperatively on the attention of botanists in the search of facts derived from its flora in confirmation or refutation of these views. Unfortunately we are in this respect very much in arrear. The botany of New Guinea is almost wholly unknown; and from Celebes we have but very little. Sumatra, Java, the Philippines, Timor, and a part of Borneo, have been more generally explored, and large collections of their plants have been deposited, chiefly in the Leyden Herbarium, but also in considerable numbers in those of Kew and in some others; but even these materials have been but little worked up in a manner to be available for the geographical botanist. The two eminent Dutch botanists who had successively charge of the Leyden collections contributed much in various ways to the progress of the science and especially to our knowledge of the flora of the principal Dutch islands, but without leaving any satisfactory general view of all that was known on that of the whole archipelago. Blume's "Bijdragen tot de Flora van Nederlandsch Indie," drawn and published at Batavia when he was still very young, was a wonderful work considering the means at his disposal; and after his return to Europe he commenced elucidating with equal ability and in greater detail several orders connected with that flora ("Flora Javæ," "Rumphia," "Museum Lugduno-Batavense"); but as general works all these remained incomplete. Miquel drew up a "Flora Indiæ Bataviæ," purposing to be complete as far as his materials allowed; but it was far too hastily compiled, without the necessary critical examination of genera and species. Since his lamented death I have seen no signs of any Dutch successor likely to take up the study of the botany of the archipelago in any scientific point of view. In the meantime the rich stores collected by P. Beccari in Sarawak are, I am informed, in the course of distribution; and that enterprising Italian naturalist has returned to the East with a view to the exploration of New Guinea and some others of the less known islands.

Grisebach, in his Indian Monsoon region, unites the archipelago with the East Indian peninsulas and continent to the foot of the Himalayas, the Island of Ceylon to the west, and the Society and the Marquesas and other coral islands to the east, embracing, as it were, the whole of Tropical Asia or Sclater's Indian, with a portion of his Australian Palæotropical regions; and certainly a cursory survey of the vegetation of this vast expanse of territory would appear to justify Grisebach's idea of its unity of character. It has also tolerably definite limits determined on the north-west by the drier rocky East Mediterranean or Persian region, on the north by the great Himalayan chain, and on the east and south by a wide extent of ocean; the exceptions being chiefly the above-mentioned innoculation, as it were, into the Japanese flora to the north-east, and more or less of an intrusion across the ocean to the westward into Tropical Africa, and over a narrower interval of sea to the south-east into north-east Australia. The principal cause of this uniformity of character, so far as it goes, is well deduced by Grisebach from climatological and physical conditions, his observations on the chief portion of the region, or East India proper, from Ceylon and the Peninsula to Malacca, being mainly derived from Hooker and Thomson's most instructive introduction to their "Flora Indica," which, from a variety of causes, was unfortunately put a stop to after the issue of the first volume. It is now being replaced by the "Flora of British India," under Dr. Hooker's editorship, of which the first part, just published in a more concise form, gives a confident hope that it may be steadily and rapidly brought to a conclusion. We shall then have ample means of instituting a comparison of the Indian vegetation with that of Boissier's "Flora Orientalis" to the north-west, of Ledebour's "Flora Rossica" to the north, of Miquel's almost as complete, though less methodical enumerations of Japanese plants to the north-east, of the "Flora Australiensis" to the south, and of Oliver's "Tropical African Flora" to the west.

The "Flora Indica" does not, however, extend to the eastern portion of Grisebach's Monsoon region, about which our information is so deficient; but where, as he observes, "the distribution of organisms involves one of the most remarkable problems in the darker regions of vegetation-centres." He further remarks that the flora of this eastern region, with the exception of the Timor group, is everywhere Indian, and regulated by climatological conditions, the vegetation of New Guinea being, as he rather hastily supposes, "thoroughly similar to that of Borneo," a result quite at variance with the distribution of animals as expounded by Wallace. As a possible explanation of this discrepancy, he proposes an hypothesis which, for fear of misrepresentation, I shall give at length:—"Thus the limits of particular forms of plants and of animals in the Indian Archipelago do not concur. Vegetation corresponds to climatological, the fauna to local (*raumliche*) analogies. This opens a wide field for speculation on the history of the globe. By a mere sinking of the land to an unimportant extent, Darwinism readily explains the origin of the fauna of these islands, but not the Indian character of the flora of New Guinea, which presupposes much greater upheavals than the origin of fauna, calculated to give rise to equatorial rainy seasons. This hypothesis would derive the endemic marsupials of New Guinea from the Australian ones after the establishment of the Torres Straits, but it gives no explanation of the way in which the peculiar palms of New Guinea could have arisen from allied Indian genera. With more plausibility, although with little more foundation on ascertained facts, may be put forward another conjecture derived from the respective relations of plants and animals to the outer world. From their organisation the former are much more dependent on climate, the latter on the vegetation which serves them for food. If an extent of sea is converted into land, its climate (independently of its geographical position) will depend on the form of its coasts and on the relief of its surface. If, now, creative forces are pronounced, the forms of vegetation will be suited to the climate. These forms correspond to the climate of the present day, as everywhere else, so also from the Malayan continent to the South-Sea Islands. If we assume that in an earlier geological period the eastern portion of the Archipelago did not yet possess its mountains, and was connected with Australia, so might the Australian climate have then extended to the Archipelago; but with the change in the climate the vegetation of the time must have disappeared. A new flora arose; but in the fauna, which was less dependent on climate, the earlier types may have longer persisted. Perhaps the present period may be regarded as one in which the Australian forms of animals are in an expiring state, because the jungle-forests do not sufficiently correspond to their demands for food. It would appear as if creative activity only wakes up at specific points of time on specific points of the earth's surface, and that during the long pauses Nature's struggles are directed only to the retaining that which exists. Vegetation, as well as the animals which it feeds, must ever be considered in relation to the geological developments. During the time which has elapsed since the mountains and the moist climate of New Guinea have been established no new creation of Mammalia has taken place. Only very few Marsupials, and scarcely any other Mammalia, have been found on this great island. But in other classes of animals forms have arisen corresponding to the present vegetation, such as the Birds of Paradise, which are unknown in Australia, but which in New Guinea hover over the forest tree-tops, whilst they can take shelter from the mid-day sun under the dense foliage. . . . The present type of organisation was already cast in New Holland in the tertiary period, whilst the endemic plants and animals of New Guinea appear to be of much later origin" (vol. ii. pp. 69, 70).

Without admitting to its fullest extent the main fact relied upon, that there is no marked line separating the vegetation of the western and the eastern portions of the Archipelago corresponding to that laid down by Wallace for animals, a premature conclusion in the present state of our knowledge,* and still less entering into speculations as to the intermittent action of creative forces which I do not quite comprehend, we must agree with Grisebach that, so far as shown by the scanty data at our command, the uniformity is much greater in the botany than in the zoology of the whole Archipelago. We may also admit with him that this comparative uniformity may be, in great measure, due to the uniformity of climate acting more upon plants than upon animals. But there are other circumstances which may probably have favoured the continued action of natural selection through countless ages in procuring this result. Dr. Hooker has very plausibly suggested a greater geological antiquity in the plant races than in those of animals, especially the higher animals, under which the former, or the ancestors from which they are descended, had become established over a wide extent of continuous land before its successive disruption, upheavals, and depressions producing the present isolation. We must next take into account that this continuity of land need not be so great in the case of plants as of animals. The dispersion of the former is passive, and takes place chiefly in a dormant state, in which minuteness and enormous multiplication afford them opportunities for crossing seas and other barriers denied to the higher animals. Plant races of accommodating (*accomodationsfahiger*) constitutions, as they successively arose and attained the full vigour of specific life, will have early spread over any continuous or but little broken area, enjoying comparatively similar physical and climatological conditions, the western and eastern forms intermingling, so as that the one should only gradually be replaced by the other, thus in early ages repeating under the tropics the phenomenon now observed in the northern temperate Europeo-Asiatic region. These vigorous or accommodating races, whether new differentiations or foreign invasions, will at the same time have gradually expelled and replaced races which in tertiary or other previous periods had occupied the land under different conditions, and which now could only maintain themselves in the struggle for life in localities affording them, in their reduced or weakened state, special protection against the effects of the altered climate and the attacks of their vigorous competitors. Such localities, suited to ancient or expiring races of few individuals, with varied but always special requirements, and generally slow of propagation, may be exemplified in the Mediterranean, the Japanese, and other regions abounding, as Grisebach terms it, in centres of vegetation; they may be faintly traced in the Nilgherries and in Ceylon; but are in general very few in Grisebach's Monsoon region, and those few are as yet but little known or wholly unvisited. Kini-Balu, in Borneo, has, however, as we learn from Dr. Hooker, supplied a place of refuge for a certain number of Australian types, and it may be conjectured that many more may have maintained themselves in those lofty mountains of New Guinea which have as yet been only seen from a distance. Continuity of vegetation probably existed in tertiary times between Australia and a vast extent of land including more or less of both of Wallace's divisions of the Archipelago. How far subsequent changes which have influenced the present distribution of animals may have affected that of the forest vegetation, can only be judged of when the floras of Borneo, Celebes, and New Guinea shall have been as well investigated and compared as have been those of Sumatra and Java.

(*To be continued*)

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* Dr. Hooker has, for instance, remarked that no Dipterocarpeæ have been found to the east of Borneo.

SCIENTIFIC SERIALS

In the *Journal of Botany* for May the editor, Dr. H. Trimen, describes a species of *Luzula* new to the Flora of Europe, *L. purpurea* Link, of which a drawing is given.—Mr. J. G. Baker concludes his revision of the Cape species of *Anthericum*.—Mr. Archer Briggs notices some peculiarities of the botany of the neighbourhood of Plymouth, principally with reference to species common elsewhere which are absent from the south-western extremity of our island.—Mr. O'Meara continues his researches on Diatomaceæ.

The number for the present month opens with a note on Dimorphism in *Eranthemum* by Mr. John Scott.—The other original articles are Notes on British Gentianaceæ by Mr. James Britten, and Supplementary Notes on the *Erysiphei* of the United States by Messrs. Cooke and Peck. Several valuable reprints, as well as many interesting short notes and queries, also appear in both these numbers.

The *Canadian Naturalist*, vol. vi. No. 3, is almost wholly devoted to Geology, commencing with a continuation of Principal Dawson's series of papers on the Post-pliocene Geology of Canada, the portion of the subject specially treated of in this number being the local details.—Prof. Sterry Hunt's "History of the names Cambrian and Silurian in Geology" has already been reprinted in our columns.—Mr. E. Billings contributes some "Remarks on the Taconic Controversy," in which he defends the views of Dr. Emmons with regard to the position of these rocks, and a note on the genus *Obolellina*.—The only new geological article is Prof. Smallwood's "Meteorological Results for Montreal for the year 1871.

SOCIETIES AND ACADEMIES

London

Royal Society, May 30.—"On the Structure and Function of the Rods of the Cochlea in Man and other Mammals." By Urban Pritchard, M.D.

The aim of this paper is to describe the true construction and use of the cochlea, so far as its task of distinguishing the various sounds is concerned. This cochlea, it must be borne in mind, consists of a spiral canal, in form and shape very similar to the inside of a snail-shell. From the axis of this spiral there proceeds horizontally a plate of bone, the *lamina spiralis*, almost dividing this canal into two. From this plate again there extend two membranes, the membrane of Russner, and the Lamina spiralis membranacea, as far as the walls of the canal, thus separating it into three minor canals.

Between the layers of the membranous spiral lamina are situated the so-called rods of Corti. These were first discovered and described by the Marquis de Corti; and although since then many observers have studied the subject, yet scarcely two investigators are agreed as to their exact form.

In a general view of the rods from above, they appear similar to two rows of pianoforte-hammers, rather than like the keys of that instrument, to which they have been likened. In a lateral view, these two rows of rods are seen sloping towards each other, like the rafters of a gabled roof. The rods consist of a shaft and two enlarged extremities, but the two rows differ considerably in form; the inner rods are attached by their lower extremities to the membrana basilaris at its junction with the lower lip of the limbus, and just external to the spot where the nerve-filaments emerge. They are directed outwards and upwards, with a slight undulation to meet the outer rods. The lower extremity is enlarged and rounded, gradually tapering to the shaft, which is cylindrical; the upper extremity is somewhat cuboid in form, but the outer surface is deeply concave, and the upper lip of the concavity is prolonged into a process.

The outer rods are attached to the membrana basilaris by a broad base, which also gradually tapers to a cylindrical shaft. Their upper extremity is less cuboid in form, and presents a convex internal surface, which articulates with the corresponding concavity in the inner rods just mentioned; from the outer and upper part there extends outwards a slender process.

One of the most important features with regard to these rods is their relative length. Most authors state that there is very little difference in the length of the two rods; in this, however, they are much mistaken; for not only do the two sets of rods differ in this respect, but the length of each varies according to its position on the cochlea. Thus, at the base, the outer rods are as nearly as possible equal in length to the inner, but proceeding upwards, both rows increase in length with great regularity, although not in the same ratio, the outer increasing with much greater rapidity, so that near the apex they are twice the length of the inner.

It was generally supposed, *a priori*, that these rods were graduated so as to distinguish the most minute variation of tone, but no one until now has been able to demonstrate this.

The rods, therefore, vary in length from about $\frac{1}{500}$ to $\frac{1}{200}$ of an inch. The number of rods in each row is not the same, there being about three of the inner to two of the outer, and, according to calculation, there are about 5,200 inner rods and 3,500 outer, in the whole cochlea.

Corti and most other authors considered this system of rods to be the essential portion of the cochlea; they supposed the rods received the vibrations conducted to them, and being set in motion, so affected the nerves as to cause the brain to appreciate the various sounds. Later German writers have attributed the appreciation of the various vibrations to certain delicate cells, which are attached to the under surface of the membrana reticularis. From this circumstance alone it appears very evident that these investigators had not suspected, much less discovered, the fact that the rods are most exquisitely graduated, for otherwise they could surely never have doubted that so beautiful and suitable an apparatus could have any other ostensible purpose than that of appreciating the various sounds. I consider indeed that the cochlea represents a musical instrument, similar in nature to a harp or musical box, the strings of the one and the tooth of the other represented by the rods of Corti. The spiral bony lamina is simply a sounding-board; around the rods are placed the various nerve-cells and nerve-fibres, and from these cells the impressions are conveyed by the fibres to the brain itself.

It is possible, therefore, to trace very completely the course of sounds or vibrations from a musical instrument or any other source to the brain, through the medium of the ear. First the vibrations are caught and collected by the auricle, and transmitted through the external meatus to the drum of the ear, next across the middle to the internal ear. Here the sound is appreciated, merely as a sound, by the vestibule; the direction is discovered by means of the semicircular canals; but to distinguish the note of the sound, it must pass on to the cochlea. The vibration therefore passes through the fluid of the cochlea and strikes the lamina spiralis, which intensifies and transmits the vibration to the system of rods. There is doubtless a rod not only for each tone or semitone, but even for much more minute sub-divisions of the same; so that every sound causes its own particular rod to vibrate, and this rod vibrating, causes the nerve-cells in connection with it, to send a nerve-current to the brain.

"Examination of the Gases occluded in Meteoric Iron from Augusta Co., Virginia." By J. W. Mallet, Ph.D., M.D.

The author stated that, whether or not his analysis be considered as furnishing presumptive evidence of the Virginia iron having come to our earth from a different atmosphere to that of which the Lenarto meteorite brought us a sample, the result differs so far from that of our sole previously recorded determination of the kind as to make it a matter of much interest that a larger number of meteoric irons from various localities should be subjected to careful examination in the same direction, thus supplementing our knowledge of the fixed constituents of these curious bodies by a study of their gaseous contents.

Anthropological Institute, June 3.—Sir John Lubbock, Bart., president, in the chair. "The artificial enlargement of the Ear-lobe in the East," by J. Park Harrison; "On Tumuli at Sapolia, Ardaschevo, Russia," by Baron de Boguschefsky; "On Ogham Pillar Stones in Ireland," by Hodder M. Westropp; and "The Westerly Drifting of Nomades from the 5th to the 19th century, Part 9: the Fins and some of their allies," by H. H. Howorth. The object of the paper by Mr. Howorth was, in the first place, to discriminate between the Fins and the Lapps, whose history, physical features, customs, and other idiosyncracies are entirely different. In the second place, to show that the Esthonians belong to the Fin rather than the Lapp stock. Then to adduce the evidence for making both Fins and Esths very recent emigrants into their respective modern habitats, and to trace them to their former country beyond the Dwina, where they were known to the Norsemen as Biarmians, and to the early Russian chroniclers as Sarvalokian Ichudes. Having divested Scandinavia and Esthonia of their Fin inhabitants, and having thrust them back into an area which was of great renown in the times of the Norsemen, we can explain how the civilisation, which the Kalevala and other evidence proves, was once peculiar to the Fins, has been lost, and also explain whence the Norsemen derived a great portion of the culture which distinguished them. The main position that was new in the paper was the deriving the Esthonians from the same area as the old Fins, and making them also to be recent emigrants, and not Autochthones, as they have been so frequently described; and the clearing up of the ethnography of the old province of Biarmia, which has hitherto been much confused.

Victoria Institute, June 3.—The Rev. Prebendary Irons read a paper on Prof. Tyndall's "Fragments of Science for Unscientific People." He first dealt with physical science and its rivalries, Dr. Irons holding that there was a want of thoroughness in Prof. Tyndall's appeal to facts. He then analysed the statements made as to the action of matter on matter, and considered Dr. Tyndall to be inconsistent in stating that science could not solve the problem of the Universe, and yet adding that we ought not to see the evidences of Divine pleasure or displeasure in the phenomena of the material world. Finally, Dr. Irons urged that science and true religion could not be supposed as opposed to each other, as some men of science would have us believe.

CAMBRIDGE

Philosophical Society, May 27.—"On some properties of Bernoulli's numbers, and, in particular, on Clausen's Theorem respecting the fractional parts of those numbers," by Prof. J. C. Adams. The author gave a comparatively simple proof of Clausen's theorem. Thirty-one of Bernoulli's numbers are already known; the author has calculated twenty-two additional numbers. He also had proved that if n were a prime number other than 2 or 3, the numerator of the n^{th} Bernoulli's number was divisible by n.—"On some of the Symptoms produced by Uræmic Poisoning in Chronic Disease of the Kidney," by Dr. Latham. These symptoms were explained by:—1. The impeded passage of the blood through the minute arteries of the system, caused by excessive contraction and hypertrophy of the muscular walls of these vessels, as has been demonstrated by Dr. George Johnson. 2. The hypertrophy of the heart, developed by the resistance offered to the circulation from the contraction of these small arteries. 3. The impoverished state of the blood, which is the necessary accompaniment of the disease.

CANTERBURY

East Kent Natural History Society, May 2.—"Remarkable objects found in the Suffolk Crag, and simulating human workmanship." Some time since the president had received from the Rev. W. Bird some perforated shark's teeth; but it was only an hour or two before the meeting that a box arrived containing a further collection, including many remarkable fossils from the red clay diggings of that district, the whole being specimens of the admirable series of such objects in the possession of Mr. Edward Charlesworth, F.G.S. The teeth were described as belonging to the genera *Otodus* and *Carcharodon*, and each of these teeth had a hole near its base, about a sixth of an inch in diameter, like in form and position to the holes which the South Sea Islanders make in the teeth of sharks at the present day in order to the formation of necklace ornaments. Of course, should the perforations in the teeth from the Suffolk Crag prove to have been the work of man, it would suggest that he had existed on our planet an immense time before that at present fixed as his original appearance here. But though these holes are such as might have been, and most probably were, made by human agency; they might, on the other hand, have been the work of some boring sponge, worm, or mollusc, especially as there are in this last class many species with a curious file of lingual teeth composed of silex; and even at the present day there is a complete mystery as to the means by which some invertebrates bore into and through very refractory substances. However, the whole evidence as to these holes in the shark's teeth preponderates in favour of the view that they were made by man. But even fully admitting that they were so made, it would not necessarily follow that the perforations in the teeth were made by man coeval with the crag in which they were found.

PHILADELPHIA

American Philosophical Society, February 16.—A memorial to Congress was adopted, praying for an appropriation in aid of astronomical expeditions, especially for one to the Antarctic region, for the purpose of observing properly the approaching transit of Venus.—Prof. P. E. Chase read a paper "On Correlations of Cosmical and Molecular Force." From the hypothesis that the entire energies of opposing attractive and repulsive forces may be considered as concentrated in one of the foci of the resulting oscillations, he deduced various interesting approximations to the ratio between the respective amounts of heat required for equivalent work under constant volume and under constant pressure, to the change of specific gravity in the conversion of $H_2 + O$ into H_2O, to the period of terrestrial rotation, and to the solar and lunar masses. Some idea of his method may be formed from the following approximation to the sun's mass and distance. According to the mean result of experiments by Dulong, Hess, Andrews, and Favre and Silbermann, one pound of H burned with eight pounds of O liberates enough heat to lift the nine pounds H_2O vapour, *in vacuo*, $\frac{34533 + 772}{9}$ feet. Such a lift would establish an oscillation, which would be perpetually sustained, by terrestrial attraction, and elastic rebound, unless otherwise counteracted. If chemicals vary as gravitating energies, the mean height of the oscillating vapour . mean height of oscillating earth :: earth mass : sun's mass. Therefore, if m = mass of sun ÷ by mass of earth, d = distance of sun ÷ earth's radius, r = earth's equatorial radius in feet, h = mean height of oscillating vapour, $T_,$ = solar year in seconds, T_0 = time of satellite revolution at the surface of the earth.

$$m = \frac{dr}{h}$$

$$T_0 = T_1\left(\frac{d^3}{m}\right)^{\frac{1}{2}} = T\left(\frac{hd^2}{r}\right)^{\frac{1}{2}}$$

Hence we readily obtain the values

$$d = 233{,}772 = 92{,}639{,}500 \text{ miles}$$
$$m = 330{,}260.$$

—Benjamin Smith Lyman read a paper "On the Topography of the Punjaub Oil Region." It aimed at a somewhat detailed account of the topography of the Punjaub Oil Region: its situation, general features, special features, &c. The different places are mentioned where each kind of topography is to be seen, and its causes and simple laws pointed out, chiefly in order to show the great usefulness of careful topographical studies to geology. A short sketch of the geology of the region, apart from structure, is also added; as to the oil, from the writer's own "General Report on the Punjaub Oil Lands, Lahore, 1870," and as to other points from older works. The general section of the rocks of the region is as follows, below the new and old alluviums:—

Miocene (Sivalik), perhaps ...	...	3,000	feet.
Eocene (Nummulitic), with oil	...	1,950	,,
Mesozoic, perhaps	...	700	,,
Carboniferous, without oil, about	..	1,800	,,
Devonian, with salt and plaster	...	2,850	,,
		10,300	,,

The oil or asphalt (dried oil), or rock tar (melted asphalt), is found at a dozen different places, and, in very small traces, at half a dozen more, all within a space of a hundred miles square. They are all in Nummulitic rocks, except one in Carboniferous. The deposits all seem of very small horizontal extent—sometimes only a few feet, seldom a hundred yards, once only as much as half a mile. In this case, too, the oil-bearing bed is a hundred feet thick, in one other forty, in two others twenty, and in the rest much less. The oil comes in some places from lime rock, in others from sand rock, or shales. The yield of one well was at first fifty gallons a day, but grew quickly less, like the ordinates of a parabola, and seems likely to reach 3,000 gallons in all within a year and a half. At a rough guess, a hundred such wells might be bored in the region, with a whole yield, then, of hardly 7,000 barrels. The natural springs (five) yield from a gill to three quarts a day. The oil is dark green and very heavy (25° B. or less). There is nothing whatever in the Punjaub oil deposits to bear out a belief in the distillation of oil from one bed to another, or in its emanation from below, or in its gradual passage from the lower parts of a bed to higher parts of the same bed, or in its origin from any source but the decomposition of organic matter in the rocks. Neither is there anything here (or anywhere else) to justify wild hopes of finding large quantities of oil by boring into cavities below the oil-bearing bed. The occurrence of salt, gypsum, and alum shales in large quantities is noticed, as well as that of sulphur, saltpetre, brown coal, and good in small quantities, and that of traces of copper, iron, and lead.—Prof. E. D. Cope read a paper on *Bathmodon*, a genus of extinct Ungulates. It was presented as Perissodactyl in general characters, but with peculiarities of dentition of a combined ruminant and suelline character. There was on the outer side of the molars but one crescent, and before this a tubercle. The inner portion of the crown a ledge. Besides the species *Bathmodon radians*, a second form, *Loxolophodon semicinctus* was referred to the group. The former animal was large as the rhinoceros, the second equal to the tapir.—A memoir on the "Geology of Western Virginia" was presented by Mr J. J. Stevenson.

March 1.—Mr. B. Smith Lyman presented for publication a topographical map of West Virginia.—Prof. Cope read a paper on two new species of Ornithosaurians from the Kansas Cretaceous. They were described as *Ornithochirus umbrosus* and *O. harpyia*. The former was regarded as one of the most gigantic of the pterodactyles, extending probably 25ft. from tip to tip of the wings The other was two-thirds the size.—Prof. Cope read a paper on *Protostega*, a genus of extinct Testudinata. A detailed account of the Osteology of *P. gigas* from the Cretaceous was given, by which it appeared that the genus had separate ribs as in *Sphargis*, and that the only carapace was formed by large radiating plates of bone in the skin. Two other species were described, *P. tuberosus* and *P. neptunius*—the last, the largest known marine turtle—Mr Eli K. Price read a paper on "Some other Phases of Modern Philosophy," in which he combatted the views of Huxley and others as to the physical basis of life.

PARIS

Academy of Sciences, May 20—M. Becquerel read a ninth memoir on the means of increasing the effects of electro-capillary actions in inorganic bodies, and on the effects of the same kind produced in living organised bodies.—M. Sainte-Claire Deville presented a note by M. G. Guéroult on the relations existing between the numbers of vibrations of musical sounds and their intervals, and on a scale-rule for acoustic calculations invented by him.—M. Jamin communicated a note by M. J. M. Gaugain on the electro-motive forces developed by the contact of metals with inactive liquids, in continuation of a former paper by the same author; and M. T. Du Moncel presented a note on the induced currents resulting from the action of magnets upon induction coils normally to their axes.—A memoir was read by M. Le Verrier on the theories of the four superior planets, Jupiter, Saturn, Uranus, and Neptune, containing an investigation of the perturbations which each of these four planets undergo by the action of the other three.—A letter was read from Father Secchi, containing a summary of observations of solar protuberances from Jan. 1 to April 29, 1872, containing a tabular exposition of the results of observations, with a discussion of their bearing upon the general question.—M. Delaunay presented an extract of a letter from M. Förster on the magnetic disturbances observed during the occurrence of auroras.—M. Coste communicated a note by M. Z. Gerbe on the segmentation of the cicatricula in the ovum of plagiostomous fishes, in which the author describes the evolution of the Rays in confirmation of the opinion put forward by M. Coste that in the plagiostomous fishes, as in reptiles and birds, it is the cicatricula alone that undergoes segmentation. M. Coste also communicated a notes by M. G. Pouchet, on blue colorations in fishes, in which the author ascribes the blue colours presented by some parts of certain fishes to the presence beneath the skin of oval or roundish bodies which he calls "iridising bodies."—A note by M. E. Prillieux, on the influence of congelation upon the weight of vegetable tissues, was presented by M. Duchartre.—M. A. Rivière read a memoir on the oolitic or Jurassic formation of La Vendée, accompanied by a geological map of that locality.—M. H. Douvillé presented a note on the coal-bearing strata of the banks of the Rhine, with especial reference to the distribution of these deposits into an upper and lower formation, the deposition of which was separated by a great dislocation.—M. S. Meunier presented a mineralogical investigation of the grey serpentines, from which it results that these rocks consist essentially of a mixture of magnetite, pyroxene, peridot, and magnesite.—M. A. Leymerie communicated a note in reply to a recent communication by M. Garrigou on the unity of composition of the Pyrenees.

May 27.—M. Serret presented a memoir by M. V. J. Berton on the determination of the limits between which a primary number of a given form occurs.—M. Chasles presented a note by Mr. A. Cayley on a flattened quartic surface; M. Ribaucour a note on the developates of surfaces.—A note by M. C. Jordan on the infinitely small oscillations of material systems was presented by M. Yvon Villarceau.—M. Faye presented a reply by M. Respighi to a recent note by Father Secchi on some peculiarities of the constitution of the sun.—M. Le Verrier communicated a note by Father Denza on meteors observed in Piedmont on the evening of April 24, including the account of a second meteor in addition to that seen at Agde by M. Perris. The author also notices the occurrence of a fine aurora on May 9. —M. Becquerel read a note on the cultivation of the vine in clay soils, in which he indicated the conditions of temperature prevailing in clay, silicious, and calcareous soils, and showed that in the former the vine could only be successfully cultivated by training it to a considerable height.—M. W. de Fonvielle presented a note embodying some fresh examples of the danger arising from the vicinity of metallic masses during storms, and M. E. Nasse forwarded a note on an instance of globular lightning observed at Brives on May 17.—M. Delaunay presented a note by M. Frou on the laws of cyclones and tempests, and on their geometrical representations.—A considerable number of papers on chemical subjects was presented, namely, a memoir on the iron contained in the blood and in food by M. Boussingault, containing a great number of interesting details upon this important subject; a note by M. A. Wurtz on an aldehyde-alcohol, $C^4 H^8 O^2$, which he proposes to name *aldol*; a note by M. G. Bouchardat on a new organic base, dulcitamine, $C^{12} H^{15} NO^{10}$, derived from dulcite, presented by M. A. Wurtz; a note by M. T. Schlœsing on the influence of vegetable mould on the mobility of soils, communicated by M. Peligot; a memoir by Mr. F. Crace-Calvert on bleaching powder, presented by M. Balard; a note by M. B. Rénault on a new process for obtaining reproductions of drawings, also presented by M. Balard; and a note by M. Sidot on the production of a crystallised phosphuret of iron, presented with some remarks by M. Daubrée.—M. Cl. Bernard communicated a paper by M. Z. Pupier, containing an experimental demonstration of the action of spirituous beverages upon the liver; the author's experiments were made upon fowls and rabbits.—A note by MM. N. and E. Joly on the supposed Crustacean, on which Latreille founded the genus *Prosopistoma*, was presented by M. Milne-Edwards; the authors show that this animal is a true insect probably allied to the Ephemerina.—M. Blanchard communicated a note by M. S. Jourdain on the anurous Batrachia with large and small tadpoles, in which the author insists upon an analogy between the development of the former and that of insects.—M. de Vibraye communicated a note on the spontaneous appearance in France of exotic forage-plants consequent on the presence of the belligerent armies in 1870-71. —M. P. Gervais presented a note on the mammalia of which the bones are associated with the deposits of phosphate of lime in the departments of Tarn-et-Garonne and the Lot. These bones appear to belong to various epochs, the oldest belonging to the fauna of the Paris gypsum, others to the Miocene or more recent times. The genera represented are *Anoplotherium*, *Dichobune*, *Entelodon*, *Cainotherium*, *Amphitragulus*, *Palæotherium*, *Rhinoceros*, *Hyænodon*, *Canis?* (*palæolycos*, sp. n.) and *Viverra?* (*ambigua*, sp. n.) At Caylux there are Rodents allied to *Theridomys*, with remains of *Chalicotherium*, *Anthracotherium*, and *Antilope? boodon*.—M. Daubrée made some remarks on the deposits from which the fossils noticed by M. Gervais are derived.—M. de Verneuil read a note on the recent eruption of Vesuvius, which also formed the subject of a communication from M. Guiscardi.

BOOKS RECEIVED

English.—Experimental Chemistry, founded on the work of Dr. J. A. Stockhardt: C. W. Heaton (Bell and Daldy).—Knapsack Manual for Sportsmen in the Field: E. Ward (Bradbury and Evans).

DIARY

THURSDAY, June 6.

Royal Institution, at 3.—On Heat and Light: Prof Tyndall, F.R.S.

Society of Antiquaries, at 8.30.—Excavations at Rome, 1871–2: J. H. Parker, C.B., F.S.A.

Linnean Society, at 8.—On some recent forms of *Lagenæ* from Deep-Sea Dredgings in the Japanese Seas: F. W. O. Rymer Jones.—On the Cutaneous Exudation of the Water Newt (*Triton cristatus*): Miss Eleanor A. Ormerod.

Chemical Society, at 8.

FRIDAY, June 7.

Geologists' Association, at 8.—On the Classification of the Cambrian and Silurian Rocks: H Hicks—On the Silurian Rocks of the English Lake District: Prof. A. Nicholson, M.D.

SATURDAY, June 8.

Government School of Mines, at 8.—On Geology: Dr. Cobbold, F.R.S.

MONDAY, June 10.

Royal Geographical Society, at 8.30.

TUESDAY, June 11.

Photographic Society, at 8—On some early Glass Pictures produced by the late Sir John Herschel, Bart.: Prof. A. S. Herschel, B A., F.R A.S.—On the Photographic Manipulations undertaken at the last Eclipse, practically described: Capt. Waterhouse, Assistant Surveyor General of India.—Spectroscopic Observations in connection with the Carbon Process: Lieut. Abney, R.E.—On the Use of Uranium in Dry Plate Photography: Colonel Stuart Wortley.

THURSDAY, June 13.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Mathematical Society, at 8.—On the Surfaces divisible into Squares by Curves of Curvature: Prof. Cayley.—On Prof. Cremona's Transformation between Two Planes and Tables relating thereto: Mr. S. Roberts. —On a Manifold Correspondence of Two Planes: Dr. Hirst.

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THURSDAY, JUNE 13, 1872

THE BRITISH MUSEUM

THE Return published by order of the House of Commons on April 19, 1872, of the progress made in the arrangement of the collections, and of the objects which have been added during the past year to the National Museum, is a document which, although only seen by a very few persons, cannot fail to be of interest to a very large proportion of English people, and also to those engaged in scientific pursuits all over the world.

Confining our attention to the Natural History Department, and commencing with Prof. Owen's general report, we find that, although every care has been taken on the part of Heads of Departments to limit in all possible ways by declining or postponing all acquisitions not absolutely and immediately pressing, the annual tide of new specimens still flows in, and during the past year 15,879 specimens have been added to and incorporated with the present crowded series.

Everywhere throughout Prof. Owen's report the old grief of "want of space" is introduced and persistently pressed upon the attention of the Legislature; but at the same time allusion is also constantly made to the time when the requirements for increased exhibition-space, and more adequate store-rooms and cabinets, will have all been met by the galleries of the new Museum.

We are glad to see that Prof. Owen puts forward other very good and sound reasons, besides the mere overcrowding, in favour of a speedy release from the too-restricted galleries and studies to which the collections under his care are at present restricted. "Inadequacy of space," writes the Superintendent, "is associated with loss of time." Thus:. "To meet the wishes and expectations of scientific visitors and students in regard to opportunities of study and inspection of specimens relating to the latest advances in natural history, the well-filled or crowded exhibition-galleries and cases have to undergo more or less of re-arrangement with removal and interchange of specimens, in order to allow place and access to the novelties."

Again, in the case of the delicate and brittle specimens, the risk which is now incurred in frequently shifting these will then be reduced to its minimum, and, consequently, the chances of preserving such treasures will be greatly increased. It now not unfrequently happens that an entire series of objects have to be removed and rearranged to gain a few feet or even inches of space in some cases.

All this labour and care in the re-adjustment of series, which would be needless if there were abundance of exhibition space, will be removed in the new Museum, and the same amount of labour can be devoted to the more satisfactory task of classification and the true advancement of Natural Science.

Prof. Owen gives in a tabulated form the annual additions made to each Department during the twelve years from 1859-70. The total increase for each is as under:—

	Zoology	Geology	Mineralogy	Botany	TOTAL
Total increase for 12 years..	435,492	78,434	24,945	123,409	662,280*

Of course, in certain years the increase in one or other of these Departments has been specially large; thus in 1863 98,754 specimens were added to the Zoological Department, whilst in the following year only 7,688 specimens were acquired. After all, numbers only convey a very imperfect idea of increase, so much depends upon the size of objects themselves. Thus, for instance, a collection of Coleopterous insects, comprising many thousands of examples, may all be contained in a small cabinet of twenty drawers; whilst a tapir, antelope, or other quadruped may when stuffed need a case all to itself, or a whale may require an entire gallery 90ft. long for its proper display. We are glad to hear that the birds and shells are better off for room than the other Natural History divisions; we have always felt when passing through this gallery with its fine series of table-cases, and the birds occupying the wall-cases, that these two groups were specially favoured; whilst the adjoining mammalian-saloon but very inadequately represented the actual exhibition needs of the higher animals. All this Prof. Owen tells us will be set right in the new Museum.

The use of methylated spirit for the preservation of Reptilia and fishes has produced in some cases injurious results to some of these specimens. Dr. Gunther will, no doubt, speedily remedy this mischief, and the purer medium must in future alone be used. All the other collections are reported as in good preservation.

There is a long list as usual of additions to each of the Departments. Dr. Gray reports a fine series of Lemurs from Madagascar; a *Grampus Rissoanus*, and a Pike whale (*Balænoptera rostrata*), from the English coast. Various Mammals from Abyssinia, chiefly of the Antilope family. Among the Birds we find Pheasants, Hornbills, and Pigeons, and many lesser members of the feathered tribe. Fishes, Reptiles, Mollusca, and tribes of Insects, have also been added, likewise specimens of the "Glass-rope-*sponge*" (called a "Coral" in the Report), the *Hyalonema* from Japan, and the beautiful vitreous sponge, the *Pheronema Grayi*, from the coast of Portugal. Making a total in all of 10,577 specimens added during the past year to the Zoological Department.

Of Fossils there seems to be a goodly supply. From the La Plata come bones of the *Toxodon*, *Mylodon*, and *Macrauchenia*, the *Drepanodon*, and the *Glyptodon*; from Australia remains of the *Thylacoleo* (that queer marsupial which Prof. Owen demonstrates to be a carnivore, and Prof. Flower essayed to make out to be only a great root-feeding Wombat), the *Diprotodon* (a gigantic kangaroo, which probably did not hop, like the modern kangaroo, but went on all-fours), the *Nototherium*, *Macropus*, and many others. The most important acquisition during the past year is that of the Van Breda collection from Haarlem. This well-known collection, commenced by Dr. Peter Camper, the celebrated Dutch anatomist, more than 100 years ago, has since been largely increased by his grandson, the late Prof. Van Breda, secretary for more than fifty years to the Royal Dutch Society in Haarlem, who inherited Camper's original collection.

The series of specimens is particulary rich in remains of the great Meuse lizard, the *Mosasaurus hofmani*, and of a giant tortoise, the *Chelone hofmani*, and many fine remains of fishes and reptilia from the Maestricht chalk, the Solenhofen stone, the Brown Coal of Bonn, the Miocene limestone of Oeningen, and the Trias and Keuper of Germany.

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* The total numbers are wrongly cast in the printed return.

Of English fossils the finest addition is that of the Wetherell collection, the author of which was for more than fifty years a diligent collector of London clay fossils at Highgate and its vicinity. This collection is a truly Metropolitan series, and will, when the new Museum is prepared, doubtless have, as it deserves, a suitable exhibition case. Altogether there have been added to the Department of Geology a total of 4,789 specimens.

Mineralogical science is well cared for by Prof. Maskelyne. Only 513 specimens have been added, but these are each one a valuable addition to the cases. Among the most important may be mentioned the Chlorides and Iodides of Silver, in distinct crystals, from Chili, and a series of remarkable crystals and masses of Iceland spar.

In the Laboratory new minerals of the Dufrenite class have been examined, to which the name "Andrewsite" has been given; a blue mineral from South Africa proves on analysis to be "Percylite," only known hitherto in the specimen preserved in the British Museum. The Cranbourne-Meteorite is still being investigated.

In the Botanical Department, Mr. Carruthers reports a long series of important acquisitions during the past year. Among other interesting additions to the exhibited series may be named a series of microscopic preparations of fossil plants, selected partly from the collection bequeathed by Robert Brown, and partly from the specimens subsequently acquired by the trustees, and exhibited so as to show their minute structure by the help of light reflected from mirrors.

It will be seen that the key-note of the Report is the hope of better times coming, not only for the National Collection, but for its Officers, in the New Museum. In this hope we most heartily concur, and trust that not the least reform which will then be inaugurated will be the entire modification of the present system, by which the actual amount of instruction gained by the public appears to be in inverse proportion to the vast resources of this great establishment if well and ably administered. This is not as it should be. There is no reason why the British Museum should not become, under its new management, one of the first educational establishments of the country.

NEW WORKS ON GEOMETRY

Solid Geometry and Conic Sections, with Appendices on Transversals and Harmonic Division; for the Use of Schools. By J. M. Wilson, M.A. (Macmillan and Co. 1872.)

Geometrical Note-Book, containing Easy Problems in Geometrical Drawing preparatory to the Study of Geometry; for the Use of Schools. By F. E. Kitchener, M.A. 2nd edition, revised. (Macmillan and Co.)

THESE works are by mathematical masters at Rugby School. Their united aim (in connection with Mr. Wilson's two previous parts) is to provide a complete course of geometrical teaching, and so to meet a crying want of our schools, which has of late led to so much agitation. It is now nearly four years since Mr. Wilson's first part and Mr. Kitchener's first edition appeared, at which date an eminent reviewer, in his notice of the "Elementary Geometry," remarked that the forces were mustering for the battle. Since that time considerable progress has been made in the agitation, and, thanks to the Association for the Improvement of Geometrical Teaching, which owes its origin in part to a correspondence commenced in this journal, there seems to be a fair prospect, if not of perfect success, yet of the whole subject of geometrical teaching being placed on a more satisfactory basis.

The very first resolution put forward by this Association in its second Annual Report is, "That some practical familiarity with geometrical construction should precede theoretical study; and that the teaching of geometry would gain by the free introduction of easy exercises and numerical examples or illustrations." We presume that a main object of Mr. Kitchener's book is to meet this suggestion; and from the fact of a second edition being called for we may fairly suppose that it has met a felt want. It will be barely necessary to do more here than point out wherein this edition differs from its predecessor. Without going into details, we may state that the size of the page has been materially enlarged; that instead of blank spaces being left for figures to be drawn by pupils on the printed pages, there are now inserted between every two pages of type two sheets of admirable blank paper. The paging is continued on these blank sheets. There are three parts in the place of two; the use of the protractor is relegated to part ii.; on p. 37 a return has been made to Euclid's definition of parallels (this is doubtless in consequence of the agreement come to and embodied in the seventh resolution of the above cited Report); the third part contains a few simple constructions connected with tangency and the circumscription of figures. There are apparently no typographical errors in this well got-up work, and we take leave of it commending it to all teachers engaged in the arduous task of instilling geometrical notions into junior students' heads.

It appears to us that the following are the only oversights to point out:—Should not 7 precede 6 on p. 49? Exercise 30, p. 25, should be expunged; it is given on p. 61, 4, where a definition is given which is wanting in the former place. In Exercise 14, p. 62, the case of parallel lines has been overlooked; and on p. 50, for 514 we get 534 nearly =.

To turn now to Mr. Wilson's book. This is characterised by his usual clearness and ability. The selections and the grouping are, in the main, all that we desire in a school treatise; we would, however, have preferred a somewhat more extended treatment of the sphere and cylinder. Some properties of the former are cited on p. 79 which have not been given in the work itself. A little fuller notice of these solids would fit the book to meet the requirements of candidates for the B.A. London Examination; as by a recent extension of the University scheme students are expected to be acquainted with the proofs of the properties of these figures, which are assumed in treatises on mensuration of solids. As indicated in the title, the work consists of three parts; the first part covers the ground occupied by Euclid xi. 1—21, and further treats of polyhedra and stereometry, the whole presented in a clear and satisfactory manner, certainly in a form not hard to be understood by an average schoolboy who has mastered any ordinary treatise on Plane Geometry. We have found our own pupils to read it with interest, partly for a reason put forward by the author, that "the geometrical imagination is exercised." We notice for the first time, we believe, in a text-book, the term "disposition," in the following connection:—"Parallel planes are those which have the same disposition in space:" the discussion raised in these columns in connection with Mr. Wilson's application for a suitable term will be within the recollection of most readers, and if we mistake not we are indebted to Dr. Hirst for the suggestion of this appropriate word. We note on this page (9) a curious oversight, which, however, the student can readily correct.* On p. 45, for $\frac{1}{2}\pi$ read 2π. In the short and handy notice of transversals we observe the use of the term "sense," as equivalent to direction. On p. 64, last line but one, read $\frac{DA}{CA} = -\frac{BD}{BC}$; and on p. 67, third line, read $\frac{AB.CD}{BC.AD} =$ &c. Other slight typographical mistakes will give the reader no trouble. The last of the three parts contains a capital summary of the chief properties of the conic sections—just sufficient, we think, for class use; for, with the limited time at our disposal now-a-days, it is almost useless attempting to take up such extended treatises as the admirable ones by Drew and Besant. The figure on p. 128 is not quite correctly lettered in accordance with the proof that accompanies it.

We have examined and used this book with much satisfaction, and hope to see it pass through several editions, as we think it calculated to raise the study of solid geometry "to a more prominent position in geometrical instruction," and to put the subject of geometrical conic sections in a more satisfactory state than it at present occupies. R. T.

OUR BOOK SHELF

The Year-Book of Facts in Science and Art. By John Timbs. (London: Lockwood and Co. 1872.)

MR. TIMBS'S books always produce upon us the effect of an ill-assorted dinner. There is plenty of solid food, but along with it some that is anything but wholesome; and the concatenation is badly managed, and the cooking none of the best. To take the concatenation first: The paragraphs in this volume are arranged under a variety of headings, but on what principle the assortment is made we have failed to discover. Thus we have paragraphs on Surface Movements of the Earth and on the Secular Cooling and Figure of the Earth, under Natural Philosophy; on Earthquakes and Volcanoes, under Geology and Mineralogy; on Protuberances of the Sun, under Natural Philosophy; and on Vast Sun Spots, under Astronomy and Meteorology; while two long accounts of the Gun-Cotton Explosion at Stowmarket are given, one under Mechanical and Useful Arts; the other under Chemical Science. Next, as to the cooking, in other words, editing. Very little pains appear to have been taken to go to the best authorities on each subject, or to trace statements to their original source. For instance, admirable papers as are the *Spectator* and *Pall Mall Gazette*, we hardly care to know what the one thinks as to the chance of men ever being able to fly, or the other about the sensitiveness of frogs during vivisection; and some more authoritative judgment on Prof. Tyndall's experiments on the purity of water might have been found than that of an anonymous writer in the *Times*. It is surely the result of careless editing to find on the same page two descriptions of the same bone-cave in Pennsylvania, although in one instance it is described, by a slight geographical confusion, as being situated "in Philadelphia." Very familiar proper names are constantly misspelt or misquoted. Thus we hardly recognise Padre Secchi under the disguise of "Seeche;" or the admirable Genevan Society which has published so many valuable contributions to science, under the name of "The Society of Physics and Natural History of Ginevra." As to the unwholesome and absolutely indigestible food, we will refer only to a single actual error. Canon Kingsley will be surprised to be made responsible, on the authority of our excellent contemporary the *Builder*, for the statement that "lime is a metal called by chemists 'calcium;' but it is never found in that state in nature. It is found in a rocky or chalk form." Other blunders almost as gross could be quoted. The book gives us the impression that the compiler was under the necessity of filling a certain number of pages, and that for this purpose the scissors and paste were freely used on the material that came the readiest to hand. The worst is that by the non-scientific public such books are taken as an authoritative record of the progress of science during the year, and of the most important inventions and discoveries, and the most striking new applications of old principles.

Righthandedness. By Daniel Wilson, LL.D., Professor of History and English Literature in University College, Toronto. Pp. 40. (Toronto, 1872.)

THIS pamphlet contains some useful facts bearing on the question why some people are left-handed, and the antecedent question why more are right-handed. Prof. Wilson takes a sufficiently comprehensive view of the subject. He admits that the problem does not concern the hand alone, but the foot, the eye, and the whole body. He admits that a similar preference for one side may be found among the lower animals; and he re-states with some fresh illustrations the grounds which have led previous writers on the subject to conclude that right-handedness is the normal condition of all the existing tribes of man, and has been so as far back as history, tradition, or language extend. He raises the question whether, in some cases, we have not translated the ancient terms inversely, so that the favourite and stronger hand may have been with certain of the ancient nations the left; but has no difficulty in showing, from the described relations to the points of the compass, from the form of weapons, and from many other sources, that what we call the right hand has always been the one chiefly used. In another passage Dr. Wilson refutes the idea that the Egyptians were a left-handed race, and shows that it has arisen from the greater convenience of drawing the figure left-handed in certain cases. The evidence from language is discussed, and the etymology of the words *dexter*, *decem*, *quinque*, *sinister* is accepted, as given by Grimm, Donaldson, and other philologists, who long ago pointed out the connection in various languages between the words which express the number ten and those for the right hand, the fingers of which complete the tale of ten.

The learned Canadian Professor does not offer any new theory of the reason for the general preference of the right hand, or the occasional preference of the left. He discusses the hypotheses advanced by Barclay, Hyrtl, Gratiolet, and Buchannan; and rightly rejects them all, as insufficient or contradicted by anatomical facts. Besides these writers, and Prof. Humphry, of Cambridge, who is also quoted, Dr. William Ogle has lately published, in the "Medico-Chirurgical Transactions," an interesting paper on "Dextral Pre-eminence," in which several new facts and observations are recorded.

Prof. Wilson's paper is a valuable contribution to the literature of the subject. It seems, however, to have been hastily revised. Thus, in four lines quoted from Ovid, there are two bad misprints, besides a doubtful reading.

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* There is a similar oversight in the proof of Cor. i, Theorem 27, p. 35.

At present the results obtained in this inquiry are the accumulation of facts and the refutation of untenable explanations. One may also, perhaps, affirm (1) that the primitive condition of man and other vertebrates was, as their early fœtal condition still is, one of complete bilateral symmetry of structure, and also of functional symmetry; (2) that this primitive ambidextrous use of the limbs is occasionally superseded in animals, and constantly in all races of men of which we have any knowledge, by a preferential use of one side; and that this is a necessary step in development as soon as the more delicate operations performed by a single hand take the place of those of digging, climbing, &c., in which both take part. It is, in fact, a differentiation produced by the same causes which have led to the specialisation of the fore and hind limbs in frogs, birds, or kangaroos, compared with their uniformity of structure and function in fishes, crocodiles, and horses; (3) the prevalent choice of the right hand when differentiation was established, must have depended on some slight advantage, at present unascertained, by which dexterity at last suppressed *gaucherie;* (4) The occasional preference for the left hand, which is often partial and sometimes hereditary, does not depend on any "coarse" structional abnormality, but is an instance of atavism—of reversion to the primitive and universal ambidextrous, or to a subsequent and partial lefthanded condition. P. H. PYE SMITH

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Ocean Currents

IF a free body on the earth's surface should be moved from a lower to a higher latitude without friction by a force in the direction of the meridian, it would acquire a certain amount of relative eastward velocity, which would be the same whether the body moved toward the pole with a very slow uniform velocity arising from a single impulse, or whether it moved with a continual accelerated velocity down a gradient by the force of gravity. If a particle of atmosphere or of the ocean is moved in the same way by a similar force, and does not acquire the same amount of relative eastward velocity, the difference between the velocities in the two cases is the true measure of the effect of friction. But from the amount of work done, or velocity generated or destroyed, nothing can be inferred with regard to the acting force, unless we take into account the length of time during which it acts. The velocity of the interchanging motion of the water between the equator and the pole, discovered by Dr. Carpenter, is extremely small, perhaps not as much as a mile per day, and less than tidal velocities in the open ocean. If a great amount of eastward velocity, therefore, is destroyed by friction in the case of a particle of the ocean in moving from a lower to a higher latitude, it is not because the friction is great, but because it acts during a very long time.

If the velocity in the direction of the meridian were one mile per day, the deflecting force, at the parallel of 45°, arising from the earth's rotation, would be about equal to the force of gravity along a regular gradient of 6ft. between the equator and the pole. Both the velocities north or south and east or west, and the amount of friction belonging to any given velocity, are unknown quantities. The force down the gradient of 6 ft. between the equator and the pole, which Mr. Croll allows to be the measure of the force due to the difference of temperature or density between the equator and the pole, would soon give the particles of water without friction the velocity of one mile per day, and the question now is, whether the force would then be sufficient to overcome the friction belonging to this small velocity; for the inertia of the water being once overcome, all that would be required of the force would be to overcome the friction.

The velocity of motion between the equator and the pole being probably of the same order only as tidal velocities, we may suppose the friction in the two cases to be somewhat of the same order. Now the friction in tidal velocities in the open sea, when not regarded as entirely insensible, has always been regarded as a quantity of a second order in comparison with the disturbing forces producing the tides, or at least as being much less. But the force producing the tides is about equal to the force of gravity along a gradient of two feet between the equator and the poles. If we therefore suppose the force necessary to overcome the friction of tidal velocities to be one-fourth of that producing the tides, then the force of gravity on a gradient of 6 in. between the equator and the poles, would be sufficient to overcome the friction belonging to the slow velocities of the motion discovered by Dr. Carpenter, if we suppose, as above, that the amount of friction in the two cases is of the same order. But this is only one-twelfth part of what Mr. Croll allows to be the available force for this purpose. According to the preceding view, therefore, the difficulty is not in finding a sufficient force for overcoming the friction of Dr. Carpenter's slow velocities, but in disposing of the abundance of force we have on hand for that purpose.

Washington, May 24 WM. FERREL

The Wanderings of the Esquimaux

I AM much obliged to Dr. Rae for his courteous letter upon a subject in which he is so deservedly an authority, while I am only a student. The question upon which he joins issue with me, namely, the southerly migration of the Esquimax, is one upon which I ought to bow to his authority; but there are some difficulties attending the solution he offers which deserve, I think, consideration. If the Greenland Esquimaux came from the north, as both of us are agreed, it is probable that the stock from which they sprung are the so-called Arctic Highlanders of Smith's Sound, about whom Mr. C. Markham has written both in the *Ethnological Journal* and the *Journal of the Geographical Society*. They are broadly distinguished from the Esquimaux of America by the use of stone igloos instead of snow huts, and by their ignorance of boats, either kayaks or omenaks. These Arctic Highlanders are a diminishing race. M'Clintock tells us "that they have become alarmed by the rapid diminution of their numbers through famine and disease" (Travels, 137). Along the shores of Smith's Sound are the ruins of many deserted huts, moss-grown, and of considerable antiquity. The skulls of musk oxen without their lower jaws, a proof that they had been killed by Esquimaux, are also found scattered along the same coast (id. 76). These facts show that the Esquimaux were formerly abundant in very high latitudes, where they have now become very few in number. What is true of Smith's Sound is also true of the great Archipelago of the Arctic circle known as the Parry Islands. These also are dotted with the moss-grown ruins of ancient and deserted huts, the remains of a once numerous race in an area now, I believe, entirely deserted by the Esquimaux. These two places are in the highest latitudes yet reached by navigators.

Dr. Rae would make these Northern Esquimaux to be wanderers from the American Continent; but, putting aside the difference of customs, which seem to show that the American Esquimaux are not the primitive stock of the race, but have been sophisticated by contact with the Indians, I may quote the following passage from Mr. Markham, who has studied the question with some care:—"The American Esquimaux never go from their own hunting range for any distance to the inhospitable north. Except in the case of the Pond's Bay natives who followed up the whalers for a specific reason, there is no instance of their having gone north, and it is unreasonable to suppose that they would do so" (*Journal of Ethnological Society*, 4, new series, 135).

When I quoted the traditions about the migration of the Esquimaux having been southerly, I had the passage especially in my mind where Sir John Franklin describes the Chippewyan legend about the discovery of copper, beginning with the words, 'The Chippewyans suppose the Esquimaux originally inhabited some land to the northward, which is separated by the sea from this country; and that in the earliest ages of the world a party of these men came over and stole a woman from their tribe, whom they carried to this distant country, and kept in a state of slavery, &c." (Franklin's Narrative, 146).

Dr. Rae, on the contrary, thinks they came from the west. Now Mackenzie, who certainly knew the country well, says:—"The progress of the Esquimaux, who possess the sea-coast from the Atlantic through Hudson's Straits and Bay, round to Mackenzie River, and, I believe, further, is *known to be westward. They never quit the coast*" (Mackenzie's Travels, 406).

The Asiatic origin of the Esquimaux seems to me to be a very problematical solution of the difficulty. There are Esquimaux in Asia undoubtedly. The so-called Tchuktchi of Tchuktchi Ness are Esquimaux, but they are a very small fragment, and are apparently emigrants from the opposite shores of Behring Straits. Between them and the American Esquimaux there is a considerable intercourse, which has led to the products of Russian manufacture being found along the shores of the Arctic Ocean; and the American Esquimaux constantly pass the Straits for purposes of barter. Beyond the fragment of this people found at the extreme north-eastern part of Asia, and a few of the Kamskatkans, I know of no Asiatic race whose language, or custom, or physique, favours the opinion that they are connected with the Esquimaux. The course of migration has rather been westerly and easterly. The Tchuktchi proper and the Koriaks, who are a very different race from the Tchuktchi of Tchuktchi Ness, have been pressing to the west, and have uprooted numerous tribes, such as the Omoki, &c. These latter were portions of a widely-spread race now represented by the fast-diminishing Jukagiri, whose language, so far as I have examined it, is very different from the Esquimaux (a copious vocabulary may be seen in Billing's travels). Again, between the travels of Cooke and those of Whymper, the language of the inhabitants of Kotzebue Sound has changed considerably, and become more Esquimaux, which seems to show that the Esquimaux have quite recently been pressing in this direction also.

All these facts point to apparently only one conclusion, that the original home of the Esquimaux was in the regions near the Pole, from which they have migrated to a more temperate climate; and I can see no good cause for such migration, except the increasing rigour of the climate. The question is one of great interest, both to ethnology and physical geography, and I hope Dr. Rae will favour your readers with some more facts on the other side.

While the evidence seems to be overpowering that the climate has been gradually growing more severe in the highest latitudes, there is a good deal of evidence which points to a corresponding decrease of severity elsewhere. We cannot read the descriptions of Gaul and Germany in Roman times and reconcile them to the state of things that at present exists there, without believing that the climate has very much improved. It is rash to take isolated seasons, but we may compare with profit the accounts of the terrible winters of Roman days, during which the Rhine and Danube were frequently frozen over, with the comparative mildness of modern times. The reindeer and the elk then ranged far to south of their present habitat. Ovid's lamentations over the fearful severity of his place of exile on the coast of Thrace are matched by the accounts of white foxes, &c., which then lived there, and by the proverbial rigour of the winters on the coasts of the Black Sea; while the diminution in the energy and vigour of the races that inhabit the Mediterranean borderland can best be accounted for by the theory which makes them to have in some measure succumbed to a more enervating climate. If this be true, we have a very remarkable fact hitherto ignored, so far as I know, by scientific men, namely, that it is possible that circumpolar climate may have been very temperate when the climate of more southern latitudes was very severe. This paradox, upon which I should very much like to have the opinion of some of your correspondents, is favoured by the following fact:—"It is a common remark of those who frequent the Polar seas, that they find always the least obstruction from ice when the preceding winter has been very severe in more southern latitudes. In the year 1766, though the frost had proved most intense through the rest of Europe, the whalers reached a high latitude; and not to multiply instances, the three last seasons (*i.e.* 1815, 16, and 17), which have been reckoned very open, have succeeded to winters notoriously cold and protracted," *Edinburgh Review*, 30, 34). We have only to extend the analogy of a season to a number of seasons, and we at once get a similar result to the one above named, *i.e.*, that an increase of severity of climate in low latitudes is balanced by a diminution of severity in high ones. I need hardly point the moral in the present letter of the value of such a result in speculations on the existence of the mammoth in Siberia and Northern Russia during the Glacial epoch, &c.

Derby House, Eccles, June 4 HENRY H. HOWORTH

Origin of Cyclones

IN NATURE of August 17, 1871, Mr. J. J. Murphy calls attention to a paper by Mr. Meldrum on the origin of storms in the Bay of Bengal, &c. This paper advocates the theory of their origin "in the meeting of the trade-winds in the northern and southern hemispheres, at some distance north or south of the equator."

Mr. Murphy says:—"Over the greater part of the Pacific cyclones do not appear to be found. The reason of this probably is that, in consequence of the temperature of the sea changing but little with the seasons, the two trade-winds over the Pacific meet each other nearly on the equator all the year round; though I do not know how far this is confirmed by observations on the winds of that ocean."

Very little is known about the meteorology of this part of the Pacific, and my object in writing is to communicate to Mr. Murphy and others who may be interested in the subject the following facts:—

1. There is rarely a year without at least one cyclone passing through, or in the neighbourhood of, one of the following groups of islands, viz., Fiji, Samoa, or Hervey.

2. The cyclone season extends over a greater part of the period during which the sun is south of the equator; consequently, when the trade winds from the north reach farthest south. Cyclones are most prevalent about the middle of the season, or rather later than the middle. They rarely visit us earlier than December or January.

3. They are usually preceded for a few days by strong northerly winds. During such winds we watch the barometer very carefully, as a sudden fall is a sure indication of a cyclone near at hand.

I may add that a strong northerly wind is blowing in this group at the present time. It was indicated by the barometer thirty-six hours before it reached us, and was preceded by a day's calm. It then burst upon us suddenly with fury, but after a few hours moderated to about the force of the S. E. trade-wind.

S. J. WHITMEE

Leulumoega, Samoa, South Pacific, Jan. 8

P.S.—Since the date of my last note we have had two earthquakes—Nov. 13, at 5.5 P.M., vertical, with a great rumbling preceding and accompanying it; Dec. 15, at 12 noon: double shock, with an interval of thirty seconds: slight.

Rain after Fire

SOME old settlers believe that great bush fires cause rain. During this summer exceptionally dry weather has prevailed over the greater part of New Zealand, more especially along the eastern coast; in several of the towns prayers for rain have been offered up in the churches. I beg to send some notes of recent date extracted from our home diary. It should be stated that this district, adjoining Banks' Peninsula, has been suffering from extensive bush fires since the 18th of last month.

"Feb. 1. Strong N.-Wester; very hot; 92° in the shade under the back verandah. Heavy bush fire on the hill still burning; showers in the evening."

On the coast here it is very unusual to have rain with a N.W. wind, which is dry and parching.

"Feb. 3. Strong N.-Easter; showers in the evening."

The bush fire was still burning, and continued to do so till the 7th. On the 5th most of the neighbours turned out to save some property in great danger of being destroyed by the fire. N.E. is a cool dry wind from the sea.

"Feb. 16. Strong N.E. Heavy bush fire under Omauhate and about Cass Peak. Slight showers from the S.W. in the morning; wind veered round to N.E."

Our rainy quarter is S.W. with a low temperature; less frequently we experience thick weather from S.E., accompanied by fine rain. These fires have been traversing a range of hills (more or less timbered in the gullies), their heights from eleven to about four hundred feet above the level of the sea. I believe the showers noted to have been as local as the fires; the direction of the wind is given as prevalent on the dates mentioned, with some indication as to its strength, but we have no memoranda as to its force during the actual fall of rain. Opportunities of watching the gradual formation of cumulus cloud above dense volumes of smoke are by no means rare in this part of New Zealand, where the occupiers of Crown land have periodical burnings of their run, or great portions thereof, in early spring. These notes are forwarded in the hope of helping to illustrate the question of whether fires cause rain, no opinion is ventured on the subject, but this curious phenomenon should be further investigated. THOMAS H. POTTS

Ohinitahi, New Zealand, Feb. 19

On Adhesion Figures

A DROP of crude carbolic acid placed on the surface of cold water exhibits the most surprising and beautiful "cohesion figures." I am not aware that Mr. Tomlinson has described the behaviour of this substance. So lively and unpredicted are the movements of the drop that its action resembles that of a living creature. At first it pulsates, then its edge breaks up into crispations, and a motion begins like the waving tentacles of a sea-anemone. Sometimes the drop will sail about in a crescent shape, or shoot out independent little rings, which gyrate and rush about like a rotifer, until at last they burst into a myriad of intensely active little specks. Warm water destroys all action, by lessening, I suppose, the adhesion of the liquids.

To your readers it is trite and useless to remark upon the interest that attaches to the careful and continued observation of the most familiar things. But I wish the leisure classes could understand this. How many pleasant and instructive hours might some of them spend in examining such common things as essential oil dropped on water, even drops of ink falling through water, or puffs of smoke through air or a candle flame, or a hydrogen flame, or iron filings sprinkled on paper over a magnet, or, among other simple things, best of all a block of ice in a sunbeam. From the contemplation of such phenomena one rises with an increasing joy, and not a little humbled at one's own ignorance before the orderly mystery that pervades everything.

24, Elgin Road, W. J. H. SPALDING

A Suggestion to Opticians

THERE is a method of mounting self-registering meteorological thermometers very commonly employed, and one which for some purposes, as, for instance, for determining the temperature immediately over the surface of the ground, is an excellent plan.

I allude to the placing of the divided thermometer stem in a larger tube forming a jacket to it, and fixing it with india-rubber packing, which makes an air-tight stopper round the neck.

Now it is frequently observed that when thermometers fitted in this manner are exposed to cold, a copious deposition of dew takes place, both on the stem and in the interior of the jacket, rendering the accurate reading of the instrument a matter of some difficulty. I would therefore suggest to makers of this class of instrument whether it would not be advisable for them to *dry*, I do not mean merely heat, the air in the tubes, or else enclose some water-absorbing substance, as calcium-chloride, in the tube before finally inserting the india-rubber stopper.

It is extremely probable my suggestion is not a new one. I have no recollection, however, of having heard of such a plan being employed by any maker. G. MATHUS WHIPPLE

Kew Observatory, June 10

The Ferrara Floods

MAY I ask for space to draw the attention of engineers to the question whether the beds of embanked rivers rise or not? The affirmative is argued (and in reference to Ferrara) in the last chapter of "Rain and Rivers," against the negative of the eminent American engineer Ellet. The title of the chapter is "Ellet on the Mississippi." GEORGE GREENWOOD

Brookwood Park, Alresford, June 8

FORCE AND ENERGY

THE CONSERVATION OF ENERGY A FACT, NOT A HERESY OF SCIENCE

IN an article entitled "The Heresies of Science," published in a recent number of the *London Quarterly Review*, two widely different principles are oddly linked together as heretical dogmas, the doctrine of Evolution, and the Conservation of Energy.

On the doctrine of *Evolution* the writer has nothing to say.

Before discussing with the "Reviewer" the validity of the *Conservation of Energy*, it is quite necessary to define the terms which may be employed, such as Force, Energy, Potential, Sound, Light, Heat.

It is much to be regretted that a far greater degree of logical accuracy in the use of terms than is usually met with, does not exist amongst even the ablest writers on physics, for many of the arguments adduced against physical principles lie not against the principles themselves, but against the indefinite language in which they have from time to time been expressed. There is probably no term employed in physics that has been more misapplied, and in its misuse has led to greater confusion of ideas, than "force."

Force has been thus defined by our ablest modern physicist.* "What I mean by the word force is the source or sources of all possible actions of the particles or materials of the universe."

This definition of force is substantially the same as the writer's definition † to which the reviewer takes exception, but which may perhaps with advantage be thus amplified: Force is a mutual action between the atoms or molecules of matter, by which they are either attracted towards, or repelled from, each other; and by this action energy is imparted to the matter put in motion. It may be further remarked that force is essentially either attractive or repulsive.

The writer sees no reason to amend his definition of "Energy"—namely, that it is *the power of doing work.* It may, however, be remarked that the existence of energy in matter implies the existence of motion, and *vice versâ;* but it by no means follows that motion and energy are convertible terms, for motion means only the act of moving, or changing the position occupied in space.

The term "Potential" applied to force or energy means inactive, but capable of being called into action. Thus, if a weight be raised, a certain amount of energy is expended in raising it, and so long as the body is supported the energy expended in raising it remains potential in it, but when allowed to fall freely *in vacuo* to the level from which it was raised, the body acquires exactly the amount of energy that was expended in raising it. In the same manner the repulsive force of the molecules of the highly ignited gases into which gunpowder is resolved by ignition may with equal propriety be said to be potential in the unignited powder.

The remarks with which the writer's interpretation of the terms "force" and "energy" have been met by the reviewer may here be appropriately noticed. Quoting the introductory chapter already referred to, he adds: "his doctrine regarding the nature of force has thus no connection with sound philosophy; by force Mr. Brooke evidently means what other advocates of conservation mean by potential energy." Does then sound philosophy consist in the impossible task of agreeing as to the meaning of terms with those who do not agree amongst themselves? Or is sound philosophy monopolised by, and crystallised in the opinions of the reviewer? Sound philosophy would seem to consist rather in assigning appropriate meanings, involving no inconsistency or contradiction, to terms of frequent occurrence in all works on physics.

If the above definitions of force and energy be accepted, it is obvious that the term "force," as used by Grove, Tyndall, and many others, means sometimes force and sometimes energy. Thus, for example, "the conservation of force" becomes a simple truism, for its exercise being a function of matter, force must necessarily be coeval with matter. The reviewer (p. 22) thus quaintly expresses the relations of force, energy, and motion:—"A given motion viewed as a cause is force, while the very same motion thought as an effect is energy." Motion, it is presumed, can mean nothing else than the act of moving; but how the act of moving, whichever way we look at or think of it, can either produce energy or do work, it is difficult to conceive; has this "any connection with sound philosophy"? and does not the reviewer here himself "forget that each thing is itself, and not something else" (p. 22, l. 89).

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* Faraday, MSS. Croonian Lectures on Matter and Force, by H. Bence Jones, M.D. p. 35.

† Introductory chapter to the Sixth Edition of "The Elements of Natural Philosophy."

The writer would commend to the serious attention of his brother-physicists the above definitions; he would also submit the following definitions of sound, light, and heat, the former of which has, however, been elsewhere declared to be incapable of definition,* as well as by the reviewer (p. 22). Sound may be defined to be the impression produced by certain vibratory movements of matter upon appropriate sensuous organs, including both the receptive and perceptive apparatus. Whether the tympanum be incapable of receiving sonorous vibrations, or whether it vibrate sympathetically while the structures of the internal ear are incapable of appreciating its vibrations, there can be no sound.

And why may not the same definition apply to light and heat? It is, in fact, far from improbable that the perceptions of light and heat may result from the impressions produced by the same identical vibrations on different receptive organs; that of light on the eye and its nervous appendages, that of heat on the organs of common sensation. In common parlance, the terms sound, light, heat, will doubtless continue to be applied indiscriminately to the vibratory motion producing, and to the impressions produced; and to this there can be no objection, provided no advantage be taken of the misnomer to found thereon an assumption of the identity of the proximate cause and the resulting effect.

The reviewer has sought to kill two birds with one stone, and has made a vigorous onslaught against the conservation of energy in general, and the writer in particular, regarding the theory of latent heat; but it may reasonably be questioned how far "sound philosophy" is shown in attempting to convict an author of admitting an insuperable difficulty in the adoption of a given principle by quoting his statement of the difficulty, and coolly suppressing his immediately subsequent explanation of it. He thus quotes the writer†:—"Latent heat has ever been held up as the great stumbling-block of the dynamic theory, because it is impossible to conceive motion to be reduced to a state of quiescence, but remaining still ready to start again into action."

But instead of continuing the quotation thus:—"This, however, is merely a confusion of ideas, the fact being that when any substance passes from the solid to the liquid, or from the liquid to the gaseous form, a certain portion of the impressed heat-force is continuously occupied in overcoming molecular attraction, and thereby effecting the change of form; and this heat cannot be imparted to other bodies so long as it is occupied in maintaining that change," he ventures to state:—"In this we quite agree, and hence we think it unnecessary to give Mr. Brooke's reasons for believing a doctrine which he allows to be inconceivable"!!! Whether "sound philosophy" or not, is this common literary honesty? What Mr. Brooke allows to be inconceivable is obviously not the doctrine itself, but the conclusion drawn from fallacious arguments adduced in opposition to it; for to assign reasons for believing what one allows to be inconceivable would be nothing less than pure and simple evidence of mental alienation.

The fact is that the term "latent heat" is an unfortunate one, and has mystified the reviewer, as well as many others. It ought long ago to have been consigned to the limbo of discarded hypotheses, together with the material or corpuscular theory of heat from which it arose. If heat consisted of material particles, it might be supposed to become latent among the molecules of grosser matter, just as a handful of shot, if dropped into a box full of marbles, would lie hid amongst them, and be lost to sense, and would so remain until shaken out again; but mere vibratory motion cannot be theoretically dealt with in this fashion.

A much better term would be *engaged* or *occupied* heat, for the so-called latent heat is wholly engaged or occupied in maintaining the change—first from the solid to the fluid state, and secondly from the fluid to the gaseous. The facts are very plain; a pound of water at the temperature of 0° C., or the freezing point, mixed with a pound of water at 79° yields two pounds at the mean temperature of 39·5°; but a pound of ice or dry snow at the temperature of 0° mixed a pound of water at 79° yields two pounds of water at 0°, because the 79° of sensible heat in the water are now employed or occupied in maintaining such an amount of vibratory motion in the molecules of the ice, that they are no longer able to obey that polar attraction by which they were previously aggregated together in given directions so as to form crystals (for though not so evident in ice, the crystalline character of snow is notorious), and the heat-energy, being thus occupied in doing work, is incapable of doing any other work, as for example on the organs of sensation, at the same time; on the principle that you cannot "eat your cake, and have it too." The same reasoning applies to the change from the fluid to the gaseous state; but in this case a much larger amount of thermic energy is employed in so far removing the molecules from the sphere of each other's attractions that the balance of their mutual forces is repulsive, and so long as that repulsion is maintained, the dry steam manifests all the properties common to the fixed gases. "Latent" heat, then, when properly understood, ceases to be a "stumbling-block to the dynamic theory of heat."

One finds oneself occasionally brought by circumstances into an unwelcome generalisation. Thus the reviewer, speaking of the supporters of "conservation" in the lump, says (p. 21) "they take it for granted that force is motion and nothing but motion." This the writer entirely and absolutely denies. The reviewer, then immediately preceding his observations on the writer's views, quotes from Prof. Bain that "Inert matter in motion is force under every manifestation." This is plainly an abuse of language, in which the writer, as one of the "they," declines to participate; inertness and force are hardly concomitant ideas, and matter, whether in motion or at rest, is assuredly not force. The term heat-potential adopted by Mr. Rankine is admissible only in relation to heat as previously defined; the thermic energy can hardly be termed potential while it is employed in doing work.

The reviewer (p. 19) quotes, and objects to, the explanation of latent heat offered by Prof. Tait: that while sensible heat is *motion*, latent heat is *position*. The writer must acknowledge his inability to derive any definite idea from this statement of Prof. Tait; he cannot therefore express either assent or dissent.

The writer must plead guilty to having made use in the treatise above referred to, in common with many others, of a phrase which is not strictly accurate, viz., that sound, light, heat, and electricity are modes of motion. It would be more exact to state that they are so many forms of energy, or effects due to matter affected by certain modes of motion.

It is rather surprising that the reviewer should have ventured to dogmatise on the very slender knowledge either of physical facts or hypotheses that he evidently possesses. Thus he states (p. 33, note) regarding the investigations of Dr. Joule:—

"By means of machinery a weight of 772 lbs. is made to turn a small paddle-wheel placed in one pound of water. Dr. Joule found that the descent of the weight with a given velocity through one foot raised the temperature of the water exactly 1° F." Now this small sentence contains a curious concatenation of errors. Dr. Joule never employed a weight of 772 lbs., for the weights he employed were all under 30 lbs.; he never employed the energy acquired by his descending weights in stirring an exact pound of water, for the stirring vessel was not constructed to hold that particular quantity; neither did he find that the descent of the weight with a given velocity raised the water exactly 1° F., for the velocity of descent has nothing whatever to do with the result; since precisely the same amount of energy is acquired by a body in descending through the space of one foot by the action of gravitation, whether one minute or one month were occupied in the descent. For the sake of convenience, and in order to avoid a source of error, it was desirable only that the weights should descend slowly, and with a tolerably uniform velocity.

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* "A logical definition of sound is impossible." Dr. M'Cann on "Force and its Manifestations," a paper lately read before the Victoria Institute.

† Elements of Natural Philosophy (p. 786).

So much for the reviewer's knowledge of facts; now as to his knowledge of theories. He writes (p. 31):—

"The supporters of the doctrine of the indestructibility (? conservation) of energy have adopted a method the reverse of scientific. They start with the assumption of perpetual motion by means of transformation. In order to make facts fit their hypothesis, they take for granted that heat, light, electricity, and magnetism, are modes of motion, but not requiring a material basis to account for their phenomena. Some, however, seem to be aware that motion of necessity implies something moving, and that this something must be matter in some of its forms, and that consequently it is a great mistake to suppose that the dynamical theory is inconsistent with the materiality of heat. Finding that they have been a little too hasty in getting rid of the old imponderables, they are now quietly bringing them back under a new name, hoping, doubtless, that their few remaining friends may not be able to recognise them. Instead of the 'imponderables' we now have the 'luminiferous ether' which fills stellar space, and even permeates all the grosser forms of material existence."

This sentence expresses a gross misrepresentation of the course of philosophic thought. Can the reviewer point out a single physicist who for a moment doubts that "motion of necessity implies something moving, and that this something must be matter in some of its forms;" it is an axiom that not "some" but all must obviously admit. When it was supposed that light and heat consisted of material particles projected with immense velocity from their radiant sources, and that electricity and magnetism were "fluids" travelling with similar velocity, it might be assumed that inter-stellar space is an absolute vacuum; but when the progress of physical knowledge developed phenomena which were partially or wholly incapable of explanation on this hypothesis (such as diffraction and interference, and subsequently those of the polarisation of light and heat); but which became perfectly intelligble on the hypothesis that these forms of energy consisted in vibratory molecular motion transmitted with the same great velocity, the existence of a highly attentuated and elastic medium as the denizen of infinite space, became a necessary part of the theory; and this, in unavoidable ignorance of its precise nature, was termed "ether." Hence, in direct opposition to the reviewer's statement, physicists take for granted that "light, heat, electricity, and magnetism" do require a "material basis" (that is, matter as a means of their transmission) "to account for their phenomena;" and his dictum about physicists getting rid of the old imponderables, and now bringing them back again, is unmitigated nonsense.

The attribute of imponderability has been ascribed to the hypothetical substance "ether" by many physicists who hold that it monopolises the property of transmitting the waves of light and heat, and is therefore intersticially deposited in all kinds of matter. The writer is, however, more inclined to believe, with Mr. Justice Grove, that all kinds of matter are susceptible of these vibratory motions, and hence that the hypothesis of *interstitial* ether is gratuitous; his reasons for such opinion being elsewhere in print, need not be here repeated.*

It may be further suggested that for all that is positively known to the contrary, all kinds of matter may possibly be susceptible of a fourth state or condition, which may be termed the "ethereal," and which in tenuity and elasticity may be as far beyond the gaseous, as the gaseous is beyond the fluid state; possessing also the mechanical properties of a jelly, rather than those of a gas.

The writer can hardly be expected to take up the cudgels for others against the reviewer; but in concluding the remarks on this point, it may be observed regarding the reviewer's emphatic denunciation of an alleged descrepancy between Grove and Tyndall, that the discrepancy exists only in his own misunderstanding the quotation from the "Correlation of Physical Forces," viz., that "it requires no great stretch of imagination to conceive light and electricity as motions, and not as things moving," in which the writer clearly contrasts the undulatory and corpuscular theories, as commonly understood. Enough has now been advanced to show that the reviewer need not look very far from home for a conspicuous example of that which he has so freely attributed to the unhappy physicists, namely that they do but "darken counsel by words without knowledge" (p. 23).

The equivalence of dynamic and thermic energy is the only one that has as yet been determined quantitatively. He must be a bold man who denies that the sun shines at noonday; and scarcely less audacious is the assertion of the reviewer that the experiments of Dr. Joule do not confirm this equivalence. Dr. Joule conducted four distinct series of experiments, three series on the amount of thermic energy produced by molecular friction in stirring respectively water, oil, and mercury; the fourth, on that produced by the friction of two iron surfaces against each other. The four numerical results accorded very nearly, and after assigning to each result its weight, according to its estimated liability to error, he deduced the mean value of 772 foot-pounds as the dynamic equivalent of thermic energy.† In the metrical system, in which the units of quantity are one kilogramme, one metre, and one degree in the centigrade scale, the above equivalent is represented by 424 dynamic units, which, for brevity's sake, we may as well agree with the French in calling "dynams."

The reviewer, in ignorance probably of the amount of labour bestowed on this subject, seems to imagine that by ignoring Dr. Joule's results, he has demolished the basis of thermodynamics; but if so, he is grievously mistaken. It is a remarkable and unprecedented confirmation of this theory, that the numerical results arrived at by three distinct methods of investigation, in the hands of as many independent physicists, should be found to agree within very narrow limits of error.

It has been found by experiment that a less amount of heat is required to raise a gas maintained at a constant *volume* one degree of temperature, than when the gas is allowed to expand under a constant *pressure*. Suppose, for example, that the gas be enclosed in a vertical cylinder under a piston of 100 square inches area, the atmospheric pressure on this piston will be 1,500 lbs., and the raising this piston is equivalent to raising a weight of that amount. Dr. J. R. Mayer, assuming that the difference in the quantities of heat in the two cases above mentioned is equivalent to the work done by the expanding gas, proceeds to determine the numerical value of these equivalent quantities. Taking the specific heat of air to be 0·267, as at that time determined by the observations of De la Roche and Berard, he found the dynamic equivalent of an unit of thermic energy to be 367 dynams. But if, in the calculation of this number, the more careful and accurate subsequent determination of the specific heat of air by Regnault be substituted, namely, 0·2375, the result gives as the equivalent 426 dynams; a result almost identical with that of Dr. Joule, but based on purely theoretical considerations.

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* Lecture on Force and Energy, delivered at the Royal Institution, *Medical Times and Gazette*, July 8, 1871.

† For the sake of those readers who are not already familiar with this subject, we may state that a foot-pound is the amount of energy acquired by a weight of one pound in descending through the vertical space of one foot, or, in other words, the amount necessary to raise one pound one foot; and the numerical equivalent here given means that 772 dynamic units are equivalent to the amount of thermic energy required to raise the temperature of one pound of pure water, at or about the mean temperature of the air, one degree of Fahrenheit's scale.

Subsequently M. G. A. Hirn* pursued a course of observations exactly the reverse of that of Dr. Joule, namely, to determine the amount of heat converted into work in the steam-engine. Taking it as an axiom, which he showed to be in strict accordance with analytical investigations, that the difference between the heat existing in the steam as it enters the cylinder and that remaining in it after its exit, must be the thermic equivalent of the work done in and by the engine (which difference in the best constructed engines amounts to about 5 per cent. of the total heat due to the combustion of the fuel), he determined the value of one thermic unit to be 425 dynams; a remarkable result, and intermediate between those previously obtained by Mayer and Joule. In the face of such overwhelming concurrent evidence will even the reviewer be still bold enough to assert that the conservation of energy is a myth?

The principle of the dissipation of energy, as a corollary to that of its conservation, is of course equally ignored by the reviewer; but as his remarks on that point have no relation to anything beyond his own inner consciousness, it must here suffice to give a familiar illustration both of the conservation and the dissipation of energy, in the action of the rifle-ball. This reaches the target with less velocity, and consequently with less energy, that it possessed on leaving the muzzle; a portion of its energy has been expended in producing heat by friction against the particles of air between which it passes, which is dispersed through the surrounding atmosphere, and thus becomes *dissipated*. On reaching the target the progressive motion of the mass is arrested, and converted into molecular motion, which is cognisable only as heat, by which the mass is reduced to the fluid state, and splashes of molten metal are scattered in all directions. These again impart their heat partly to the air through which they pass, partly by radiation into space, and partly to the ground on which they fall; and thus the whole energy of the ball becomes dissipated. An analogous explanation will apply to all other cases of the dissipation of energy; thus the principle may be indefinitely illustrated, but it is hoped that it has now been sufficiently established that the conservation of energy is a fact, not a "heresy of science," the reviewer's opinions to the contrary notwithstanding, for

A man convinced against his will
Is of the same opinion still.

Want of space forbids the consideration of the larger cosmical relations of the conservation of energy to both organic and inorganic existence; one proposition only shall be alluded to as having been by some writers rather overstrained, viz., that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity. This may be taken as a postulate, and is probably true, but it is a proposition that is equally incapable of proof or of disproof, because the amount of potential energy in a body can be determined only by its development into actual energy, and cannot therefore be predicated. For example, two stones of equal weight lie one at the bottom of a well, the other on the ground at its edge; both are raised and placed side by side on the top of the windlass frame, much more work has been done on one than on the other, but can any one predicate in which of the two the greater amount of potential energy resides? Or is there any conceivable difference in the amount of energy acquired by either, otherwise than as dependent on its descending through the greater or the lesser distance?

It may, in conclusion, be remarked with much regret that the principle of the conservation of energy has by some been misapplied in a fruitless endeavour to supersede the necessity of a creative intelligence. To the mind of the writer, and, it is earnestly hoped, to that of most of his readers, the indisputable establishment of this principle conveys only a more exalted idea of that infinite wisdom by which the perpetually recurring transformations and interchanges, not only of the materials, but also of the powers, of Nature are rendered subservient to predetermined laws, which govern the comfort and welfare of all created beings.

CHARLES BROOKE

SPECTROSCOPIC NOTES*

ALTHOUGH P. Secchi and others have recently published descriptions of the different varieties of solar prominences which have been observed, well illustrating the many forms in which these outbursts from the sun's chromosphere occur, a careful record of such disturbances as may be out of the more common order may in the end assist us to a further knowledge on this subject.

While looking for bright lines near F in the spectrum of a low prominence situated 25° north of east, at 11.35 A.M. (April 2), my attention was called to what appeared to me a sudden displacement of the F line toward the violet end of the spectrum.

I immediately brought the C line into the field of view, in order to discover if any change was taking place in the form of the prominence, but was wholly unprepared for the spectacle which met my eye. Upon widening the slit I found that where only twenty minutes before there had been a comparatively low mass of prominence matter not exceeding 50″ in height, and remarkable only for the brightness of a jet issuing from the southern portion which was but slightly raised above the chromosphere, there had been an eruption of matter on a grand scale.

Fig. 1 represents the prominence as first seen at 11.15 A.M. At 11.35 A.M. the northern portion had entirely disappeared, and from the low mound, at the point where the jet mentioned above had been seen, an eruption had taken place far exceeding anything I have ever witnessed. Far above the chromosphere the air was filled with long wisps of glowing hydrogen, ranging from 20″ to 50″ in length, with the appearance of having been ejected in quick succession. Above them floated detached masses, in the form of thin fleecy clouds, and the highest point reached by these was fully five minutes of arc, or about 135,000 miles above the sun. In the mound appeared a low, sharp horn, exceeding in brightness any other portion of the prominence. The grandeur of the eruption lasted but a few minutes, gradually fading in brightness and diminishing in size.

Fig. 2 gives a correct idea of the general form and extent of the prominence as seen at 12 M.

When next observed at 1.40 P.M., nothing was left but a small cloud mass of about the height of the prominence when first seen, at 11.15 A.M. To the north of this, however, were two very small horns, of great brightness. It has generally been noticed that the appearance of these bright points in the chromosphere is a forerunner of increased activity. The forces at work beneath were only gathering strength for a final outburst, which, if not so great in extent, proved to be equally magnificent. I had not long to wait; for after a few minutes spent in examining the F line, just as the clock was striking the hour, I again brought C into the field, and found myself only in season for the grand *finale*. The chromosphere had again belched forth, and far above the sun could be seen the ejected matter, reaching to the height of 94,500 miles. The *débris* was in the form of jets or wisps, which appeared to be falling toward the chromosphere, here and there fretted with sharp horns or bristles. The remains of the first eruption existed unchanged since last seen, at 1.40 P.M., but the changes in the form of the new one were rapid; at one point the filaments were so interlaced as to resemble close network. In the more elevated portions the jets soon lost their brilliancy, and in less than an hour scarcely anything remained but a few stray wisps floating low down near the chromosphere.

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* Cosmos, v. 22, 1863.

* Reprinted from advance sheets of the Journal of the Franklin Institute furnished by the Editor.

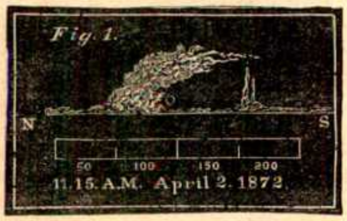


Fig. 1.

11.15 A.M. April 2. 1872.

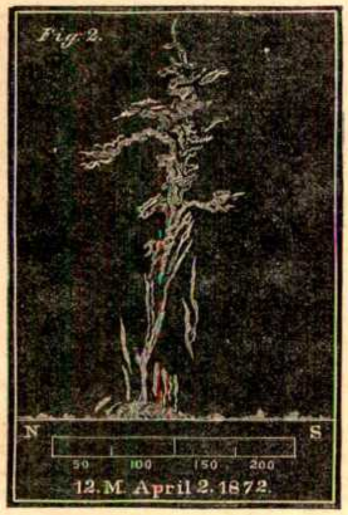


Fig. 2.

12. M. April 2. 1872.

Fig. 5 exhibits the change which the last outburst had undergone at 2.20 P.M.

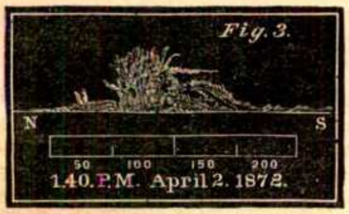


Fig. 3.

1.40. P.M. April 2. 1872.

During the phenomena the C and F lines were completely broken up, being displaced toward both the red and violet ends of the spectrum, the greatest displacement being toward the violet, in the F line extending to the iron line above F numbered 2082 in Kirchhoff's map. The D_3 line also suffered a sensible displacement. I examined the magnetic needle during the outbursts and an hour afterwards, but could detect no unusual disturbance. In the evening there was a slight auroral display.

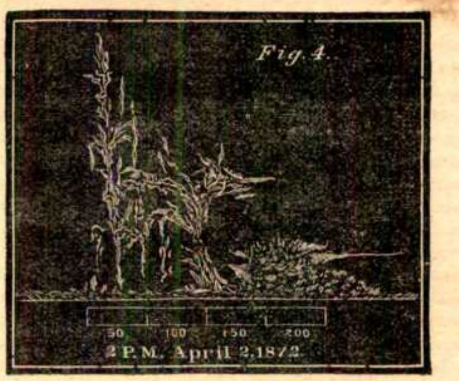


Fig. 4.

2 P.M. April 2, 1872.

The portion of the chromosphere in which the eruptions took place was in the neighbourhood of a group of spots which were just making their appearance upon the eastern limb of the sun. The spots were completely surrounded by faculæ which radiated from the spots themselves, and could be traced to the sun's edge. Fig. 6 shows the position of the spots upon the sun at the time of the eruptions. The sketches were made at the time of observation, and are as correct as the duration of the phenomena would allow, the general form and most of the details being preserved.

JOHN H. LEACH

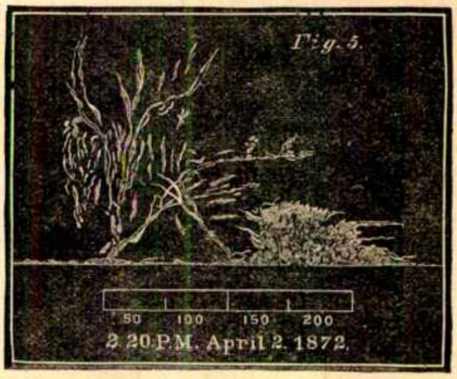


Fig. 5.

2.20 P.M. April 2. 1872.

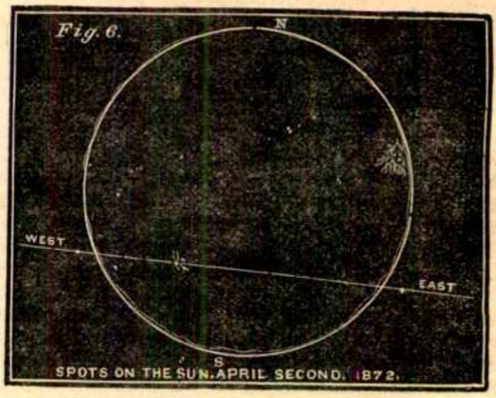


Fig. 6.

SPOTS ON THE SUN. APRIL SECOND. 1872.

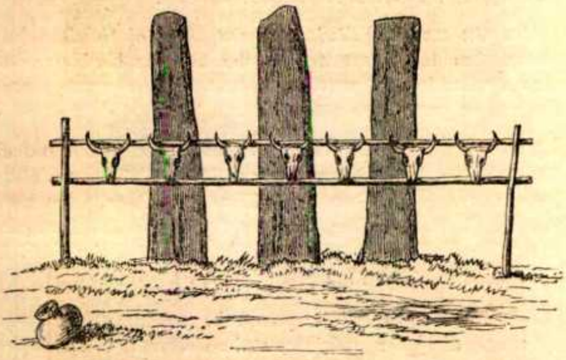
ANCIENT AND RECENT STONE MONUMENTS

IN Mr. Fergusson's new book on Ancient Stone Monuments, mention is made of the Menhirs and Dolmens, both ancient and modern, which are found in the Khasia Hills, North-Eastern Bengal.

Having been for some time engaged in the survey of these hills, I can furnish a few particulars concerning these monuments which may be of interest. On one occasion, returning to my camp after a day's work, I was startled by hearing a loud shouting as of a number of men exerting their strength together and getting the time by shouting in chorus, much as sailors do on board ship.

I found that the sound proceeded from an assembly of Khasias, who were putting up three of these menhirs to the memory of a deceased villager. They were at a considerable distance from me, so that I could not clearly see their mode of procedure, and as on occasions of funerals and the ceremonies connected with them the Khasias are invariably more or less drunk and unruly, it would have been inexpedient to have gone amongst them. I was therefore compelled to wait until the next morning, when I went and inspected the scene of operations.

I found that three menhirs of no very great size had been put up, and that the stones had been raised in a very simple way by the use of long levers formed of young trees and ropes made of an exceedingly tough kind of creeper found in abundance all over these hills. The whole affair had been made the occasion of a feast on a very large scale; bones of slaughtered cattle, and empty grog jars lay around in numbers; the skulls of the oxen (some fourteen or fifteen in number) being arranged in a very fantastic way before the menhirs. As the arrangement of these skulls at once suggested to my mind the probable origin of a well-known architectural ornamentation, I attach a sketch showing how they were placed, viz., in a row at even distances apart on two horizontal poles, which were themselves supported on two upright poles.



I was at some pains to inquire the meaning of these menhirs, but could only gather that they were intended to perpetuate the memory of some local celebrity.

In the case of the dolmens, so often found put up in front of the menhirs, I was informed that they served to give a kind of rough shelter to the ashes of the deceased, these ashes being kept for a year or two in the house and then brought out and scattered under the broad flat stone of the dolmen.

Moreover, the Khasias use these dolmens in their extraordinary form of divination by the breaking of eggs. This divination, which may be said to form their nearest approach to a religious worship, and which is on that account of peculiar interest, is conducted as follows:—On the top of the dolmen the Khasia who officiates puts five little pellets of clay and chewed betel nut, in the form of a semicircle, thus—

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Then he stands upon the stone, and commences a wild kind of chant, having a rhythm and intonation utterly different from that of their ordinary songs. At a certain period in this chant he draws an egg from his pouch, and dashes it down on to the stone, as near the centre of the semicircle as he can. If the mass of the yolk scatters towards and over the pellets the omens are propitious, each pellet of the five having its meaning; but if the yolk scatters away from the semicircle of pellets the prognostication is unfavourable.

In his book Mr. Fergusson seems to suppose that the Menhirs and Dolmens mark places of assembly; but this is not the case, for near almost every large and old-established village there is found a place of assembly provided with stone seats, often prepared with much labour, and well adapted to the purpose for which it is intended.

I would also demur to Mr. Fergusson's inference that the Khasias are a physically inferior race; on the contrary they are a race possessing muscular strength to a singular and exceptional degree, as witness the fact that it is a very ordinary feat with them for one Coolie to carry a full-grown man in a kind of chair strapped on his back, along a road more than eight miles long, and ascending upwards of 4,000 feet. M. T. SALE

DISCOVERY OF EXTINCT MAMMALS IN THE VICTORIA CAVES, SETTLE

THE scientific public will be pleased to hear that the Committee who have been exploring the Settle caves for two or three years past, have at length met with a great success. Till within the last fortnight we had discovered only remains of different ages from the Neolithic period to the present. These, though of great interest as throwing light on the vicissitudes and succession of later races, had to a certain extent been forestalled by the previous researches in this district of Mr. James Farrer, of Ingleborough, and by Mr. Jackson, the original discoverer of the Victoria Cave, and present superintendent of the work carried on in it by the Committee. Those remains were all in comparatively recent deposits. Beneath them was a great thickness of barren ground, consisting of a laminated clay in some places twelve feet thick, and below that again a great accumulation of angular fragments of limestone in a matrix of clay.

At a depth of about twenty feet in this we have now found elephant, rhinoceros, hyæna, a crushed canine of a much larger carnivore, &c. The elephant's teeth found belong to a young individual, and the number of gnawed bones and other indications of the cave having been a den of some large carnivores render it probable that the elephant was dragged into it by them.

The facts have a special interest, from this older group of mammals not having been previously met with in this district. It is to be hoped that when we can investigate these important beds more thoroughly we may get some light thrown upon the relation of man to these extinct animals, and of both to the Glacial period, undoubted deposits of which occur in other caves hard by.

Great credit is due, amongst others, to Mr. Walter Morrison, M.P., and to Messrs. John Birkbeck, Sen. and Jun., who have spared neither time, trouble, nor expense, and that in face of many discouragements.

R. H. TIDDEMAN

NOTES

THE belief in the safety of Dr. Livingstone caused by the telegrams which we published a fortnight ago, has been strengthened by further intelligence received since our last issue. The following telegram was to hand in London on Saturday last by the Falmouth, Gibraltar, and Malta Telegraph Company:—"Kirk reports from Zanzibar Livingstone safe at Unyanyembe. Visited north end of Tanganyika. Rivers said to flow into Lake Tanganyika. Stanley near the coast with letters. –GOVERNOR." No date is attached to the telegram; but at the meeting of the Royal Geographical Society on Monday evening, Sir Henry Rawlinson stated that he considered all doubt now to be removed, and that the intelligence now in hand is really authentic. He laid great stress on the well-known cautiousness of Dr. Kirk in receiving and communicating information respecting Dr. Livingstone. The following despatch from the Sultan of Zanzibar to Sir H. Rawlinson was read at the meeting:—"In the name of the Most Merciful God.—To our esteemed friend, Sir Henry Rawlinson. May the Almighty preserve him in health and happiness.—Your friend is quite well, and the object of our letter is to inform you that at the auspicious moment of our safe return from performing the pilgrimage to the holy cities of Mecca and Medina, my friend the Consul called on me and presented to me Lieutenant Dawson and his companion, and at the same time he delivered to me the letter from Her Majesty's Secretary of State for Foreign Affairs, Earl Granville, and also the gift presented by the Royal Geographical Society, through the President. And it has pleased me much to do that which is considered advisable, and that I am enabled to aid the people in their search for my friend Dr. Livingstone, and I pray God that certain information regarding him may soon be received, and I will give my aid to those gentlemen whom you have sent in attaining their object. And the Consul having requested me to grant the use of my steamer to the above gentlemen to convey them to Mombassa to procure men to accompany their expedition, I have done so, and, please God, I will continue to render assistance to those whom you have sent in your endeavour to explore the mysterious regions of the unknown country, because their object is praiseworthy, and tends to increase our knowledge of what the Almighty has created in these our countries.—From your friend, BURFASH BIN SAYYID.—Dated Zanzibar, the 5th day of Safr, year 1289 of Hejira, corresponding with the 14th April, 1872." Further telegraphic intelligence may be expected at any moment, and should this arrive we purpose next week to resume the present state of our knowledge respecting Dr. Livingstone's safety.

WE learn with great regret, from the scientific editor of *Harper's Weekly*, of the death of Dr. William Stimpson, late Secretary of the Academy of Sciences of Chicago. Dr. Stimpson's health has been quite precarious for several years past, making it necessary for him to proceed every winter to the warmer climate of Florida, and the past winter was spent by him in the same region. He was engaged in the earlier part of the season on board the United States Coast Survey steamer *Bache*, in superintending a series of dredgings, which, however, he was compelled to abandon through increasing ill-health, and returning not long since to the residence of his father-in-law, near Baltimore, he became gradually worse, and died there on the 26th of May. For a time a pupil of Prof. Agassiz at Cambridge, Dr. Stimpson made his first mark as a scientific author in 1851, in a work on the shells of New England, which was soon followed by a paper on the marine invertebrates of Grand Manan, published by the Smithsonian Institution in 1853, and which is still a standard work on the zoology of the mouth of the Bay of Fundy. Shortly afterwards he was appointed zoologist to the North Pacific Exploring Expedition, in which he was occupied for several years. When the late Mr. Robert Kennicott went to Alaska, in 1865, in the service of the Russian Telegraph Expedition, Dr. Stimpson moved to Chicago to take charge of the general affairs of the Chicago Academy of Sciences, and maintained that connection until his death. As a scientific investigator Dr. Stimpson occupied a very high rank for the thoroughness of his researches and the clearness and accuracy of his descriptions, in these respects leaving nothing to be desired. No one, with the exception, perhaps, of Prof. Dana, has described so many new species of marine animals as he. The detailed accounts of his new species, forming a large number of valuable zoological monographs, with large numbers of illustrations, and nearly ready for publication, were unfortunately all destroyed by the Chicago fire, together with most of the types of his species—a calamity which of course affected him severely, and in all probability materially influenced the state of his health. Among these works were synopses of the mollusca of the East coast of North America, and of the crustacea of both coasts, to be published by the Smithsonian Institution.

IT is rumoured that the dignity of K.C.B. is about to be conferred upon Mr. G. B. Airy, Astronomer Royal, and President of the Royal Society.

THE Albert medal of the Society of Arts has been awarded by the Council this year to Mr. Henry Bessemer, "for the eminent services rendered by him to Arts, Manufactures, and Commerce, in developing the manufacture of steel." The Conversazione of the Society will be held on Wednesday, June 19, at the South Kensington Museum.

MR. CHAMBERS, F.R.S., Superintendent of Colaba Observatory, has been presented by the Bombay Harbour Board with a valuable gold watch and chain for his service towards the port in putting up a time ball.

THE examiners in the Natural Science School at Oxford, Dr. W. Ogle, Mr. R. H. Bosanquet, and Mr. A. W. Reinold, have issued the subjoined Class list:—Class I.: Henry Cooper, All Souls'; J. P. Earwaker, Merton; C. J. Moullin, Pembroke. Class II.: H. Green, Queen's College. Class III.: E. C. D. Fox, Exeter. Class IV.: J. A. Lloyd, St. John's.

THE following ladies have passed the examination for special certificates of higher proficiency at the University of London:—In Mathematics and Mechanical Philosophy, Mary Stewart Kilgour, Ladies' College, Cheltenham; in Geology and Palæontology, Laura Gertrude Eaton, Ladies' College, Cheltenham; in Political Economy, Jane Ellen Harrison, Ladies' College, Cheltenham; and in Harmony and Counterpoint, Mary Amelia Bennett, North London Collegiate School for Ladies.

PROF. HUMPHRY, F.R.S., will commence his Course of Three Lectures on Human Myology at the Royal College of Surgeons on Monday, June 17, at 4 P.M. They will be continued on Wednesday and Friday at the same hour. The lectures will discuss the morphology and teleology of the muscular system of man.

A TELEGRAM from Madras, printed in the *Times*, states that a court of inquiry is being held, with closed doors, on the conduct of Mr. Pogson, the Government Astronomer, who seems to be held responsible for the damage done by the late disastrous cyclone. Whether it is thought that he could have prevented the cyclone does not appear.

REPORTS have been received from Prof. Agassiz and his party on the *Hassler* up to the 18th of March, at which time they had reached the coast of Patagonia. They were busily engaged in dredging and carrying on explorations along the shore, with a very fair measure of success. The details have not yet been announced, but we trust we shall have an opportunity before long of presenting these to our readers.

THE Annual Report of the Trustees of the Museum of Comparative Zoology at Cambridge, U.S.A., for 1871, has made its appearance, and presents the usual satisfactory account of progress in the preceding twelve months. No institution of the kind in America, and few anywhere, has so extensive and thoroughly organised a corps of scientific assistants (amounting to between thirty and forty) as that at Cambridge; and, with the immense amount of material constantly coming in, the result in greater part of Prof. Agassiz's indefatigable personal labours, supplemented by purchases of entire collections, it is not to be wondered at that the museum is rapidly occupying the foremost rank among such establishments. Prof. Agassiz, the director, calls attention to his expected absence from the country in the expedition of the *Hassler*, gives an account of the arrangements made temporarily to supply his place, and presents the special reports of the various assistants upon the work accomplished in 1871, and to be continued during the year 1872.

AMONG the more interesting collections lately received by the Smithsonian Institution at Washington, in the department of ethnology, is a mummied human head, retaining all the form and features of life, including hair, lips, &c., but reduced by some peculiar process so as not to exceed the size of an ordinary fist. These heads are found among the Javaro tribes in the province of Chimborazo, in Peru, and are said to be of great antiquity, there being no indication of recent preparation. They are believed to be the heads of enemies slain in battle, and preserved in this way as trophies of victory. The interior of the head has been entirely emptied of flesh, bones, and brain; and the skin, which alone remains, by its contraction is thickened to the amount of more than an eighth of an inch. The lips are closely compressed, and through them are strung a series of knotted cords, which in their character call to mind the guipos of the ancient Peruvians. There is also a cord which is knotted inside the top of the head, by which it is suspended. No satisfactory explanation of the mode of preparation has been given, although there is a tradition that it is effected by introducing heated stones or sand into the cavity after the removal of the portions of the head referred to.

THE Transactions of the Norfolk and Norwich Naturalists' Society for 1871–72 show evidence of good work being done by its members. The president, in his address, speaks of the satisfactory progress made in the important task which the Society has in hand, of compiling well-authenticated, and, as far as possible, complete lists of the fauna and flora of the county; and the following papers are printed in the Report—some of them, although referring to local subjects, of more than local interest:—"On the Occurrence of the Ringed or Marbled Seal (*Phoca hispida*) on the Norfolk Coast," by T. Southwell; "Scoulton Gullery," by H. Stevenson; "The Norfolk Broads and Meres Geologically Considered," by J. E. Taylor; "Further Notes on Coast Insects found at Brandon," by C. G. Barrett; "The Marine Mollusca of the Norfolk Coast," by F. W. Harmer; "A List of Land and Freshwater Shells found in Norfolk," by J. B. Bridgman; and "On the Spongeous Origin of Flints," by F. Kitton.

WE have received from the Royal Cornwall Polytechnic Society, Falmouth, a list of a large number of subjects in which prizes and premiums will be awarded in the course of the present season. The fortieth Annual Exhibition of this Society will open on Wednesday, August 21. Medals and prizes in money will be awarded in the following Departments:—Mechanics—Machinery and Models; Mechanical and other Scientific Inventions and Improvements; Specimens of Naval Architecture; Essays and Scientific Papers, &c. Fine Arts—Pictures and Drawings by Professional Artists and Amateurs, Sculpture, Architectural Drawings and Models, and Specimens of Ornamental Art. Photography—Photographs by Professionals and Amateurs. Natural History—Essays, Local Observations, Collections of Specimens, &c. School Productions—Mechanical and Freehand Drawings, Specimens of Penmanship, &c. Plain Needlework, &c., British Lace, and all objects of interest connected with Science and the Fine and Industrial Arts, which may be considered deserving by the Judges. List of Prizes and Premiums, and all further information, may be obtained from the Secretary, J. H. Collins, F.G.S., Polytechnic Hall, Falmouth.

SCIENCE and Art teaching seems to be flourishing in Plymouth, if we may judge from the number of papers worked at this year's examinations (just concluded) by the students of the Charles Science and Art Schools. Three hundred and thirty papers have been sent up in the various subjects, being an increase over 1871 of fifty-six papers, or 20 per cent. Owing to the Department holding the examinations in several subjects in one evening, students are prevented from being examined in more than one of these, which will, in most cases, account for the falling off in those few cases where there is a decrease.

MR. N. VON MACLAY, who is in charge of a Russian scientific exploration of the Atlantic and Pacific oceans, reports to the Academy of Sciences of St. Petersburg that on the passage of the *Witjas* from the Cape de Verde Islands to Rio he made an experiment on the 3rd of February for the purpose of determining the temperature of the sea at a depth of 1,000 fathoms in the region of calms, about 3° north latitude and 24° west longitude. The temperature of the water at this depth was 38·30° F., that of the surface water being 81·68° F. It is interesting to compare this with the temperature obtained during the past winter by the Coast Survey steamer *Bache*, at about the same depth, in the deep water between Cuba and Yucatan, in the latter case the temperature amounting to about 39·50° F.

IT seems almost impossible to exhaust the richness of the deposits of vertebrate fossils of the Western territories of the United States, Prof. Leidy having lately added to the number by the description of two extinct tapir-like animals, one about the size of a raccoon, and the other about the size of a rabbit, and an insectivorous animal of the dimensions of the hedgehog. The are from the tertiary formation of Wyoming Territory.

Harper's Weekly notes that a remarkable fact connected with the interchange of animal species between Europe and America is seen in the frequency with which North American birds occur in England, and the scarcity of European birds in America. Nearly seventy species of the birds characteristic of the American fauna have so far been detected in Great Britain, the latest announcement of this kind being that of the black-billed cuckoo, which was taken at the end of September 1871 in Antrim, ten miles from Belfast. Very few of the European land birds have been found in North America, with the exception of a few species that are really Arctic in their distribution, although less frequently seen in the New World than in the Old. The water-birds of Europe are more common as stragglers. Among them we may mention the English green-winged teal, the widgeon, the woodcock, &c. The entire list, however, does not amount to a dozen species. The causes of this difference are doubtless to be met with in the comparative prevalence of certain winds. Most of the captures of American species take place in Ireland in autumn and early winter, and in all cases are species belonging to the northern portion of America which migrate southward at the close of the breeding season. At that time the prevailing winds are from the west, and the birds in their flight become confused, and are carried across by the winds, taking an occasional rest on passing vessels.

MR. J. COBBIN, of Durban, forwards to the *Natal Colonist* the following account of a "sea-serpent" seen by him:—"During my late passage from London, I saw no less than three sea-serpents, but an account of the last will suffice. On 30th December last, on board the *Silvery Wave*, in lat. about 35′ 0″ S., and long. 33′ 30″ E., at 6.20 P.M. solar time, an enormous serpent passing nearly across our bows compelled the alteration of our course. He was at least one thousand yards long, of which about one-third appeared on the surface of the water at every stroke of his enormous fan-shaped tail, with which he propelled himself, raising it high above the waves, and arching his back like a land-snake or a caterpillar. In shape and proportion he much resembled the cobra, being marked by the same knotty and swollen protuberance at the back of the head on the neck. The latter was the thickest part of the serpent. His head was like a bull's in shape, his eyes large and glowing, his ears had circular tips and were level with his eyes, and his head was surmounted by a horny crest, which he erected and depressed at pleasure. He swam with great rapidity and lashed the sea into a foam, like breakers dashing over jagged rocks. The sun shone brightly upon him; and with a good glass I saw his overlapping scales open and shut with every arch of his sinuous back coloured like the rainbow."

ABNORMAL DISPERSION OF BODIES WITH SURFACE-COLOURS[*]

THESE memoirs relate to the abnormal phenomena of dispersion produced by certain bodies which reflect the rays of some colours in the manner of transparent media, and those of other colours in the manner of metals.

In transparent bodies the velocity of light is less than in a vacuum, and the index of refraction increases as the length of undulation diminishes, whereas, in the case of metallic bodies, theory leads to essentially different results. From the researches of Jamin on the elliptic polarisation of light reflected by metals, and from the more recent experiments of Quincke, it appears that the formulæ of Cauchy, Beer, and Eisenlohr give for certain metals, as gold and silver, an index of refraction less than unity, whence it follows that in these metals the velocity of light is greater than in the vacuum. Cauchy's theorem, verified by the experiments of Jamin, likewise shows that the index of refraction in metals varies with the angle of incidence, and that in most metals dispersion takes place in a manner opposite to that which occurs in transparent bodies, that is to say, that the rays of shortest wave-length are less refracted than those whose undulations are longer.

The only physicist who has hitherto attempted a direct verification of these theoretical results is Quincke; but his experiments have yielded contradictory results according to the method employed, and have therefore afforded no decisive information as to the indices of refraction and dispersion of metals. Kundt, in studying the properties of a particular class of bodies which approach the metals in their optical properties, has succeeded, if not in determining the absolute values of their refractive indices, at least in observing certain anomalous phenomena of dispersion, which appear to indicate the direct road towards a new verification of the theoretical formulæ.

The bodies in question are media intermediate between transparent bodies and metals; inasmuch as they behave like metals towards rays of certain colours, and like transparent bodies towards rays of other colours; they also reflect light with a certain degree of metallic lustre. These substances may be included under the denomination of *bodies with surface-colours*. Most (but not all) of them are strongly tinctorial, and exhibit, both in solution and in small fragments, a beautiful coloured transparency; such are most of the aniline-dyes, indigo, carthamine, potassium-permanganate, &c.

The optical properties of these bodies have been studied by Brewster, Haidinger, Stokes, and several others, the chief result of whose researches is the law, first enumerated by Haidinger, that the light transmitted by these media is exactly or nearly complementary to the light reflected from their surfaces, and, therefore, to the colour of the surface. Moreover, Dale and Baden Powell observed that indigo and Prussian blue, in reflecting light, polarise it elliptically, like the metals; and von der Willigen determined the reflection-constants of pale indigo, and found that the principal angle of incidence diminishes from the line B to E, and then increases from E to G.

The author's views respecting these bodies are based on the hypothesis that they exhibit the most general case of dispersion, that, namely, in which the index of refraction may not only increase or diminish when the wave-length in air diminishes, but may even become less than unity.

The examination of the light reflected by these bodies is alone sufficient to lead to this hypothesis. For when a medium is transparent for a particular ray, the intensity of the reflected light is expressed by the formula $\left(\frac{n-1}{n+1}\right)^2$, the value of which increases or diminishes with the increase of n, according as n is greater or less than unity. In most transparent bodies the variation of n for the different colours,—that is to say, the dispersion—is so small, that the whole of the reflected light exhibits the same colour as the incident light, so that, if the incident light is white, the reflected light is white also. Applying this formula to bodies with surface-colours, it would follow that the rays which form the superficial colour, that is to say, those which are reflected in very great proportion, should have, relatively to the others, a very large or very small index of refraction.

As the rays which form the surface-colour may have any wave-length whatever, it would follow from the formula which gives the intensity, that the dispersion of bodies with surface-colours should be, or at least might be, abnormal, in a manner which is perfectly arbitrary. It might even happen that a portion of the transmitted rays, if made to pass with a sufficient intensity through a prism of the substance, would be refracted on one side of the direction of the incident rays, and the rest on the other side. In fact, for rays which are reflected in large proportion, that is to say, rays for which the bodies under consideration act like metals, the preceding formula is not applicable, and it is necessary to employ the formulæ given by Cauchy, in his theory of metallic reflection.

These formulæ give, for the normal incidence:

$$I = \tan\left(\psi - \frac{\pi}{4}\right)$$

where ψ is determined by the relation

$$\cot \psi = \cos \epsilon \sin 2 \tan^{-1} \theta,$$

ϵ and θ being two constants to be determined by experiment, according to the relations—

$$\theta \cos \epsilon = n\,; \quad \theta \sin \epsilon = \gamma,$$

n being the index of refraction, and γ the coefficient of extinction under the normal incidence.

According to these formulæ it is not absolutely necessary, as remarked by Cauchy, that the index of refraction of metals should be very large. Nevertheless it appears from the experiments of Jamin, and those more recently made by Quincke, that the constants of elliptic polarisation have values which indicate for most metals a very large refractive index, and for silver and gold an index less than unity.

Cauchy's formulæ, applied to bodies with surface-colours—admitting that the elliptic polarisation produced by these bodies is analogous to that produced by the metals—renders probable the existence of refractive indices either very large or less than unity: hence the probability of abnormal dispersion resulting from transmission through these bodies.

When one of these surface-coloured bodies is dissolved, it preserves in solution its abnormal properties relatively to dispersion. This abnormal dispersion will be combined with the

[*] By A. Kundt (Pogg. Ann, cxlii. 177; Journal de Physique, 1872, p. 38) nd J. L. Soret (Pogg. Ann., cxliii. 325; Journal de Physique, 1872, p. 45).

normal dispersion of the solvent; and even if no ray can then acquire an index of refraction less than unity, or be completely isolated from the rest, the succession of colours in the dispersion-spectrum of the solution will be sensibly different from that which is commonly observed.

The method employed by Kundt for the observation of abnormal dispersion in solutions of bodies with surface-colours (suggested by a memoir published by Christiansen of Copenhagen), is as follows:—A drop of the concentrated solution to be examined is placed on a glass plate, and over this drop is fixed, at an angle of about 25°, a sharp edge of a second glass plate of the same breadth as the first. At a small distance from the line of contact of the two plates, the liquid prism formed by capillary action is usually opaque; but close to the line of contact there is a prismatic layer of liquid, scarcely broader than a hair, which is transparent for most of the colours. The dispersion may be recognised by viewing through this layer a narrow and bright flame or an illuminated slit. The observations of course require a certain amount of practice, and the observer must be on his guard against deception arising from phenomena of reflection or refraction. The observation is generally made by carrying the liquid prism to the place of the glass prism in the spectroscope of Bunsen and Kirchhoff, the most favourable angle to give to the liquid prism being determined by trial.

Christiansen has observed that the index of refraction of a strong alcoholic solution of fuchsine increases from the line B to D, then diminishes rapidly as far as G, and increases again beyond that line; and the recent observations of Kundt, made as above described, have demonstrated the generality of this anomalous dispersion for surface-coloured bodies in the state of solution; that is to say, that in the dispersion-spectra of these solutions the order of the colours is not the same as in the solar spectrum, or in the dispersion-spectra of ordinary solutions. Such was found to be the case, not only with fuchsine, but with all specimens of aniline-blue and aniline-violet, with aniline-green (Hofmann's iodine green), indigo (dissolved in fuming sulphuric acid), indigo-carmine, carthamin, murexid (dissolved in potash), cyanine, potassium-permanganate, and carmine.

All the bodies in this list refract red more strongly than violet light; and in bodies for which the green forms the principal part of the surface-colour, and can be distinctly recognised in the transmitted spectrum, the green rays are the least deviated. Cyanine, aniline-violet, and aniline-blue, as well as indigo-carmine, give, therefore, in their dispersion-spectra the following series of colours: green, blue, red, the green being the least deviated. Cyanine is particularly well adapted for producing the abnormal spectrum, and exhibits the following series of colours: green, light blue, dark blue, a dark interval, red, and traces of orange. The dispersion varies however with the concentration of the solution; in dilute solutions all the bodies above enumerated exhibit normal dispersion. Potassium-permanganate and carmine exhibit reversed spectra only when their highly concentrated solutions are mixed with fine solid particles so as to form a sort of pulp, and the two glass plates are pressed strongly together. A change in the dispersion is also observed according to the angle of incidence.

Soret pours the solution under examination into a hollow prism having an angle of about 30°, and places this prism in a glass trough with parallel sides, filled with the liquid which serves as solvent. By this arrangement the reverse spectrum may be obtained with solutions less concentrated and therefore more transparent than when the liquid prism is merely surrounded by air.

If the ordinary prism of a spectroscope be replaced by a hollow prism filled with a concentrated solution of fuchsine, the reversed spectrum will be seen without the aid of the trough above described, provided the light is very strong and passes very close to the edge of the prism. With a less concentrated solution the spectrum is normal, and with a solution of intermediate concentration the spectrum is reduced to a single red line. In this case the anomalous dispersion due to the fuchsine is entirely compensated by the normal dispersion produced by the alcohol, and the result is deviation without dispersion. If now the prism containing this last-mentioned solution be immersed in a trough containing alcohol, the deviation of the rays produced by the alcohol will be almost wholly destroyed, while the abnormal dispersion of the fuchsine will remain, the red being more strongly deviated than the violet. With this arrangement it is no longer necessary to employ so strong a light, or to make the rays pass so close to the edge of the prism.

With the prism in air the deviation of the red rays is about 11° 30′; but when the prism is immersed in alcohol, the violet is scarcely deviated, the red by fifteen minutes, and the orange by twenty-three minutes.

Similar results were obtained with aqueous solutions of aniline violet and potassium-permanganate.

MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY[*]

(*Concluded from p.* 114.)

To Grisebach's notes on the connections of the tropical African flora with that of other countries I shall have but few observations to add. The intergrafting with the South African flora along the eastern side of the Continent may well be attributed to climate and other present physical conditions. The European character of the higher mountain vegetation of Abyssinia and the Cameroons may be indicative of the remains of that western flora, the mysteries of whose distribution north and south of the tropics I have on several occasions alluded to. The supposed evidences derived from the vegetable kingdom of a once existing connection between west tropical Africa and east tropical America through an ancient Atlantis gradually disappear on further investigation. No traces of a western Atlantic or American vegetation were met with by Mann in the mountains of Fernando Po and the Cameroons, nor by Dr. Hooker in the western Atlas of Morocco. The tropical American races found in Western Africa are chiefly confined to the coast region; they are more generally identical than representative species, and they may have been brought over in the course of ages by some of those means of transport which even now may occasionally occur, such as the Gulf Stream, as mentioned by Grisebach. You may recollect, for instance, a short notice by Dr. Dickie inserted in our Journal (Botany, vol. xi. p. 456) of a green floating mass, twelve to fourteen miles broad, crossed by Captain Mitchell in the Atlantic, within 300 miles of the mouth of the Gambia, which had evidently, as Dr. Dickie believes, come from some part of America within the influence of the Gulf Stream, probably passing between the Cape Verde Islands and the mainland of Africa. Besides algæ, the portions of this mass picked up by Captain Mitchell and examined by Dr. Dickie contained, amongst other substances, fruits, seeds and "seedling plants several inches long, all with a pair of cotyledons, roots, and terminal bud, quite fresh."[†] With regard to those American genera represented chiefly in eastern tropical Africa, to which I called your attention in my paper on Compositæ, there are various considerations, requiring too much detail for me now to enter upon them, tending to show a greater probability of an ancient interchange having taken place far south of the tropics, or eastward over lands long since submerged, than across the tropical Atlantic. A prevailing eastern element in the tropical African flora has, indeed, been frequently pointed out. An interchange with continental India is so well marked north of the equator as to have been generally admitted; but south there are many distinct types represented only in Madagascar, Ceylon, Malacca, the Archipelago, or Australia. This would lead one into speculations put forward also by naturalists in other branches as to a vast continent once bridging over the Indian Ocean, and extending even far to the eastward into the Southern Pacific. Similar views derived from zoology have been recently put forward by Grandidier in a most interesting sketch of the physical geography and natural history of Madagascar, contained in No. 46 (May 11) of this year's *Revue Scientifique*. This island, whose evident antiquity and long isolation, aided by its broken surface, has enabled it to become the seat or centre of preservation of a very large number of endemic monotypes, shows also in its vegetation, besides African, many Archipelago and even Australian types. Grandidier believes that in zoology the more distant eastern connection is at least as evident, if not more so, than that with the almost adjacent African continent. In plants the African connection is decidedly predominant.

I shall not attempt to follow Grisebach in discussing the peculiarities of the remainder of his regions. We may observe throughout the same careful investigation of the climatic conditions and their influence on their vegetative character of the individual plants, and on the general aspect of the whole vegetation they constitute (*Vegetationsformen* and *Vegetationsformationen*), with the same high estimate, or we might say over-estimate, of its effects on the typical character of the species as compared with the complicated consequences of previous possession, foreign invasions, and natural selection in the struggle for life, which he seems disposed to ignore, and with the same allusions to certain mysterious creative or productive forces beyond the reach of our inquiries. A closer examination of his regions show them to be much better conceived in his phyto-climatic point of view, than I had at first thought them to be when regarded as phyto-geographical regions; and although further explorations may cause him to modify their limits in several instances, yet, in regard to all of them, the data he has collected and methodised will be found to be an important contribution to the scientific study of geographical distribution, the value of which is enhanced by copious references to the sources whence he has derived his information.

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[*] Delivered Friday, May 24, and abridged.

[†] It may require, however, as suggested by Dr. Hooker, some further evidence to show that this green mass might not as well have been brought down from some African as from some American river.

There are two general subjects upon which the bulky mass of literature continues to receive considerable accessions, both in this country and on the Continent, without perhaps adding much to our stock of information, and which would at any rate require long and patient study to extract what may be really of value; these are Darwinism and so-called Spontaneous Generation. Darwinism in some shape or other, or something under that name, enters more or less into almost all general discussions on points of natural history, especially on the Continent, and in so far as it is applicable to what the Germans call the *Descendenztheorie*, it is being more or less tacitly adopted by the great majority of naturalists; but in a general way, the comprehensive hypotheses propounded by Darwin in his various works are still the subject of much polemical discussion. Seidlitz, in his work entitled "Die Darwinsche Theorie," fills thirty pages with the mere titles of the works, memoirs, or papers published on the subject since 1859, and to this enumeration many additions might be made. Amidst this great mass it might have been expected that I should have selected some to bring specially under your notice—that I should have followed up the observations I made on the "Origin of Species" in my address of 1863, and on the "Variation of Animals and Plants under Domesticity" in that of 1868, by some notice of the "Descent of Man," as well as of some recent works of other writers, such as Mivart's "Genesis of Species;" but these have been already fully discussed by naturalists much more competent than a purely systematic botanist to deal with the question in the phase which it has now reached, and I have not met with any other work in which any connected series of observations has been methodised and brought to bear more directly on the general life-history of animals and plants. The detached observations upon several points connected with Darwin's general theories, especially those relating to dichogamy and cross-fertilisation in plants, continue to be very numerous, as well as the endeavours to connect recent with geologically ancient races of both animals and plants, without, however, making any one move of importance towards the solution of the problems before us; and we are still anxiously awaiting from Mr. Darwin himself that long-promised second portion of his great digest which is to treat of the variations of undomesticated animals and plants.

Spontaneous Generation has perhaps been of late the subject of more controversy in this country than abroad. Since Prof. Huxley, followed by Prof. Tyndall, placed the matter in so clear a light at the Liverpool meeting of 1870, Dr. Bastian has returned to the charge. In his work entitled "The Modes of Origin of Lowest Organisms," he has published an account of numerous experiments further illustrating his views in opposition to those of Huxley and Tyndall, and confirming, in his mind, the theory of Archebiosis, the name he gives to what is commonly called Spontaneous Generation. On the other hand, Mr. N. Hartley has communicated to the Royal Society ("Proceedings," xx. No. 132) his experiments concerning the evolution of life from lifeless matter, which appear to have been conducted with great care, and in some measure under the guidance of Dr. Odling and Prof. Tyndall. From these he concludes that so far as our present knowledge guides us, whether we term it Spontaneous Generation, Abiogenesis, or Archebiosis, the process by which living things spring from lifeless matter must be said to be only ideal. The same number of these "Proceedings" contains abstracts of papers by Dr. Crace Calvert on the development of protoplasmic life, its influence on putrefaction, and the effect of various substances in promoting or arresting its progress, all of which papers are connected with, and in continuation of, his former experiments and conclusions tending to support the theory that this protoplasmic life is derived from invisible germs floating in the atmosphere. Dr. Bastian, at a later meeting of the Royal Society, again returned to the subject in a paper entitled "On some Heterogenetic Modes of Origin of Flagellated Monads, Fungus-germs, and Ciliated Infusoria," inserted at length in No. 133 of the "Proceedings." The experiments and observations here detailed are very interesting as to the development of these organisms in the pellicle that forms on infusions of organic matter when exposed to the atmosphere; but they do not affect the question of the origin of the living components of the pellicle itself, which he considers to have been fully proved by his own former papers, as well as by the well-known experiments of Pouchet and others, to have been evolved from lifeless matter by archebiosis. A more extended work, giving the fullest details of his views of the "Beginnings of Life," is announced, but I have not yet seen it.

If, then, Spontaneous Generation may as a theory in the minds of some persons have become referred to the class of paradoxes like the quadrature of the circle, yet it is still supported by so many naturalists whose opinions are entitled to consideration, and there is so much to be said for as well as against it which appears unsusceptible of direct and positive proof, that it is likely to be long maintained as a subject of controversy, without any further much more definite result. But there is one question of a more practical nature, often supposed to be connected with it, which has excited, and is still calling for the serious attention of men of science, experience, and judgment, as well as of various Governments. I allude to those parasitical scourges which within the last thirty years have made such havoc in several important articles of European food and industry. Thirty years since, and I believe up to the fatal year 1845, the potato-disease, the silkworm-pébrine, and the oidium of the vine were unknown in Europe; and we can most of us remember how the sudden appearance and rapid extension of each in succession produced the famine in Ireland, and the ruin of so many French and Italian silk-breeders and wine-growers of the Mediterranean region, Madeira, and Bordeaux; and how for long men of science have been baffled in their efforts at ascertaining the true history of the attendant fungi, and devising an efficacious remedy. The potato-disease appears now to have settled down into one of those chronic epidemics whose varying intensity, according to season and other circumstances over which we have little control, must enter into the calculations of every potato-grower. This useful tuber can no longer, indeed, be advantageously cultivated in that wholesale manner which induced the late Thomas Andrew Knight and others to attach to it so high an economic value, but it may now again be fairly depended upon as an important article of household food.

The pébrine of the silkworm, from the latest reports I have seen of the Commissions of Lyons and other places, shows but little abatement of its intensity, although it has in some measure changed its character, and is, it is to be feared, through the carelessness or cupidity of interested dealers, spreading even into those eastern regions which have been looked to for the supply of "seed" free from the fatal germ. The oidium, on the contrary, has been got more under control; and experience now shows that in many districts at least its ravages can be checked or entirely stopped by means within the reach of every intelligent cultivator. But within the last few years a new plague has in the south of France excited even more alarm than the oidium itself, from its insidious invasion and complete destruction of many of the most valuable vineyards; this time, however, the offending parasite is brought much more within the scope of direct scientific observation. The germs of the potato-fungus, of the pébrine, of the oidium, are all invisible and inappreciable by any of our instruments; the history of their diffusion and early development, and even their very existence, can only be judged of from their results and other circumstantial evidence; whilst the *Phylloxera vastatrix* can be watched in every stage of its varied existence, from the first deposit of the fertilised eggs, through its several agamic generations, to the latest winged form. The researches, accordingly, which have been already applied to it have not been altogether barren of results, throwing some light even generally upon the origin and dispersion of these pests. Considerable sums of money, either from the French Government or from private subscriptions, have been applied to the purpose, and the investigation has been chiefly carried on by our foreign member, Dr. J. E. Planchon, of Montpellier, assisted by

M. J. Lichtenstein, a relative, I believe, of the late distinguished Prussian zoologist. These gentlemen, since the first discovery of the disease in France in 1868, have devoted much of their time to it. They have compared their observations with those of others who in other countries have studied the insect, especially Mons Laliman, of Bordeaux, Mr. Riley, of Missouri, and with those of Prof. Westwood in our own country; and they have now, in a pamphlet which, by some inversion of dates not uncommon abroad, is supposed to form part of the Proceedings of the session of the French scientific congress at Montpellier in 1868, given a *résumé* of nearly five hundred memoirs, communications, or journal articles which have been published on the subject up to the close of last year (1871).

The main facts given as having been hitherto elicited as proved or probable may be shortly resumed as follows:—

The *Phylloxera*, like other Aphides, goes through a number of apterous generations of a single sex, but multiplying with enormous rapidity; for one or two individuals will lay as many as five hundred eggs, fertilised without previous copulation. It also gives birth occasionally to a winged generation of both sexes, the females of which lay only two or three eggs each.

The apterous *Phylloxera* is also dimorphous, a smooth bodied form living in little galls formed on the leaves of the vine, where it is comparatively harmless; and a tuberculate form living in the nodules it produces on the root-fibres, causing first the smaller and then the main roots to rot, weakening, in the first instance, and finally killing the whole vine. Each form has its winged generation.

The insect is evidently of North American origin, although the precise history of its transmission to this country has not been ascertained. It was first described by Asa Fitch in the Transactions of the New York State Agricultural Society for 1854; but living there chiefly on the leaves of the native vines, it had not attracted any peculiar attention. More recently, however, Mr. Riley has found reason to attribute to the ravages of the subterranean form the ill success of the various attempts made to establish in America the European grape-vine. In England, where the introduction of the insect from America may be readily conceived, Prof. Westwood's attention was first called to it in 1863, and again from various quarters in 1867 and 1868, whence resulted the above-mentioned account in the *Gardener's Chronicle* for January 1869 (p. 109). With us it does not appear to have spread much, and has therefore not called for any further observation, the damp soil, the mode of treatment, or other external circumstances, proving unfavourable for the development of the underground form. But having by some means reached and established itself in the dry, naturally-drained vineyards of the south of France its general character underwent a change; natural selection at once gave an enormous preponderance to the underground over the epiphyllous form. It was first discovered there in July 1868, and by the close of that year its ravages caused a panic among the vine-growers in many parts of Lower Languedoc and Provence, similar to that which we may remember in this country on the rapid spread of the potato disease in the autumn of 1845. It was immediately made the subject of scientific investigation, which has ever since been steadily pursued. As one result Dr. Planchon inclines to believe that the oidium and the potato disease, like the *Phylloxera*, and, in former days, the American blight of our apple-trees, had all been imported from America. It would seem that all these parasites, whether insects or fungi, capable of enormously rapid and extensive propagation, remain unnoticed so long as they are kept in check by the mutual relations of their constitution, habits, food, and other circumstances in which they are placed; but that the moment that a change, often very slight, in one or other of these conditions destroys the balance, they may at once and suddenly gain the upper hand, so as to be classed in the popular mind amongst those varied phenomena collectively designed as blights. That such a change is often the consequence of the transportation of the insect from one country to another may be regarded as more probable if Riley is correct in his belief that in America, as in Europe, introduced insects, when once established, are more noxious than indigenous ones. In the case of the *Phylloxera* some clue to the nature of the influencing alteration may be derived from the success attending one of the remedies applied, the inundation and continued submersion of the diseased vineyards during the winter months. The comparative dryness of the soil in the new over that of the original station of the insect has been the change which natural selection seems to have seized upon to effect the extraordinary development of the underground form, aided, perhaps, by some slight attendant change in its constitution. Prolonged, or even temporary inundation, is not however, practicable in the majority of the South of France vineyards, nor, indeed, in any of those producing the best wines. Amongst other remedies, soot (the soot of wood-smoke I presume) promises to be one of the most efficacious applications.

Amongst the various publications which these phenomena have called forth, we may still see cropping up not unfrequently the popular notion that they are blights mysteriously connected with meteorological conditions, against which it is vain to struggle; but, fortunately, the need of separately investigating every one of them is becoming generally recognised. In France, Government has appointed special commissions for inquiries into the silk and wine diseases. In Germany the ravages committed by insects on their forests have been the subject of various works, published chiefly under the patronage of the Austrian Government and scientific associations. In North America Mr. Riley, as Missouri State entomologist, makes annual reports on noxious insects to the Board of Agriculture of that State, pursuant to an appropriation for this purpose from the Legislature.[*] In Italy a special institution has been formed at Padua, under official patronage, for the study of cryptogamic parasites; and our Royal Horticultural Society is also making arrangements for the special encouragement of the study of economic entomology. To these and similar institutions it is the duty of science, in the interest of mankind, to give its unqualified support, to divest itself of all preconceived theories and prejudices, to avoid those polemical discussions which appear to have gone beyond the security they give for the exhibition of facts in all the various points of view they may bear, but impartially to study every detail connected with these scourges, which have so much increased during the present century, fostered, perhaps, by the advance of civilisation and high cultivation.

SCIENTIFIC SERIALS

THE *Lens* (No. 2), April, 1872.—This second number of the new American journal of microscopy contains little that is new or of importance. "The Flora of Chicago and its Vicinity" is continued by H. H. Babcock from the previous number, as is also the "Conspectus of the Families and Genera of the Diatomaceæ," by Prof. H. L. Smith. This second part of the Conspectus is occupied by a "Synonym Register," which promises to be useful, and is in fact the most complete attempt of the kind yet made. "Microscopical Memoranda for the use of Practitioners of Medicine," by Dr. J. J. Woodward, is also a continuation, and consists of two parts, viz., staining the sections, and mounting the stained sections in Canada balsam. There is also a chapter "On the double marking of *Triceratium*," by the same author, accompanied by a Woodbury print of two frustules of *Triceratium fimbriatum*. "On the effect of the reversal of the current of the Chicago River on the Hydrant Water," by H. H. Babcock. "Where to search for Diatomaceæ" is a reprint from the *Intellectual Observer*, and "Alternation of generation in Fungi," by M. C. Cooke, from NATURE. This number of the *Lens* is increased to double the thickness of the previous one by the insertion of the catalogue of optical instruments manufactured or sold by an American firm in Philadelphia and New York.

IN the *American Naturalist* for April we have an article by Dr. J. J. Woodward on the Use of *Amphipleura pellucida* as a Test-object for High Powers, illustrated by a photograph. Dr. Abbott concludes his exhaustive paper on the Stone Age of New Jersey; and the remainder of the number consists of reviews and book notices, and of short paragraphs under the headings of the various departments of natural science.

In the number for May is a note by Mr. J. G. Henderson on the use of the rattles of the rattlesnake, in which he comes to a somewhat different conclusion from Prof. Shaler, believing that it is protective in its object.—Mr. J. A. Allen contributes some ornithological notes from the West, the present communication referring to the birds of Kansas.—Prof. A. H. Tuttle gives the result of a careful study of the genus *Urella* of Flagellate Infusoria, illustrated with a number of woodcuts.

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[*] Since writing the above I have seen a proof-sheet of a portion of the forthcoming fourth report of the Missouri State entomologist, Mr. Riley, in which he enters into further details of the history of the *Phylloxera*, collected during a recent visit to Europe, as well as from closer observations on the subject made in America, where it appears to be acquiring more serious importance. I have not, however, yet seen enough of the report to learn what further conclusions Mr. Riley may have arrived at.

There are also useful practical papers of instructions for herborising, for collecting micro-lepidoptera, and for preparing birds' eggs; as well as a large number of interesting paragraphs of information under the heads of the natural sciences.

THE *Journal of the Franklin Institute* for May contains a good drawing of Danks's patent puddling furnace, and a large number of paragraphs under the head "Items and Novelties." The substantial articles include a continuation of Prof. Nourse's paper on inter-oceanic communication across Central America; Mr. John Warner on the diamond rock drill; Mr. C. Van Bruol on a new modification of the Holtz machine, and a list of auroral displays during February at a number of stations in the United States; and the continuation of various other papers commenced in the preceding numbers.

THE number of the *Transactions of the Linnean Society* just published, vol. xxix. part 1, contains the commencement of Colonel Grant's Botany of the Speke and Grant Expedition, including an enumeration of the plants collected during the journey of the late Captain J. H. Speke and Captain J. A. Grant from Zanzibar to Egypt in 1860-63. The determinations and descriptions of the species are by Prof. Oliver, Mr. J. G. Baker, and other botanists connected with the Kew Herbarium; while Colonel Grant writes an introductory preface, alphabetical list of native names, and notes. It is prefaced by a good map of the whole of the journey; and illustrated by thirty-seven drawings on stone of new or remarkable species. The total number in the whole paper will be 100.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, May 22.—Prof. Morris, vice-president, in the chair. The following communications were read:—1. A communication from the Right Hon. Earl Granville, inclosing a report from H. M. Minister at Rome, relating to the recent eruption of Vesuvius. 2. "On the Phosphatic Nodules of the Cretaceous Rock of Cambridgeshire," by the Rev. O. Fisher, M.A., F.G.S. This paper contained an attempt to explain the origin of the phosphatic nodules which lie in a thin bed at the base of the Chalk in Cambridgeshire, and are largely extracted by washing the stratum for the purpose of making superphosphate of lime. Two hundred and seventy tons per acre, at the rate of fifty shillings a ton, represents the valuable yield of the deposit, which is followed to the depth of about 18 feet. The nodules and other fossils of the bed are chiefly derivative, forming a concentrated accumulation from a deposit belonging to the Lower Cretaceous period. Some of the fossils are, however, believed to be indigenous to the deposit. *Plicatulæ* are attached to all the derivative fossils and nodules, and the sharp, broken surfaces of the latter, with *Plicatulæ* on them, show that they were mineralised before they were deposited in their present *gisement*. The green grains of chlorite have been drifted into patches. Certain calcareous organisms are preserved, but many genera of molluscs only occur as casts in phosphate of lime. The phosphatic matter has been determined in its deposition by *animal* substances. There are two chief varieties of the "ordinary" nodules. The first are amorphous, or else finger-shaped; the second formed like a long cake rolled partially or wholly upon a stick. The surface of these two kinds of nodules is coriaceous and wrinkled, and they usually show marks of attachment to some foreign body. Certain species, clearly zoophytes, are converted into phosphatic nodules, and, when sections are made of these, they are found to show under the microscope structures and spiculæ allied to those of Alcyonaria. Slices of the *common* nodules show similar spicular, and occasionally reticular structure. When casts in plaster are made from *Alcyonium digitatum*, and coloured to resemble the nodules, the similarity in general form and structure of surface is very striking. The phosphate was probably segregated by the animal matter from its solution in water charged with carbonic acid, which is a known solvent of the phosphate; an analysis of the matrix has proved that phosphate of lime is appreciably present in it. The author doubted the derivation of the nodules from the denudation of the subjacent Gault, and exhibited a collection of these to show that they were distinguished by more stunted growth. The deposit was on the whole considered to represent the thin band with similar fossils at the base of the Chloritic Marl, as seen in the West of England, in which district it is underlain by the true arenaceous Greensand. The absence of the true Greensand was attributed to the intervention of the old palæozoic axis of the London area; and it was finally suggested that a similar axis might stretch from Leicestershire to Harwich, causing the change in character of the Lower Cretaceous beds between Cambridgeshire and Norfolk.—3. "Some observations on the Upper Greensand formation of Cambridge," by Mr. W. Johnstone Sollas. The Greensand Formation consists around Cambridge of a Chalk marl containing harder portions of a different nature disseminated throughout it, these are separated from the Chalk Marl by levigation, and sorted by sifting into larger bodies, consisting almost entirely of the so-called "coprolites," and smaller bodies—the so-called "Greensand." The author gave a general account of his conclusions regarding the "coprolites," reserving details for a future communication. Of all the facts the most obvious is the connection between presence of "coprolite" and former existence of organic matter; when coprolite is found incrusting a bone or other fossil, it is precisely on those parts where animal matter adhered most abundantly. Instances were cited, as in *Palæocorystes*, where the absence of animal matter on the back of the carapace is marked by an absence of phosphatic incrustation; while the sternal side, where animal matter could easily escape, is often altogether embedded in "coprolite." Coprolites are the fossilisation of organic matter derived from very various sources. In many cases they owe their origin to sponges, almost certainly so in the case of cylindrical coprolites perforated by a cylindrical cavity, now filled up with Chalk Marl; other forms have an *allied* origin. Thus coprolites are the flints of the Gault. The Greensand is a mixture of calcareous, silicious, and dark-coloured grains of uncertain chemical composition. The calcareous grains consist of sponge spicules, minute shells, fragments and prisms of shell substance, bivalve entomostraca, microscopic corals, minute echinoderm species, polyzoa, and foraminifera. A list was given of the foraminifera, the abundant occurrence of *Lagena* here being particularly noticed, as, with the exception of *L. apiculata*, mentioned by Reuss, the genus had not before been noticed below the Maestricht Chalk. The silicious grains consist of fragments of various rocks, some of volcanic origin. The dark coloured grains are coprolitic *débris* and true green grains. The green grains are almost all casts of foraminifera, derived chiefly from *Bulimina*; others are derived from *Lituola*, *Rotalina*, *Globigerina*, and other forms. Some green grains of exactly the same nature had been found by the author in the silicious sand of Blackdown.—Prof. Phillips was glad that his casual remark had produced such satisfactory results as the paper he had heard. It was satisfactory to find that the bulk of the phosphatic nodules exhibited such marked traces of an organic origin. Though he had to some extent been prepared for this, it appeared that the view might be extended much further than would at first sight have been anticipated. He drew an analogy between the preservation of the forms of sponges in their silicified fossils with that of the soft organic bodies in the Greensand by phosphatic matter. In each case the surrounding water contributed a large amount of either flint or phosphate of lime, which was segregated and accumulated round certain centres or nuclei of organic bodies.—Prof. Ramsay inquired from what sources the abundance of phosphatic matter requisite for the production of these fossils could have been derived. In such thin strata, which seemed to indicate a transition from a land to a marine surface, it was a matter of great difficulty to his mind to account for so great an abundance of phosphatic matter.—Mr. Godwin-Austen remarked that phosphoric acid was largely present in sea water, and instanced the present seas, where, as on the Newfoundland banks, fish existed in enormous quantities, and no doubt also phosphatic matter. The Cambridge beds, though so rich, were by no means unique of their kind. He referred to a paper communicated some years ago to the Society by Mr. Payne, as affording many interesting particulars with regard to such beds. He considered that much of the phosphate attaching to decaying animal matter might have been derived from comminuted excrementitious deposits floating in the water.—The Rev. T. G. Bonney remembered a fact quoted by the late Dr. Mantell as to the large quantities of dead Mollusca which had been observed floating down some of the American rivers, and which had been regarded as a plentiful source of phosphatic matter. Small fishes might also have furnished a considerable quantity, and their value as manure was recognised at the present day. With regard to the nodules being Alcyonaria or sponges, he observed that what spicules he had seen appeared more like those of sponges. He agreed with Mr.

Sollas as to the foraminiferal origin of many of the green grains. He did not agree with Mr. Fisher in attributing all the nodules to the bed in which they were found, but thought that a considerable portion might be referred to the upper part of the Gault. In proof of the washing the Gault near Cambridge had undergone, he mentioned the occurrence there of a number of boulders of rocks quite foreign to the district.—Mr. J. F. Walker thought that most of the fossils of the phosphatic band at the base of the Chalk-marl were derived from the Gault, whilst the bed differed from chalk only by green grains becoming gradually more abundant. The fossils were generally much water-worn, the characteristic fossils of the Warminster Greensand were absent, and the most abundant fossils were all of Gault species. It seemed that wherever these accumulations of phosphatic matter occurred denudation had taken place, and that they were the residuary heavy materials of a large thickness of rock. This might also be observed in the Upware and Potton beds.—Mr. Whitaker observed that the Upper Greensand thinned out as much to the south as to the north of London. He inquired as to the alleged abundance of phosphate of lime in the upper part of the Gault. He doubted whether the thin band at Cambridge could represent the great thickness of Upper Greensand which was to be found in some other districts. He regarded it rather as a gradual passage into Chalk, though the line of demarcation was evident on the Gault. Though agreeing with Mr. Walker as to some of the fossils having been derived from the Gault, he could not regard them all as having come from that source.—Mr. Meyer thought that the Greensand had always been absent in the Cambridge district, and mentioned the occurrence of a bed of much the same character as that in question at Knighton in the Isle of Wight.—Mr. Forbes pointed out that the amount of phosphatic matter in fishes was so small that it was difficult to assign such an abundance as that described to this source. In limestones almost entirely composed of shells, he could find only from ½ to 1 per cent. of phosphate of lime. Even with true coprolites, he thought that they had become richer in phosphate since their deposition; but whence it was derived he would not pretend to say. He thought this question of derivation still open.—Prof. Morris mentioned the occurrence of similar deposits near Wissant, on the coast of France, and near Calne, in Wiltshire. He called attention to the extremely quiet nature of the sea in which the phosphatic bed had been deposited, and observed on the existence in recent times on certain sea-shores of ooze containing a large amount of phosphatic matter.—Mr. Fisher, in reply, stated that he had in his paper but slightly touched on the sources of derivation of the phosphate of lime; but as to the possibility of that substance being localised and derived in large quantity from fish, he pointed out that the principal manure of modern times, guano, was derived from this source. He alluded to the possibility of some process of dialysis having contributed to the segregation of the phosphate. He disputed the identity of the nodules in the Gault and in the Chloritic Marl of Cambridge. As to the character of the fossils, he regarded it as the same as that to be found in a thin band at the base of the chalk in parts of Hants and Dorset. Mr. Sollas had examined sections of the fossils from the Cambridge beds under the microscope, but had failed to find the canals or tuberculated spicules characteristic of Alcyonaria. He had, however, in the sand found numerous indisputable sponge spicules. He had, moreover, found in sections of the coprolites spicules such as were regarded by Dr. Bowerbank as characteristic of sponges. He hoped, however, to recur to the subject. Both Mr. Fisher and himself concurred in removing these nodules from the category of concretions, and placing them under the head of organic fossils The transported blocks in the beds bear evidence of glacial action, and he considered had been brought from Scotland or Scandinavia. The cold sea then existing at the base of the Scandinavian chain of mountains flowed southwards over the bottom of the ocean, carrying with it mineral matter in solution, particularly phosphates, so that in this way he thought that some portion of the phosphatic matter was derived from the decomposition of the volcanic rocks north of Lammermuir, which were rich in this substance, and of which rocks he had found fragments near Cambridge. He considered that, under certain circumstances, the phosphate matter present in water would combine with animal matter, and hoped at some future time to offer some remarks on this subject to the Society.

Zoological Society, June 4.—Prof. Flower, F.R.S., V.P., in the chair. Mr. G. Dawson Rowley exhibited a specimen of *Zonotrichia albicollis*, which had recently been captured alive near Brighton, being the second recorded instance of the occurrence of this bird in the British Islands.—Mr. P. L. Sclater exhibited a specimen of the American Black-billed Cuckoo (*Coccyzus erythropthalmus*) killed in Ireland. This specimen had been referred by Mr. Blake Knox to the Yellow-billed Cuckoo (*Coccyzus americanus*), and by Lord Clermont, subsequently, to the Black-billed species (*C. erythropthalmus*). Mr. Sclater remarked that there could be no question of the latter determination being correct.—The Secretary communicated extracts from a letter received from Captain Henry Pain, of the S.S. *Scanderia*, on the habits of the Sea-Lion (*Otaria jubata*) and Fur Seal of the Falklands (*O. falklandica*).—Prof. Owen, F.R.S., read a paper on Dinornis, being the nineteenth of his series of memoirs on this genus. The present communication contained the description of a femur, indicative of a new genus of large wingless bird (*Dromornis australis, Ow.*) from a post-tertiary deposit in Queensland, Australia.—Prof. Flower, F.R.S, read a note on some points in the anatomy of the Two-spotted Paradoxure (*Nandinia binotata*), and showed that the cæcum is absent in this animal, contrary to the almost invariable rule which distinguishes the Arctoid subdivision of the Carnivora.—A communication was read from Dr. John Anderson, Curator of the Indian Museum, Calcutta, on the osteology and dentition of *Hylomys*. Dr. Anderson came to the conclusion that this form was most nearly allied to *Gymnura*, and belongs to the Erinaceidæ.—Mr. E. T. Higgins read a paper describing some new species of shells discovered by Mr. Clarence Buckley in Ecuador.—Mr. F. Moore communicated a paper by Captain Thomas Hutton on the Bats of the North-Western Himalayas, in which several new species were described.—Mr. P. L. Sclater read some additional notes on rare or little known animals, now or lately living in the Society's Gardens.—Dr. J. Murie read a paper on the Indian Wild-Dog (*Canis dukhunensis*); his observations being based on two specimens formerly living in the Society's Gardens. After noting points in their anatomy, Dr. Murie specially referred to the variations in the pelage and skulls, which distinguish the four supposed species of the genus. These he was inclined to regard only as one species, viz. *C. primævus*, with geographical varieties.—A second paper by Dr. Murie contained observations on the Bornean Ape (*Macacus maurus*), being the first of a series of papers on the rarer forms of this group.

Entomological Society. June 3.—Prof. Westwood, president, in the chair. Mr. Stainton exhibited specimens of a very large black *Coccus* on the cork oak found at Cannes by Mr. Moggridge. Also specimens of *Antispila rivillei*, bred from larvæ mining the leaves of the vine at Massa di Carrara, found by the Hon. Miss de Grey. This insect was first discovered in the island of Malta about 1750 by De Riville, but was not again noticed until 1871.—Prof. Westwood exhibited a large cottony mass, in which were enveloped the cocoons of a minute parasitic Hymenoptera which infested a large caterpillar in Ceylon; one of these caterpillars had produced at least 1,000 of this parasite .Mr. F. Moore had observed a similar occurrence in the larva of a large *Bombyx* from Bombay.—Prof. Westwood also exhibited apple twigs, the buds of which were destroyed by a larva, apparently of a *Tortrix*.—Mr. Higgins exhibited a selection of magnificent species of *Cetoniidæ* from Java obtained from Dr. Monicki —Mr. Weir observed that he had recently found the larvæ of *Gonopteryx rhamni* feeding upon *Rhamnus alaternus* in his garden at Blackheath; this insect had not been seen there during sixteen years, and not until he planted this *Rhamnus*, which it immediately discovered, although so totally unlike the two indigenous species of the genus on which the larvæ habitually feed here.—Mr. Miller called attention to a paragraph in the daily newspapers concerning the enormous increase of ants on the island of May to such an extent as to render the land useless to the lighthouse-keepers. The subject had been brought to the notice of the Northern Lighthouse Commissioners, and a visit had been made to the island for the purpose of investigating the matter.

Linnean Society, June 6.—Mr. G. Bentham, president, in the chair. The president appointed Mr. Geo. Busk, Dr. J. D. Hooker, Mr. John Miers, and Mr. W. W. Saunders, vice-presidents for the year. The papers read were:—On some recent forms of *Lagenæ* from Deep-sea Dredgings in the Japanese Seas, by J. W. O. Rymer Jones; On the Cutaneous Exudation of the Water newt (*Triton cristatus*), by Miss E. A. Ormerod.

Chemical Society, June 6.—Dr. Gilbert, F.R.S., vice-president, in the chair.—"On a Remarkable Salt deposited from the Mother Liquors obtained in the Manufacture of Soda," by Prof. E. T. Thorpe; "On the Composition of Ceylon Jargons," by M. H. Cochran; "On a Double Sulphide of Gold and Silver," by Mr. Pattison Muir; "On the Solvent Action of various Saline Solutions upon Lead," by the same author; "On the Magnetic Sand of Mount Etna," by J. B. Hannay; "New Tests for some Organic Fluids," by J. A. Wanklyn; "Dendritic Spots on Paper," by A. Liversidge; "On Chinoline and Leucoline," by C. Greville Williams, F.R.S. A letter from Mr. Dewar of Edinburgh was then read by the secretary on some derivatives of chinoline.—Dr. C. R. A. Wright read a paper on the "Action of Phosphoric Acid on Morphine," and Mr. W. H. Perkin, F.R.S. "A Note on the Secondary Colouring Matter produced in the preparation of Alizarine from Anthracene;" "On the Effects of Temperature on the Absorption of Gases by Charcoal," by Mr. J. Hunter. Dr. Armstrong then brought forward a series of "Communications from the Laboratory of the London Institution, No. V. On the Nitration Products of the Dibromo-phenolsulphonic Acids; No. VI. On Bromo-phenosulphonic Acid; No. VII. On the Formation of Substituted Nitro-phenolsulphonic Acids," and finally the secretary read a letter which had been received from M. E. Maumené of Paris.

PHILADELPHIA

Academy of Natural Sciences, October 3, 1871.—Dr. Ruschenberger, president, in the chair. Mr. Thomas Meehan referred to some observations made by him last spring before the Academy in regard to the office of bud scales and involucral bracts. The general impression was that they were formed for the purpose of protecting the tender parts beneath. At that time he exhibited the branches of *Fraxinus excelsior* on which some of the buds were entirely naked, and others clothed with scales in the usual manner. They could scarcely be for protection in this instance, as both were equally hardy. He now had to exhibit an ear of corn which had been produced without the usual involucral bracts or husks, and yet was as perfect as if clothed in the usual way, showing that the husk was of not much importance as a protecting agent. An interesting point was that this ear had been formed on the end of a male panicle or tassel. It was not uncommon to find scattered grains of corn amongst male flowers, bnt a perfect ear like this he had never before seen. The ear was eight-rowed, and contained two hundred perfect grains. It was the variety known as "popcorn."

PARIS

Academy of Sciences, June 3.—Mr. A. Cayley communicated a paper on the surfaces divisible into squares by their curves of curvature, and on Dupin's theory.—A memoir by M. Yvon Villarceau on isochronous regulators derived from Watts's system was read.—M. de Pambour read a note on the additional friction due to the load of machines.—M. Le Verrier communicated some observations on magnetic declination made at Batavia and Buitenzorg during the solar eclipse of December 12, 1871, extracted from a letter from M. Bergsma, in which the author stated that this eclipse exerted no influence upon the direction of the magnetic needle either at Buitenzorg, where the eclipse was total, or at Batavia, where it was nearly so. M. Le Verrier also presented a note by M. C. Grad on the magnetic declination in Algeria, containing the results of a series of observations made at fourteen stations during the last winter.—A paper was read by M. Berthelot on the formation of acetylene by the obscure electrical discharge; and another by MM. Berthelot and Bardy on the transformation of ethylnaphthaline into acenaphthene.—A note by the Abbé David, on a new species of *Paradoxornis*, was read. This bird was obtained near Shanghai by Father Heude, and is described under the name of *P. Heudei*.—M. C. Robin presented a note by M. A. Dufossé on the noises and expressive sounds uttered by fishes.—M. Bouillaud presented some considerations on chlorosis and anæmia in the human subject, with reference to M. Boussingault's paper, read at the last meeting, on the iron contained in the blood and in food; and M. A. Dumont read a note on the distribution of the water of the Rhone at Nîmes.

BOOKS RECEIVED

ENGLISH.—Michael Faraday: J. H. Gladstone (Macmillan and Co.)—My Garden, its Plan and Culture: Alfred Smee (Bell and Daldy).—A Lecture on Science and Revelation, 2nd edition: Jas. Stuart (Longmans and Co)—A Discourse on the Inductive Philosophy: A. C Finch (Longmans).—First Lessons on Health: J. Berners (Macmillan).

DIARY

THURSDAY, JUNE 13.

ROYAL SOCIETY, at 8.30.—Further Experiments on the Effect of Alcohol and Exercise on the Elimination of Nitrogen: Dr. Parkes, F.R.S.—On the Spectrum of the Great Nebula in Orion, and on the Motions of some Stars towards or from the Earth: W. Huggins, F.R.S.—On Blood Relationship: F. Galton, F.R.S.—Report of further Scientific Researches in the Mediterranean, Aug.-Oct., 1871: Dr. Carpenter, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—Ancient Rings from Palestrina: C. D. E. Fortnum, F.S.A.—Polychrome Vitreous Beads: J. Brent, F.S.A.

MATHEMATICAL SOCIETY, at 8.—On the Surfaces divisible into Squares by Curves of Curvature: Prof. Cayley.—On Prof. Cremona's Transformation between Two Planes and Tables relating thereto: Mr. S. Roberts.—On a Manifold Correspondence of Two Planes: Dr. Hirst.—Note on a Special Case of the Anharmonic Ratio Sextic: J. J. Walker.

FRIDAY, JUNE 14.

ASTRONOMICAL SOCIETY, at 8.

MONDAY, JUNE 17.

ANTHROPOLOGICAL SOCIETY, at 8.—The Tribes of North Aracan: St. Andrew St. John.—Australian Languages and Traditions: Rev. W. Ridley.—Indian Picture Writings in British Guiana: C. B. Brown.—Exhibition of Photograph of the Tattooed Man, and Short Description: A. W. Franks.—The Ainos of Yeso: Commander H. C. St. John.

TUESDAY, JUNE 18.

ZOOLOGICAL SOCIETY, at 9.—On the Cranial Appendages and Wattles of the Horned Tragopan (*Ceriornis satyra*, Linn.): Dr. J. Murie.—On the Cetacea observed during the voyage round the world of H.I.M.S. *Magenta*, 1865–68, with the description of several new or little known species, and of a new genus of Fin-backed Whale: Prof. H. H. Giglioli.

WEDNESDAY, JUNE 19.

GEOLOGICAL SOCIETY, at 8.—On the Discovery of Palæolithic Implements in association with *Elephas primigenius* in the Gravels of the Thames Valley at Acton: Col. A. Lane Fox.—On the Evidence for the Ice-sheet in North Lancashire and adjacent parts of Yorkshire and Westmoreland: R. H. Tiddeman.—On a new Species of Coral from the Crag: Prof. P. Martin Duncan, F.R.S.

METEOROLOGICAL SOCIETY, at 7.—Anniversary Meeting.

THURSDAY, JUNE 20.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

LINNEAN SOCIETY, at 8.—On the structural peculiarities of the Bell Bird (*Chasmorhynchus*): by Dr. Murie, F.L.S.

CHEMICAL SOCIETY, at 8.—On Deacon's Method of obtaining Chlorine, as illustrating some principles of Chemical Dynamics: H. Deacon

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, JUNE 20, 1872

LIVINGSTONE

DR. LIVINGSTONE is one of those men, becoming scarcer now in these nervous days of hurry and excitement, who do what they put their hands to with all their might. He went to Africa to discover certain regions then unknown, and especially to determine the extent and character of the great catchment basins on the eastern side of the continent. His object was not solely, or even chiefly, the advancement of geographical knowledge. In his eyes geography is only a means to an end. He hopes, through an extension of the knowledge of the interior of Africa, to call forth a spirit which may be the means of securing the great objects of his life—the extinction of the slave trade, and a permanent improvement in the condition of the negro race.

Some six years ago Dr. Livingstone landed at the mouth of the Rovuma, and disappeared from the knowledge of European seekers for news. Then there came a wild report of his murder, and staunch old Sir Roderick sent out an expedition, under Mr. Young, to Lake Nyassa, which successfully performed its mission, and gave us the assurance that the report was false and that Livingstone was alive. All this while the great traveller was toiling steadily at his appointed task, and had completed the solution of one great geographical question, namely, that of the northern limits of the basin of the Zambesi river. Another long period elapsed, and once more a letter was received from Ujiji, on the banks of Lake Tanganyika, announcing the progress of the work. Having cleared up the problems relating to Lake Nyassa, Livingstone had ascended highlands which form the water-parting between the Zambesi and another great system of rivers and lakes to the north. He had been in a land where the vegetation was saturated with moisture—a land unlike all previously-conceived ideas of this part of Africa. The work was beginning to tell upon him. He described himself as a mere "bag of bones." But he gave no sign of faltering in his purpose. His great discovery was not half achieved, and the time for rest was still distant. His will was unsubdued; his life-work must be completed before he could turn aside to be refreshed; and thus he disappeared again.

Years passed away—first one, then another and another, and for a third time the anxiety of the country began to increase. For Britain still cares for and watches over her great sons. The indomitable yet unostentatious resolution of this grand old man has touched the heart of the nation to its very core. Sir Roderick Murchison died in the full hope and expectation of soon receiving tidings of his friend. No truer nor more steadfast friend ever lived; and the news of Sir Roderick's death will be the saddest words that Livingstone has heard since he lost his brave wife in the wilds of the Zambesi. Then it began to be felt that it would be wrong to wait longer. Our patience was exhausted; an appeal was made to the country which was warmly and munificently answered; Lieut. Dawson left this country in command of a search expedition, reached Zanzibar, and proceeded without delay to make preparations for his march into the interior.

The rest of the story must be gathered from the telegrams which have arrived from Bombay and Aden within the present week. News, it seems, came down to Zanzibar last May that Livingstone was alive, that he had reached Kazeh, on the road between Lake Tanganyika and the coast; but that he declined to return home until his work was completed. In those years of enforced silence, during which his letters had been intercepted by Arab slave traders, he had been working hard. He had completed one more great discovery; but still the work was not all done, and he would not come home. All honour to this man of iron will and unchanging purpose!

The second great discovery of Livingstone, since he landed at the mouth of the Rovuma, is more important, if possible, than the first. His first discovery was the north-eastern water-parting of the Zambesi. His second, the tidings of which arrived by telegram last week, is the limits of the great basin of Lake Tanganyika, and that a vast and separate system intervened between the basins of the Nile and the Zambesi. The discovery of the basin of Tanganyika, extending from about 3° to 10° S. latitude, and 27° to 39° E. longitude (or 700 miles long by about 450) is the last and not the least important of Livingstone's discoveries. It would appear, from the telegram, that the great explorer traced the chain of lakes and the streams which flow from them, until he discovered that all the waters found their outlet in the Tanganyika. He then, it would appear, visited the northern end of the lake, and found that the rivers still flowed into it. The waters of the lake are fresh; and it is, therefore, to be inferred that the lake has an outlet. Livingstone now knows the southern, western, northern, and north-eastern sides of the lake. The south-eastern side alone remains to be explored, and there, if anywhere, the great outlet for its waters must be. That outlet must be discovered and examined before Livingstone's great achievement is ended; and thither, therefore, he will now proceed.

We already have some knowledge of the river which, as it would now seem, flows from Lake Tanganyika to the sea. Mr. Desborough Cooley, in 1841, gave the information obtained from an intelligent Sawahili named Khamis bin Othman, who came to London in 1835. This man had travelled up the ravine of a river named Lufiji, from its mouth due west of the island of Monfia (south of Zanzibar) to its source in the lake. Nearly half a century ago, when Captain Owen was making a running survey of part of the East Coast of Africa, he was off the mouths of this river Lufiji, and they are shown on his chart, published in 1825, though Mr. Cooley and Captain Burton appear to have overlooked them. But Captain Burton, in his exhaustive paper on these lake regions, has shown that the Lufiji is the same river as the Rua or Ruaha, though he says that the source is unknown. It will be found on the maps to the east of the south end of Lake Tanganyika. It must not be confused with another Rua, mentioned by Livingstone to the west of Lake Tanganyika, and north of the Lake Moero. The sentence in Lieut. Dawson's telegram, "Underground village next attracts Livingstone's attention," has, perhaps, been satisfactorily explained by Colonel Grant. He gathered, from the intelligence he and Captain Speke obtained in the country, that the waters of the Tanganyika force their way through a rent in the mountains, at the south-eastern extremity of the lake, and that under the river there is a natural tunnel. This tunnel was described to Colonel Grant as being two months' march from Unyanyembeh, and as a tunnel made by God, which takes a caravan from sunrise to noon to march through it. An unfordable river, with rocky cliffed sides, flows over the tunnel at right angles with Lake Tanganyika. This river is now supposed, on apparently good grounds, to be the Ruaha of Burton, and the Lufiji whose mouths are shown on Captain Owen's chart.

We now learn that Livingstone has reached Unyanyembeh, that stores are being sent up to him as rapidly as possible in charge of his son, and that he will march southward to explore this Ruaha or Lufiji river, this mighty outlet of the great system of waters that he has discovered, with its lofty cliffs and alleged natural tunnel. Thus, for the third time, all fears have been dispelled, again we get a glimpse of this true knight-errant, and again we find him stedfastly working at the task he set himself to do six years ago, and which he will not abandon until it is finished. This last section of his labours will comprise the complete discovery of the great basin of the Tanganyika, including the collection of accurate information respecting its limits, its varied climates, its productions and capabilities and people, its rivers and lakes, and its outlet to the Indian Ocean. The addition to geographical knowledge will be enormous, and we may well hope that this knowledge will be the means by which a new country will hereafter be opened to European enterprise, and the object of Livingstone's life be attained. If he dies in the midst of his discoveries he may well be envied, for a nobler and more glorious end can hardly be imagined. If, as we all hope and as is more likely, he is spared to return home, and perhaps to watch in his old age the progress of the mighty work which he is now initiating, he will receive a welcome from his countrymen such as few have experienced and fewer still have so justly earned.

CONVERSATIONS ON NATURAL PHILOSOPHY

Conversations on Natural Philosophy. By Mrs. Marcet author of "Conversations on Chemistry," &c. Revised and Edited by Francis Marcet, F.R.S. 14th Edition. (Longmans, 1872.)

WE opened this new and revised edition of Mrs. Marcet's "Conversations on Natural Philosophy" with expectation and interest; we closed it with disappointment and regret. The influence Mrs. Marcet exerted upon the early career of Faraday, besides awakening the first love for science in hundreds of the last generation, will cause her name always to be remembered with gratitude and respect. Science, however, has made great strides since Mrs. Marcet wrote; and if her admirable works are touched at all, they should, where necessary, be carefully and accurately revised. That this has not been done in the book before us we will briefly point out. Opening at the Conversation on Heat, we read the following statement on p. 207:—"It is because heat, light, and electricity are not subject to the general properties of other bodies, and in particular to that of gravity, that they are commonly known by the name of 'imponderable fluids;" and on the next page we read "that modern chemists having adopted the new word *caloric*, to denote the principle that produces heat," we are told that "caloric is found to exist in a variety of forms or modifications; and we shall consider it under the two following heads, viz.: 1. Free or radiant caloric: 2. Combined caloric. The first free or radiant caloric is also called heat of temperature," &c. Again further on, p. 250, the same instructor says, "the two principal solvent fluids are *water* and *caloric*," leading thereupon to a lively conversation as to how caloric dissolves bodies. This, we are told, is the way:—"Caloric, we may conceive, dissolves water, and converts it into elastic vapour by a process similar to that by which water dissolves salt. . . . It is now ascertained that the solvent power of the atmosphere depends solely upon the caloric contained in it"! Vivid pictures of caloric are given, as "a fluid so extremely subtle that it enters and pervades all bodies whatever, and forces itself between their particles;" in similar language specific heat is defined, on p. 275, as "that which is employed in fitting the capacity of a body for caloric, in the state in which that body actually exists." Thus the minds of young children for whom this book is intended are drilled into the needless and obsolete jargon of the material theory of heat.

Even the most elementary facts are often left wrongly explained. Thus, on p. 255, the formation of hoar frost is accounted for in this way:—"The freezing of the watery vapour, which the atmospheric heat could not dissolve, produces what is called a hoar frost; for the particles descend in freezing and attach themselves to whatever they meet with on the surface of the earth." We venture to think there are few intelligent unscientific people who could not correct this.

We have dwelt thus far on the subject of heat, for here it is that new editions of once famous books need most revision. But glaring errors are to be found in other parts. The diagram representing the shadow which a large luminous body casts behind a small opaque body (Plate 21, Fig. 3), is incorrectly shown, the converging umbra only being represented without the accompanying diverging penumbra. On p. 313 the luminiferous ether and water are spoken of as inelastic bodies. The absence of a sound shadow in air is affirmed on the same page, whereas among other instances every one must have noticed when watching the approach of a distant railway train, how, as it winds along and is occasionally hidden from view, corresponding sound shadows flit across the ear.

We have only space to indicate a few more blunders that catch our eye. In voltaic electricity the electric light does *not* "dart from one point of charcoal to another," as soon as the points are brought from "half an inch to an inch" asunder. The thickness of a silk handkerchief (as the writer has tried with a battery of nearly 200 cells) will prevent the discharge taking place before contact is made. The term "conjunctive wire," p. 428, was used by Oersted, but is not now used to express the wires which join the poles of a battery. In the electric telegraph the current does *not* return through the earth to the battery whence it came. This is a very popular error.

We regret to be obliged to call attention to these serious defects in what might have been made a useful book. We still more regret to think that this volume, owing to Mrs. Marcet's excellent name, will find its way into families and schools; many will thus gain their only knowledge of science from a volume which contains not only many obsolete phrases, but which also omits all reference to the conservation of energy or the correlation of the physical forces.

W. F. B.

THE GEOLOGICAL SURVEY OF OHIO

Geological Survey of Ohio. Report of Progress in 1870, by J. S. Newberry, Chief Geologist, including Reports by the Assistant Geologists, Chemists, and Local Assistants. (Columbus: Nevin and Myers, State Printers, 1871, pp. 568.)

THE labours of Prof. Newberry and his colleagues during the year 1870 have resulted in the accumulation of a great many details relating chiefly to the structure of that portion of the great Appalachian Coal-field which extends over a considerable part of Ohio. Without the aid of a good map it is somewhat difficult to follow the descriptions given in this Report, the numerous local references and details having a tendency to bewilder the reader. This, however, is unavoidable under the circumstances; and those who desire to obtain a full and clear conception of the geological structure of Ohio will have to wait the completion of the map and final report promised by Dr. Newberry, the present volume not pretending to be more than its title implies. Nevertheless, it contains a very large and varied amount of information, which will, no doubt, be duly appreciated by those for whom it has been prepared. Especially noteworthy are the numerous illustrative sections of Carboniferous strata, and analyses of coals, ironstones, fireclays, and soils, as also two ably written sketches "On the Present State of the Manufacture of Iron in Great Britain," and "On the State of the Steel Industry," both of which will repay perusal by those of us here who are interested in these matters.

Scattered through the purely geological portion of the Report are many points of interest, which arrest attention as one glances over the pages. Thus we are told that "at Zaleski, in mining the Nelsonville coal, a fine boulder of grey quartzite was found half imbedded in the coal, and the other half in the overlying shale. The quartzite is very hard, and the boulder was rounded and worn by friction before it came to the coal." It measured 17 in. by 12 in., and had adhering to it in places bits of coal and black slate which showed a slickensided surface. The stone appeared to have settled into the coal when the latter was in a soft state. Prof. Newberry speculates with diffidence on the possibility of the boulder having been "brought down by river ice from some higher and colder part of the old continent, which was skirted by the coal-producing lowlands." In connection with this it is somewhat interesting to find that a local deposit of quartz conglomerate occurs here and there underneath and skirting the coal-strata, and is believed by Dr. Newberry to represent an old beach of the period. From some such gravel and shingle deposit the boulder may have been transported, but whether by means of ice, water-plant, or land-plant, who shall tell?

Another exceedingly interesting and readable portion of the Report is the "Agricultural Survey," by Mr. J. H. Klippart, in which the writer discusses, amongst other subjects (such as prairies, forests, &c.), the origin of the soils in certain districts of the State. Those geologists who believe in the former existence during the Glacial epoch of mild interglacial periods will find much here to support their opinion. We are told that the succession of the Drift materials, beginning with the oldest, is as follows:—

a Glacial drift.
b Erie clays.
c Forest bed.
d Iceberg drift.
e Alluvium.
f Peat, calcareous tufa, shell marl.

The oldest deposit is believed to be the product of land-ice, and the presence of the Erie clays betokens that after the disappearance of the great glaciers, wide sheets of fresh-water overspread some districts of the State. The forest bed (consisting of roots, trunks, branches, and leaves of such trees as sycamore, beech, hickory, and red cedar) shows that by-and-by the fresh-water basins were in some places filled up, and the new soil covered with an abundant forest-growth. After this came a period of depression, when great deposits of gravel and sand gathered over the surface of the drowned land, and large boulders and erratics were floated by ice from the north.

These and other matters of interest and importance will, no doubt, be fully treated of in the final report, which is to consist of four volumes, the first two being devoted to the geology and palæontology of the State, the third to its economic geology, and the fourth to its agriculture, botany, and zoology. A large collection of fossils has been made, many species being new to science. It is to be hoped that the good people of Ohio will not grudge the money that will be required for the adequate representation and description of these remains, but that when published the final report will be found in every way as complete as those admirable works which have been issued by other States of the Union. Professor Newberry seems to have little doubt that it will be so, for he thinks that the value and significance of fossils are coming to be generally appreciated. "There are, however," he says, "yet some intelligent men, even editors and members of legislature, who cherish the notion that there is nothing which has any value in this world but that thing which has a dollar in it, and that so plainly visible as to be seen by them. Such men, to quote the language of one of them, 'don't care a row of pins for your clams and salamanders, but want something practical.'" This "practical" man must surely have been related to that colonial official who is said to have objected strongly to the expense of "engraved portraits of extinct bugs and beetles," as he irreverently styled certain Silurian fossils. But the day of such wiseacres has gone past, and it may be confidently expected that Dr. Newberry and his colleagues will have no difficulty in getting the necessary funds voted for the completion of their important Survey.

J. G.

OUR BOOK SHELF

Meibauer's Physische Beschaffenheit des Sonnensystems. (Berlin: Carl Habel.)

THIS is a second and freshly-arranged edition of a comprehensive little treatise on the nature of the solar system. It requires no great acquaintance with the present state of science to vindicate the accuracy of the author's preliminary remark, as to the difficulty that students experience from the wide dispersion of modern observations among heterogeneous memoirs and journals in various languages, and the necessity of a large library and abundance of leisure; and it is impossible not to appreciate his attempt to combine these scattered materials in a condensed and accessible form. Nor can it be doubted that a considerable amount of labour has been devoted to the work, which has been made attractive by perspicuity of treatment and facility of style, as well as by occasional ingenuity in hypothesis. Yet the execution cannot be said to correspond with the excellency of the design; and the deficiency, more apparent perhaps to our own minds than to those of Continental readers, is such as necessarily results from one-sided and imperfect views. The eternity of matter, an idea to many minds especially and utterly abhorrent, should not, to say the least of it, have been assumed; and other less objectionable hypotheses and statements are adopted, which may not be as incontrovertible as unwary readers will be led to suppose. No doubt the author, in employing as part of his motto the words of Darwin, "False facts are highly injurious to the cause of science," was quite unconscious that the result of an inquiry into some of his own facts (or rather assertions) would not be quite satisfactory. But we do not know what to make of such statements as these—that Priestley called his vital air (oxygen) by the name of Phlogiston—that Huggins found in the nuclei of comets the lines of nitrogen, hydrogen, and carbon similar to those given by the Geissler tubes—that there are two bright lines in the spectrum of Sirius, one of which is displaced by the star's movement—that the red, green, and yellow tints of the aurora never lose their relative positions; that the force of gravity at the upper limit of the atmosphere may be considered not materially different from that on the earth's surface, while the centrifugal (tangential) force perceptibly increases. Nothing but an unkind, or bitter, or self-ignorant spirit would refuse to leave a fairly broad margin for inevitable human imperfection; but it must be a very large paper copy indeed that would find room for statements such as these. Nor is it easy to understand why Lockyer's just claim should have been ignored to an equal share with Janssen in the grand discovery of prominences round the uneclipsed sun; or why discredit should have been thrown upon the connection of the solar-spot maximum with Sabine's magnetic period, or the planetary one established by the Kew observers. Other omissions might be pointed out, and the work would have been greatly improved by a discussion of the effects of temperature and pressure in modifying elementary spectra—a branch of inquiry to which allusion has barely been made, but which is of essential importance in spectrum analysis, and the fuller development of which alone, perhaps, promises a more satisfactory solution of many cosmical phenomena. But while it appeared a matter of duty to mention these deficiencies, we must add, in all fairness, and with greater pleasure, that some of his theories are very interesting and well handled; such as that in which he would account for the eruption of the protuberances by the unstable condition of gaseous matter on the confines of fluidity, discovered by Andrews and Thomson; or that of the unlimited extension through space of the planetary atmospheres in extreme tenuity; and there is much ingenuity, at any rate, in the idea of accounting for the variations of atmospheric pressure and electricity between the tropics by the resistance, however infinitesimal, which our globe sustains in its rapid passage through a space to which neither Newton nor Laplace ascribed absolute vacuity. The curious inconsistency with which, as a denier of equivocal generation, he calls in the germs of terrestrial vegetation from external space, where they have been educed under certain conditions of temperature, pressure, and time, is but a specimen of the difficulties to which every hypothesis is subject, that ignores the existence of an omnipotent will; but there are some who will look with amusement, and some few with a warmer feeling, at his vigorous onslaught on the idea of a luminiferous æther; concluding with the keen remark, that to prove the existence of such an æther, recourse is had in turn to the very phenomena which it was invented to explain.

T. W. W.

Knapsack Manual for Sportsmen on the Field. By Edwin Ward. (Bradbury and Evans, 1872.)

ONE who has come so much in contact with sportsmen as Mr. Ward must have done should surely know that men do not go out with knapsacks when intent on killing big game. The title "Knapsack Manual" is most unfortunate. Moreover, if the book is intended for sportsmen *on* the field, why should a considerable portion of it be given up to the mode of setting up a tiger, which a sportsman is very unlikely to do for himself, and certainly would not attempt in the field? Mr. Ward, though he seems to have considerable regard for artistic treatment and compatibility in the setting up of skins, would yet appear to put lichens with his stuffed birds in the conventional style. What a relief to the eye it would be to see a case of stuffed birds without a particle of dead wood or lichens in it! The directions given for skinning and preserving specimens are not full enough; there are better works on the subject in existence. The lists of game to be found in various parts of the world, at the commencement, form the most useful part of the book. The account of a Museum of Natural History of the Earth from man to a granite stone contained in a case 10 ft. long by 7 ft. high, displays a lamentable amount of ignorance. Some of the remarks about the various creatures are very amusing, as, "Gasteropoda proceed by the belly." "Armadillos are very remarkably swift in flight." Altogether this book appears to be of the nature of an advertisement, and we think a not very successful one.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Method of Least Squares

IN the number of NATURE for June 6, Prof. Asaph Hall, of Washington Observatory, called attention to what he regards as a singular oversight in the history of this subject, viz., that in 1770-1773 Lagrange published an elaborate memoir at Turin under the title "Mémoire sur l'utilité de la Méthode de prendre le Milieu entre les résultats de plusieurs Observations, &c." Prof. Hall remarks that the only notice of this memoir he has seen is contained in the *Berliner Jahrbuch* for 1853, and that in the abstract of a memoir of mine on the subject in the notices of the Royal Astronomical Society for April 1872, the name of Lagrange does not appear.

As regards myself, I need only state that Lagrange's memoir, as well as Simpson's, is referred to in my paper; although, as no examination is made of it there, the name is omitted in the Abstract, where reference is only made to the authors of investigations in which an attempt is made to prove either the law of facility or the method of least squares, and which were therefore referred to with more or less detail in the paper itself.

Further, I should not regard it as an omission if in the history of Least Squares no mention was made of Lagrange; in fact, when I was examining all the investigations I could find on the subject, after looking through Lagrange's memoir (and reading carefully Todhunter's *résumé* of it), I came to the conclusion that it contained nothing that could, properly speaking, be regarded as an anticipation of the later investigations of Gauss, Laplace, &c., and I contented myself therefore with merely a passing reference.

Lagrange's paper, as its title implies, gives a mathematical justification of the choice of the mean of a series of discordant observations, and a determination of the chance that the resulting error lies between certain limits, with developments, &c.; but the method of Least Squares may be described as an extension of the principle of the arithmetic mean to the combination of linear equations, involving more than one unknown; the problem being to obtain the best values of the unknowns from a series of discordant *linear simultaneous equations.*

The method of Least Squares was first proposed in print by Legendre in his "Orbites des Comètes" (Paris, 1805), as a convenient way of treating observations without reference to the Theory of Chance. Legendre's words are "la méthode qui me paroît la plus simple et la plus générale, consiste à rendre minimum la somme des quarrés des erreurs . . . et que j'appelle Méthode des moindres quarrés. The method, regarded from a practical point of view, is a very natural one; we shall clearly get a good result by determining the quantities to be found so as to make the sum of the $2m$th powers of the errors a minimum, and in order that the resulting equations may be linear (and therefore manageable), we must take n equal to unity.

Though first published by Legendre, the rule was applied by Gauss, as he himself states, as early as 1795, and the method is explained and the usual law of facility for the first time found in the "Theoria Motus Corporum Cœlestium, Hamburgh, 1809 (not 1808, as in Prof. Hall's letter). The principle on which Gauss proceeds may fairly, I think, be stated as follows:—If there are given a number of discordant observations V_1. V_2, &c., of a quantity x, so that we have the equations $x - V_1 = 0$, $x - V_2, = 0$, &c., then it is known that a very good result is obtained by giving to x the arithmetic mean of its observed values, and writing $x = \frac{1}{n}(V + \ldots + V_n)$; and it is required to find an equally good rule for determining x, y, z, &c., from a number of discordant equations of the form $a_1x + b_1y + c_1z + \ldots = V_1$, $a_2x + b_2y + c_2z + \ldots = V_2$, &c. Assume therefore that $x = \frac{1}{n}(V_1 + \ldots V_n)$ is the most probable value of x derived from the first system of equations, and find the law of facility of error that this may be the case; then, having this law, the most probable values of x, y, z, &c., can be found for the second system.

The law of facility Gauss finds to be represented by $\frac{h}{\sqrt{\pi}} e^{-h^2x^2}dx$, viz., this is the chance of an error of magnitude intermediate to x and $x + dx$; and thence it follows that the most probable values of x, y, z, &c., are found by making $(a_1x + b_1y + c_1z + \ldots - V_1)^2 + (a_2x + b_2y + c_2z + \ldots - V_2)^2 +$, &c., a minimum. Gauss then proceeds to determine h in the manner still generally adopted.

Subsequent writers, Laplace, Poisson, &c., have in consequence investigated how far the arithmetic mean is the most probable result, &c., and in one sense Lagrange (and *a fortiori* Simpson) may be said to have very slightly anticipated a portion of the analysis required in these researches, although, as far as the method of Least Squares is concerned, there is no anticipation. A slight examination will show how greatly superior Laplace's analysis is to Lagrange's on the same subject.

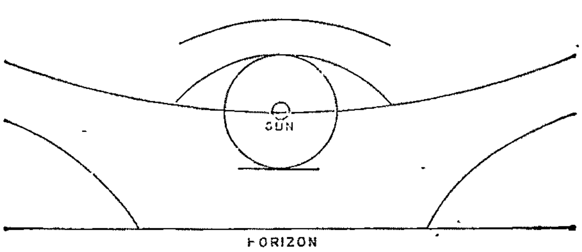
With reference to the independent discovery of the method of Least Squares by Dr. Adrain of New Brunswick, U.S. (see Prof. Abbe's note in the *American Journal of Science*, June 1871), I may remark that if for distinction we call the introduction of the merely practical use of the rule its "invention," and its philosophical deduction by the Theory of Probabilities its "discovery" (so that Legendre invented the method and Gauss discovered it), then Dr. Adrain can only be credited with the independent invention of the rule, viz , he only did what Legendre had done two years previously. This is worth noticing, as from the occurence of the function e^{-x^2} in Dr. Adrain's paper, it might be supposed that it contained some anticipation of Gauss' investigation; but such is not the case, and Dr. Adrain's reasons for the adoption of the law are of so trivial a nature that it is incredible that any mathematician should have been led to the discovery of the method by means of them. I imagine that he had noticed the practical convenience of the rule, and subsequently endeavoured to justify it analytically; it may be noted that it is possible that Dr. Adrain may have seen or heard of Legendre's memoir published two years before; his silence on the matter, however, renders it unlikely that this was so. On the whole, by far the greater part of the merit of the introduction of the method is due to Gauss; while the credit of the first suggestion of the practical rule must be assigned to Legendre, Dr. Adrain having, in all probability independently, also suggested the same rule subsequently. It is necessary to be thus particular, as Gauss' publication having taken place in 1809 and Adrain's in 1808, it might be thought that the latter had anticipated the former to some extent, which is in no wise the case.

In writing the history of the Theory of Errors or the Theory of the Treatment of Observations, there are several memoirs anterior to Legendre's that would have to be included, and notably Thomas Simpson's "Miscellaneous Tracts," 1757 (which is the work Prof. Hall doubtless refers to), Daniel Bernoulli's "Dijudicatio maxime probabilis plurium observationum discrepantium," &c. Acta. Petrop. 1777, Trembley's paper in the "Berlin Memoirs," 1801, "Observations sur la méthode de prendre le milieu entre les observations," &c. For the above references I was indebted to Todhunter's "History of the Mathematical Theory of Probability from the time of Pascal to that of Laplace" (London, 1865), which contains a notice of every work or memoir on the subject to the commencement of the present century (there is a *résumé* of Lagrange's memoir occupying 13 pages), so that no one need have any fear of passing over any writings published previously to 1800. Having had occasion to make much use of the work, I may be permitted to say that its value, both as regards accuracy and completeness, cannot be over-estimated. J. W. L. GLAISHER

Trinity College, Cambridge, June 8

Solar Halos

A BEAUTIFUL combination of solar halos was visible here during the morning of March 2. At 10·45 the sun having an altitude of about 40° was surrounded by a complete rainbow-tinted circle of some 18° or 20° radius, red inside and blue outside. An arc of a larger circle coloured in the same way touched the complete circle at its highest point, rendering the point of contact dazzlingly bright. A short arc touched the lowest point of the circle in the same manner. A white halo passed through the sun's position parallel to the horizon, and two fainter white arcs intersected it obliquely in the point opposite to the sun, forming a conspicuous sun-dog. There were also two rainbow-arcs having their convexities toward the sun. These were blue inside and red outside, and their centres appeared to be about 90° from the sun, and some 15° below the horizon. Later an arc concentric with that touching the complete circle appeared above it, having the colours reversed, namely, blue inside and red outside. These appearances lasted about an hour and a half before beginning to fade away. W. W. J.



Gambier, Ohio, March 5

The Volcanoes of Central France

THE Auvergne volcanoes threaten to be as periodic a subject of controversy as the authorship of the letters of Junius. It is only seven years since the last eruption of letters. At that time I contributed a paper to the *Geological Magazine* (vol. ii. p. 241), in which I collected, printed, and translated all that I could find on the subject, and came to the conclusion that it was very probable there had been some local outbreak of volcanic action. Thus I agree with Mr. Garbett, but it appears to me that he has not made his case in one respect so strong as it might be. In the passage "nunc ignes sæpe flammati *caducas culminum cristas* superjecto favillarum monte tumulabant" (as the edition which I follow has it) he translates *culmina* "roofs," and again in the parallel passage of Avitus. I think it more likely to mean summits (of mountains), and to refer to the formation of one or more new cones in the hill country.

My reason for this may be given in the words which I used in the paper above named—"though Sidonius is inclined to bombast, he scarcely seems equal to a flight like this. . . . In the parallel passage in Avitus, the reference to Isaiah ii. 10, 19, 21, and Luke xxiii. 30 appears too clear to allow any other meaning than mountain-top to be assigned to *culmen*." To this I may add that the ridge-roofs, *cristæ culminum*, would be those least likely to be broken by a shower of ashes, and the ridges would be the part where the smallest quantity of ashes would rest.

T. G. BONNEY

St. John's College, Cambridge

Force and Energy

REFERRING to Mr. Brooke's article in NATURE of the 13th on Force and Energy, I would suggest that though it is quite true that heat is a "mode of motion," this is probably not true of magnetism and static electricity. Heat is molecular *motion*, magnetism and static electricity are molecular *tensions*.

I would also remark that the term "radiant heat" ought to be discarded as misleading. Radiant heat is not a kind of heat; it is quite distinct from heat, but it is nearly identical with light. We ought to introduce the word *radiance*, and then we get to this statement:—All rays of radiance have more or less heating power, and some of them have also the power of producing the sensation of light. But the fact that only some rays, and not those which have the most heating power, produce the sensation of light, belongs rather to the retina than to the rays.

Mr. Brooke thinks the proposition that the sum total of energy in the universe is unchangeable is incapable of proof. I do not speak as having any authority, but it seems to me that if this is not true the conservation of energy cannot be universally true.

JOSEPH JOHN MURPHY

Pelagic Fish-Nest

SEEING an extract from NATURE with reference to the nest of the pelagic fish, allow me to inform you of the discovery of what I presume to be a similar nest in lat. 25° N., long. 65° W., whilst on a voyage between Buenos Ayres and New York last January. I had improvised a drag-net out of a barrel hoop and a biscuit bag, to fish up for examination the straw-coloured floating gulf-weed, which covered the sea in long lines and patches between 20° and 32° N. lat.; and one day there came up in the net a mass of weed compactly woven by strong, white, silky fibres into a round ball of about ten inches in circumference. The surface of this ball was covered with a network of these fibres, to which large numbers of glassy eggs, about the size of partridge shot, were attached. The eggs were transparent, and their cases very tough. The only living inhabitants of the ball were one or two small shrimps and a small crab, who was carrying his own particular egg-sac.

Another curious fact I am tempted to mention. About 200 miles from Cape Frio, the sailors caught a dolphin, which had in its stomach twenty pieces of coal, varying from a large walnut to a marble in size, together with the heads of four iron nails about an inch in length each. I am tolerably certain that these articles had not been thrown from our vessel, but they did not appear affected by the internal wear and tear, however long they might have been digesting.

GEORGE J. HINDE

Toronto, Canada West, May 18

Why are Red Sandstones Red?

I HAVE lately been interested in the reply to this question given by Prof. Ramsay, and stated by Prof. Geikie in his recent edition of "Jukes's Manual of Geology" (pp. 567, 568). But the explanation, viz., that the red colour is derived from the precipitation of red (consequently anhydrous) peroxide of iron in inland seas, appears to me to give rise to this other question—Why should the precipitated peroxide be anhydrous, and not hydrous and brown, as is the case with limonite, which is found deposited in marshes, ponds, and lakes?

I have tried some experiments in precipitating the peroxide of iron from a solution made as saturated as possible by long boiling of water or oxide of iron (obtained from a natural spring), common salt, and finely divided sulphate of lime (these last two minerals being found to accompany the red rocks), filtering hot, and allowing to stand till cold. For want of experience in these matters, probably, I have not yet succeeded in obtaining any red colour.

I have, however, to-day fallen on a paper describing a similar experiment to account for the presence of anhydrite in the Stassfurt mines. In this case it is stated that the *anhydrous* sulphate of lime was obtained on evaporating a concentrated solution of gypsum and rock salt.

I should be glad to learn whether the attention of any of your readers has been drawn to this question, and whether they have succeeded in obtaining (under conditions analogous to those of an evaporating inland sea) a precipitation of the red colouring matter.

A YOUNG GEOLOGIST

Mounting of Thermometers

I HAVE experienced precisely the same inconvenience as that mentioned by Mr. Whipple in NATURE last week.

I several times removed the outside case of a thermometer such as he describes, and took every precaution to dry the air before replacing the packing, but the moisture in the tube persistently reappeared. It then occurred to me that the amount of moisture was out of all proportion to the quantity of air confined, and that the mischief arose from the packing not being air-tight; and fresh damp air was thus continually finding its way into the tube, and depositing moisture. Accordingly the tube was again removed, and after drying carefully, I replaced it, and pushed in the india-rubber packing about an eighth of an inch. The intervening space was filled up with common putty, which was made to assume a conical form round the thermometer stem. After being left for a day or two to harden, the putty was painted over with two or three coats of sealing-wax dissolved in alcohol. This thermometer has been constantly exposed on the grass for about four months, and though I purposely took no means to dry the air in the case, not the slightest inconvenience from a deposition of moisture has since been experienced.

REGINALD BUSHELL

Hinderton, Neston, Cheshire, June 17

A Few Millions

IN your reprint of Prof. Mayer's paper, entitled "Acoustical Experiments" in NATURE for May 9, 1872, there occur some strange numerical errors, which perhaps it will be well to point out, lest some of your readers should make use of the numbers given at the end of the paper without previously testing them. After describing his experiments, he proceeds:—"We will now examine the analogical phenomena in the case of light:—Let fork No. 1, giving 256 vibrations a second, stand for 595 millions of millions vibrations a second, which we will take as the number of vibrations made by the ray D_1 of the spectrum." Taking the velocity of light as 185,300 miles per second, and the wave-length of D_1, as given by Angstrom, at 0·00058950 millimetres, gives 5,058,700,000,000,000 vibrations per second, or a little more than five thousand millions of millions, instead of a little less than six hundred millions of millions vibrations per second, as given by Dr. Mayer. But to proceed—"Then fork No. 3 will represent 590 millions of millions vibration per second," this should be 594 millions of millions vibrations, "which give a wave-length ·0000042 millimetres longer than D_1." This again is not quite right, even according to Dr. Mayer's own showing; it should be ·00000495 of a millimetre longer than D_1. Dr. Mayer then goes on to say that such a wave-length nearly corresponds with an iron line situate ·42 div. below D_1 on Angström's chart; and "we saw that fork No. 3, giving 254 vibrations a second, had to move toward the ear with a velocity of 8·734ft., to give the note produced by 256 vibrations per second emanating from a fixed point; so a star sending forth the ray which vibrates 590 millions of millions a second will have to move toward the eye with a velocity of 28,470 miles per second to give the colour produced when ray D_1 emanates from a stationary flame." This again, according to Dr. Mayer's own method, should be 1,557 miles, or less than a nineteenth of the velocity given by him.

Instead of involving ourselves in millions of millions, and the translation of millimetres into English miles, it seems simpler to avoid the calculation of the number of vibrations per second, and to get at the required velocity by a simple rule-of-three sum, thus: As the emitted wave length is to the difference between the observed wave-length and the emitted wave-length, so is the velocity of light to the required velocity, to or from the observer.

A. COWPER RANYARD

PROF. CANNIZZARO'S FARADAY LECTURE

THIS lecture was delivered on May 30, by Prof. Cannizzaro. The lectureship was founded by the Chemical Society in honour of the illustrious Faraday, to be held by some eminent foreign *savant*, who, during the term of his tenure is to deliver a discourse before the Society. Dr. Frankland, in introducing the lecturer, said that in 1869, M. Dumas had honoured them with his presence there, and on that night they were to listen to Prof. Cannizzaro, of Palermo. After alluding to the numerous investigations which the Professor had made in organic chemistry, and amongst others the discovery of benzylic alcohol, the first normal aromatic alcohol that had ever been prepared, and to the important theoretical views which he had originated, the President, in the name of the Society, presented to him the Faraday Medal, struck in honour of his visit.

Prof. Cannizzaro said that when he received the flattering invitation to deliver the Faraday Lecture, he was placed in very unfavourable circumstances to respond to it, as he had no definite results to lay before the Society, and was, moreover, on the point of suspending his labours and abandoning his old laboratory in order to remove to Rome, and establish a new one there. In this difficulty a subject for a discourse fortunately presented itself, one which the celebrated French chemist, Dumas, had promised to treat of in 1847, namely, the form which the theory of chemistry should take at the present time. Although this could not be fully discussed in so short a space of time, it would at least have the advantage of directing the attention of chemists to a question of great importance in the transition stage which our science is at present going through.

In recalling the promise which M. Dumas had made to the Academy of Sciences of Paris in 1847, to examine the form which theoretical instruction in chemistry should take in the present state of the science, the lecturer proposed to consider in his discourse the limits within which the exposition of general theories should be included in teaching chemistry, and the form that it was desirable that they should assume. Whilst giving a broad sketch of the progress of modern chemistry, he showed that the atomic theory had become more and more intimately interlaced with the fabric of chemistry, so that it is no longer possible to separate them without rending the tissue, as it were, of the science; and that up to the present time we have been unable to enunciate even the empirical laws of chemical proportion, independently of that theory; for those who employ the term equivalent in the sense that Wollaston did, commit an anachronism. Consequently, in the exposition of the value and use of symbols, formulæ, and chemical equations, not only are we unable to do without the atomic and molecular theory, but it is inconvenient to follow the long and fatiguing path of induction which leads up to it. By one of those bold flights of the human mind we can at once reach the height whence we discern at a glance the relations between facts.

He then went on to show that the solid basis, the corner-stone of the modern molecular and atomic theory, the crown of the edifice of which Dalton laid the foundation—is the theory of Avogadro and Ampére, Koenig and Clausius, on the constitution of perfect gases, to which chemists, unknown to themselves, have been led in the progress of their science. He thought the time had arrived for reversing the order which had hitherto been followed in teaching chemistry, that instead of setting out from the criteria for determining the weight of molecules, and then showing their ratio to the vapour densities, they ought, on the contrary, to commence with the latter, with the theory of Avogadro and Clausius, demonstrating it from physical considerations; to found upon that the proof of the divisibility of simple bodies, that is to say, the existence of atoms; and to show, as occasion presented itself, that the weights of the molecules and the number of the atoms deduced by the application of this theory, are in accordance with those which are deduced from chemical criteria. By this means we can measure the degree of confidence to be placed in the latter criteria; since so-called compound equivalents do not suffice to determine the weight of molecules, or even to prove their existence, although they may be deduced from a single principle, the theory of the constitution of gases. This is the natural transition from physics to chemistry.

The Professor then stated in detail how he applied the principles he had laid before them. He introduced his pupils to the study of chemistry, by endeavouring to place them on the same level as the contemporaries of Lavoisier, and to teach them to appreciate the importance of the principle of the conservation of the weight of matter, showing them that this is quite independent of any idea of its nature or constitution. They are thus led to examine the ponderable composition of substances, so that the student passes rapidly from the epoch of Lavoisier to that of Proust, and then to that of Berzelius at the time when he commenced his researches on proportions. At this stage the same impulse is given to the pupil as Berzelius received on becoming acquainted with the hypothesis of Dalton. The latter is laid before him without any accessory, the use of symbols and formulæ being introduced dogmatically. There will now arise in his mind the same doubts and difficulties that assailed Berthollet, Sir Humphrey Davy, and Wollaston in the application of Dalton's theory, and at the same time a desire for an explanation of the simple relation which exists between the vapour volumes of bodies which react on one another and of the products which are obtained. Now is the moment to state or recall to mind the physical theory of the constitution of the perfect gases. Commencing with a rapid glance at their general and special characters, he insisted, that in this part of the instruction the mind of the student should not be diverted from the numbers expressing their relations, by considerations of the variations caused by changes of temperature and pressure. In applying the theory of the constitution of gases, it will be perceived that the molecules of simple bodies are not always the atoms of Dalton, and a certain confusion will thus be produced in the mind of the beginner in the conception of the ideas of atoms and molecules. The hypothesis of Dalton can now be laid aside, substituting as a starting-point the theory of the relation of molecular weights to the vapour densities. A table must be prepared of the vapour density compared with that of hydrogen as 2, that is to say, the weights of their molecules compared with the weight of the semi-molecule of hydrogen taken as unity. We must then compare the composition of the molecules containing the same element—including, or not, the molecule of the element itself—and thence deduce the law of the existence of atoms, that is to say, the amount of each element which always enters, by whole multiples, into the molecules which contain them. We here have the atoms of Dalton which, in the present state of the science, express not only all that Dalton discovered, but also the composition of equal volumes of their vapours, and in the *choice* of which those doubts can no longer arise which embarrassed Davy and Wollaston. The ideas of molecules and atoms suggested to the student by this law are devoid of all considerations of form, size, continuity, or discontinuity; the only property indissolubly connected with them is that of ponderability; the very definition of matter.

Recollecting that no physical theory of the constitution of matter had yet been advanced which thoroughly conformed to chemical ideas, he insisted upon the advisability in teaching the molecular and atomic theory, to keep it free from all that is not absolutely essential, so that it may preserve sufficient plasticity to adapt itself to the progress of our physical and mathematical knowledge. For this purpose he thought it useful to allow the student in the first place to glance at the changes in the hypothesis of the constitution of matter, and then to cause him to estimate the degree of confidence they merit in the actual state of our knowledge. Having thus placed upon a solid basis the fundamental notions of atoms and molecules by the comparison of the composition of equal volumes of the bodies in the gaseous state, it becomes necessary to consider the difficulties which arise in the application of these notions when the vapour densities are wanting; he explained and justified the use of the various auxiliary criteria to which we have resource in these cases, proving them in the first instance by the touchstone of the theory of Avogadro and Clausius, by showing that they gave results in accordance with that theory whenever the two methods can be employed simultaneously.

He believed that we should never lose sight of the starting point, nor give the formulæ of all compounds as of equal probability. "It is not by concealing the obscurity of these questions that we shall enlighten the student; on the contrary, we should estimate each fact at its true value by showing him that our science does not merit an equal degree of confidence on all points." This forms the introduction, the preparation for the study of the transformations which matter undergoes; the real object and aim of our science.

The comparison of the atomic composition of molecules has led chemists to the law of substitution, to the theory of types of Dumas, then to that of Williamson and Gerhardt, and lastly to the theory of the different valency of atoms and their modes of union, or the so-called theory of atomicity which includes the former. Although at present it is impossible, in teaching chemistry, entirely to eliminate this latter theory, which gives a summary of several laws, and guides us ordinarily in the co-ordination and even prevision of a large number of facts, yet it is difficult to keep it within just bounds so as to avoid infusing into the mind of the beginner illusions which are dangerous for his intellectual education. In order to avoid this, it is advisable to bear in mind the progress of this doctrine and the actual phase of development which it has at present reached. It is still far from being a complete and well-established theory, but is in a state of transition; for although doubtless it embraces a large number of facts, as yet it does not embrace them all. It is only a partial representation of the reality, and that from a restricted point of view, showing but little relation to our views of the constitution of matter, for it is the result of a comparison of diverse facts expressed by means of the atomic and molecular theory. It is convenient, therefore, to consider each part of this doctrine exclusively in relation to the group of facts which has suggested it.

It is unadvisable to define the valency of atoms as a property inherent in them, and then to deduce as a corollary their different modes of union; on the contrary, it is preferable to regard each portion of this doctrine as a deduction from the observation and comparison of a determinate group of facts, until an opportunity offers to unite these fragments into one whole, not forgetting, however, to notice the gaps which exist, never going beyond what the facts themselves suggest, and never applying to all bodies indiscriminately, the laws which suit only a single group. For instance, we must not pass over in silence the facts that whilst certain elements are bi tetra- or even hexa-valent, others are tri- and penta-valent; but the pupil should be prevented from acquiring mechanical and geometrical ideas of the cause and effects of the valency of atoms, by frequently reminding him that chemical facts show nothing about the size, form, continuity, or relative position of atoms. If we are sometimes obliged to employ the expression, "relative position of atoms in the molecules," and even to represent them graphically, we must warn the student that these are only artifices to express certain transformations, and that we are really ignorant of the relative position of the atoms either in space or in the mutual action of different portions of matter. With these reservations, it is possible, in teaching to derive considerable advantage from the theory of atomicity and at the same time to avoid its inconveniences.

In the study of the transformations which matter undergoes, we should direct the pupil's attention, not only to the ponderable changes in the composition of molecules, but also to the electrical and calorific phenomena which accompany these transformations. Even from Lavoisier's time it has been recognised that we cannot separate the study of matter from thermic considerations; and every day the connection which exists between chemical and thermic phenomena becomes more apparent.

As in the study of ponderable changes we were guided by the law of the conservation of weight, so in the connection between chemical and dynamical phenomena we are guided by the law of the conservation of force; the two studies mutually supplementing and illustrating one another. Not only will the atomic and molecular theory and that of atomicity help us to compare dynamical phenomena, but the study of dynamical phenomena will show us analogies and differences between chemical actions which would not be observed in the ponderable equations. We should therefore instruct the student in the little definite knowledge which we at present possess concerning thermic and electric phenomena, and especially fix in his mind the fundamental notion of a mechanical equivalent, and the manner of comparing it with chemical action as expressed by the atomic theory. In this we should be aided by the previous or simultaneous instruction of the student in physics under the form and language of the thermodynamic theory.

The lecturer concluded by observing that in the choice of methods and of matter for a course of chemistry, it should always be borne in mind that it was eminently a progressive science, and that even at the time of its most rapid development. The student should start not only with a knowledge of certain definite and fixed principles, but with an aptitude and sufficient preparation to enable him to follow the science in its unceasing transformation and progress, whether he intends expressly to cultivate chemistry, or has only learnt the elements of the science as an auxiliary to other studies or professions. Moreover, the end of chemical instruction for both these classes of students is not only to fix in their memory a certain amount of knowledge, but to assist in their intellectual education. For this, chemistry of all sciences is one of the best, offering both in verbal and practical instruction—excellent occasions for the exercise and harmonious development of all the faculties of the human mind.

He had desired to call attention to what he considered to be the most efficient means of imparting a knowledge of chemistry so that it might serve as an instrument of intellectual education, and that the student, by following it in its ulterior developments, might judiciously apply it to the study of the other branches of natural science. If the attention of the eminent chemists and professors there present were once attracted to this subject, he felt certain that a bright light would be thrown on it, and that our young professors would find numerous suggestions to direct them in teaching chemistry, and that at the very moment when instruction in our science had become so difficult, on account of the rapid transformation which it was undergoing.

Dr. Williamson said, that those who were there present ought not to separate without some expression of the pleasure that they had felt on listening to so learned, vast, and eloquent a discourse, treating as it did of a most difficult and important problem. There was scarcely anything of greater moment in the scientific education of youth than the rightly setting before them those wonderful transformations of matter which it is the province of chemistry to explain. These great and growing truths, for, as the lecturer had said, they were growing truths, should be set before youth in such a manner as to form a coherent whole. He hoped to study this masterly discourse with profit and delight, and would now propose a vote of thanks to his illustrious colleague for the honour which he had done them in delivering to them the Faraday lecture.

Prof. Tyndall said he had heard the discourse with deep interest, for it showed that the lecturer knew the importance of a teacher's vocation, and that his province was not merely to communicate knowledge, but to do it in such a manner as to arouse an interest in and love of the subject in the pupil by presenting it in its proper relations. He would have welcomed the lecturer to that Institution, even had he come to tear in pieces the notions which he cherished regarding atoms and molecules; how pleasant it was then to find such a broad agreement between their views. The chemist cannot halt at equivalent proportions—he must ask himself whence they arise, and the inevitable answer is some form of the atomic theory. This theory, however, cannot be confined to chemical phenomena. The motions of those atoms and molecules underlie all our explanations of the physical cause of light and heat, and it is already taking up the field of magnetism and electricity. Consider, for example, the heat of gases, both as regards the motion of translation of the molecules which produce temperature, and the motions of rotation and vibration of their constituent atoms, which, though they do not express themselves as temperature, constitute a portion of the heat. Clausius has shown that even in the simplest gases nearly two-fifths of the whole heat is due to these internal motions; while in gases of complex molecular constitution which condense on combining, the ratio of the total heat to the heat of temperature is still greater. The experiments of Regnault, which show that the specific heat of a perfect gas at a constant volume is constant, proves, as Clausius has shown, that the one kind of motion is proportional to the others.

The lecturer had also referred to atoms of the same kind combining together, so that, free oxygen and free hydrogen being considered as composed of molecules each containing a pair of atoms, has certainly simplified the results. But it must not be forgotten that this combination of like atoms is widely different from that of unlike atoms. The union of oxygen with oxygen or nitrogen with nitrogen produces no such effects upon the luminiferous ether as the union of oxygen with nitrogen. With the same quantity of matter the amount of *vis viva* sent forth as radiant heat may be augmented a thousandfold, perhaps a millionfold, by the act of diverse combination. This act seems to carry with it a condensation of the ether to a dense atmosphere around the atoms. Had a cannon the power of gathering round itself a dense atomsphere, it would send forth a greater amount of *vis viva* as sound. A gun fired at Chamouni may be heard upon Mont Blanc, while the same gun fired on Mont Blanc may not be heard at Chamouni, because the air on which the concussion takes place is denser in the one case than in the other. In the same way the diverse atoms vibrating in the denser atmosphere formed on combination show their vast superiority as radiators over like atoms which, except in such special cases as ozone, &c., are incompetent to produce a similar condensation. The speaker then asked them to echo the resolution so well put to the meeting by Prof. Williamson.

THE OBSERVATORY ON MOUNT VESUVIUS

WHILE the scientific world and his own countrymen are rivals in doing honour to Prof. Palmieri for his zeal in remaining at his post in spite of all danger, it may be interesting to examine in some detail the work done at the Observatory of Mount Vesuvius. We know wonderfully little about the origin and mutual dependence of volcanic phenomena. This is due to a want of accurate observations. For the complete investigation we require first to know at what dates earthquakes and eruptions occur at different parts of the earth. Next we must have observations of the direction and exact hour at which a wave of disturbance passes different places whose positions are known. This gives us the velocity of the wave, and helps to determine the position, under the earth's surface, of the centre of disturbance; or, if a wave be propagated over the sea, we obtain a means of estimating the average depth of the intervening ocean; for the velocity of a wave increases with the depth of the sea. This method gives one of the best determinations we possess of the depth of the Pacific Ocean. But beyond this we must have observations made systematically at some place subject to earthquakes and volcanic eruptions. No place in Europe is more suitable for this than the neighbourhood of Mount Vesuvius; and it was for such observations that an Observatory was established there.

Everyone knows that Mount Vesuvius consists of a great cone of lava and ashes, at the top of which is the great crater. On the northern side, separated from it by the deep valley called the Atrio del Cavallo, rises the precipitous and semicircular Monte Somma. This once formed the crater of the volcano; and the present cone seems to have been formed inside that great crater at the time when Pompeii was overwhelmed. On a spur of rock, a mile or two in length, running down from the Atrio del Cavallo, the Observatory is placed. It is close to the well-known "Hermitage," or half-way house, in the ascent of the mountain. Being raised on this ridge above the surrounding country, it is comparatively safe from the molten lava that flows at times on either side of it.

The building itself is handsome; in fact it is to be regretted that so much money should have been devoted to the masonry instead of to additional instruments. On the ground floor are the inhabited rooms, all scantily furnished; but the pursuers of science cannot always expect bodily comfort. On the first floor we find the Museum, with a fine collection of minerals found on the mountain. Perhaps it may be as well here to correct a common mistake as to the nature of the yellow substance found about the craters, whose brilliant colours remind one so much of the Solfatara. This substance is not sulphur, but copper. The most interesting objects in the Museum are the "fumerolles," or smoke-holes. Occasionally at the end of an eruption you may see at the bottom of the crater a small cone of lava, with a hole in its top, through which the steam pours with a hissing noise like a wave breaking on a pebbly beach, or like a blast furnace, or as Pliny has it, like the grinding of a saw; the intensity of the sound varying with your position. These small cones are the fumerolles; they are a foot or two high; and Palmieri has actually had several of these natural chimneys cut off and transported to the Museum.

We now pass on to the Observing Room. There are solid piers carried up from the ground to support the instruments. First comes the elegant seismograph for the automatic registration of earthquake shocks. The object of the instrument is twofold: first to measure the direction and intensity of a shock; and, second, to write down a history of the earthquake. The shock may be either vertical or horizontal, or partly vertical and partly horizontal. For the vertical shocks a fine metallic point is suspended by a coil of wire over a cup of mercury. The coil of wire acts as a spring, and the slightest upward motion of the earth is sufficient to cause the point to dip into the cup of mercury. This completes a galvanic circuit, which stops a clock at the exact half second at which the shock occurred, and rings a bell to call the observer, and also does other work which we shall speak of again. There are three or four helices of wire of different strengths, which support small magnets above a cup of iron filings. When a vertical shock occurs some of these magnets dip into the iron filings. To one of these a light index is attached for measuring the intensity of the shock.

For horizontal shocks there are four glass tubes. Each of them is bent twice at right angles, so as to form a U tube. One arm of this tube has more than double the diameter of the other, and is shorter. The four tubes point in the directions of the four cardinal points. Each tube has a certain quantity of mercury poured into it, and on the surface of the mercury, within the narrow arm of the tube, there rests a small weight attached to a silk fibre, which passes over a delicate ivory pulley, and has a counterpoise attached at the other end. Each pulley has an index and circular scale to mark the angle turned through. The extremity of a wire is fixed at a small distance above the surface of the mercury in each tube. If then a horizontal shock occur, the mercury rises in the corresponding tube; but it rises higher in that one which has its long arm to the north. The pulley is turned through a certain angle, which is measured by the index, and at the same time the mercury in rising comes in contact with the fixed wire, and so completes a galvanic circuit which rings a bell, and stops the clock at the exact half second when the shock occurred. If the shock comes from some intermediate point two of the indices will be moved, and the direction and intensity can be measured by observing both of them. We have seen up to this point that the instrument will measure the direction and intensity of a shock, will mark the time at which the shock occurred, and will ring a bell to attract the attention of the observer on duty, who may register succeeding shocks, or, if the earthquake has ceased, may reset the apparatus. But this is not all. The galvanic circuit, which is completed at the moment a shock occurs, releases at the same instant the pendulum of a second clock, which has been held out of the vertical by means of a detent. This clock allows a roll of paper to be unwound off a drum, as in any registering telegraph, at the rate of three metres an hour. A pencil rests nearly in contact with the strip of paper. It is connected with one arm of a lever, the other arm of which is slightly distant from an electro-magnet. As often as the current passes this end of the lever is attracted to the magnet, and the pencil in consequence is made to press on the paper, to be released only when the current ceases. By this means then a continuous history of the earth's trembling is registered, a pencil mark corresponding to a time of trembling, and a blank space to a period of cessation.

This instrument is extremely delicate, and registers motions of the earth which are too slight to be perceptible to the human frame. When we examined it some one happened accidentally to touch the casing of the instrument. The alarm was immediately given by the bell, and the two clocks were respectively checked and put in motion by the galvanic current.

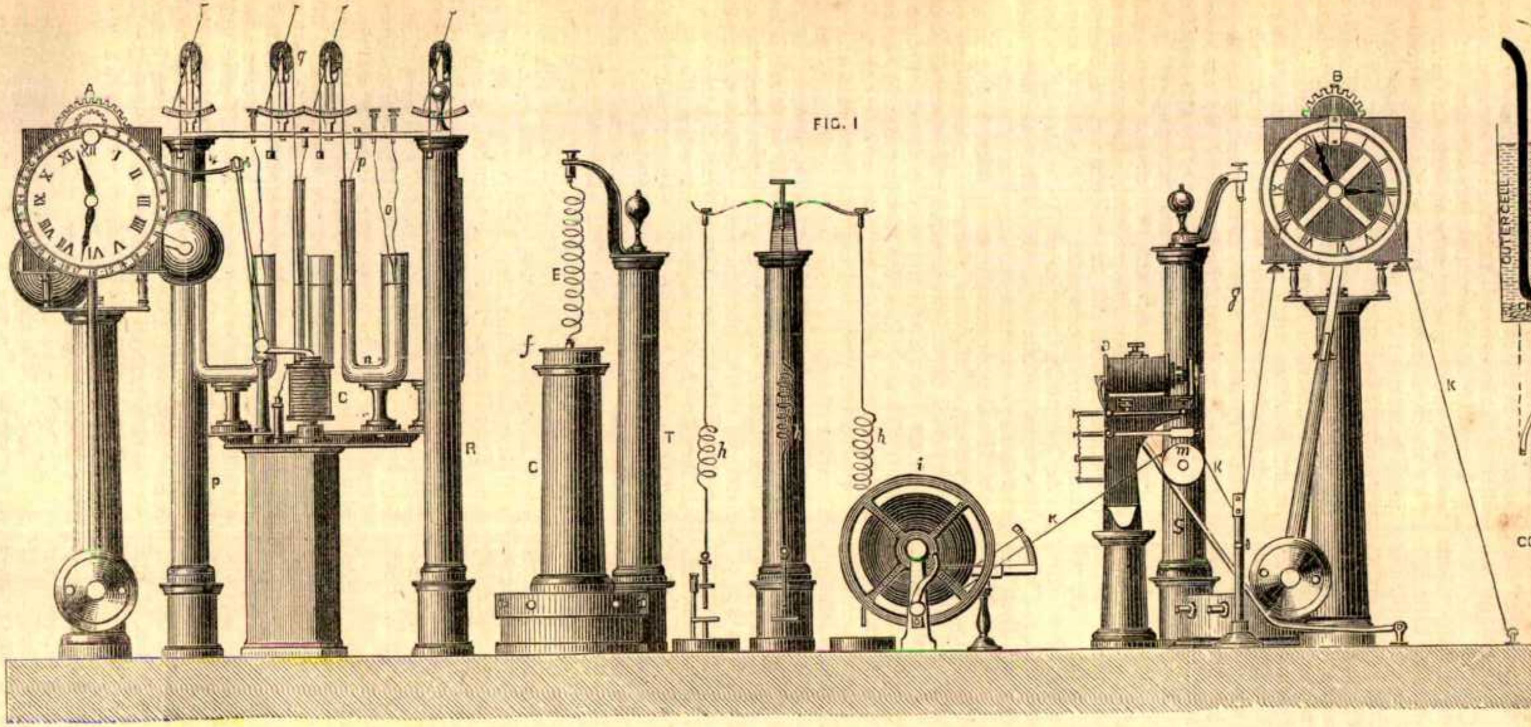
The accompanying figure (borrowed from the *Engineer*, for the use of which we are indebted to the courtesy of the editor of that journal) may help to make the above description more intelligible. In Fig. 1, the clock A is shown with the pendulum arrested, as after a shock has occurred. The pendulum of the clock B is in a position ready to be set free when a shock occurs. At the same time the strip of paper *k k k* will be rolled on to the cylinder *i*, and at each trembling of the earth the electro-magnet D will cause a pencil to make a mark on the paper at the point *m*. P and R are two pillars, between which are shown the U tubes containing the mercury, the pulleys and indices are shown above. These pillars and tubes are also shown in plan. Metallic bars are seen connected with R, and passing over the short arms of the tubes. From these hang the wires that dip into the mercury. From the pillar P, metallic bars are also shown passing over the long arms of the tubes; to these are attached the wires that are almost in contact with the mercury, and which complete the circuit when a shock occurs. The metallic spring E, supported by the pillar T, above the cup of mercury *f*, is the apparatus for making a current during a vertical shock; *h h h* are the springs with magnets attached, which dip into iron filings. The index for vertical shocks is shown more clearly in Fig. 2.

For more violent shocks the heavy bob of a freely suspended pendulum is placed in the centre of a horizontal ring in which eight tubes are placed lightly, all pointing to the centre. From whatever direction a horizontal shock comes it will drive out one of these tubes. The tube driven out will show the direction of the shock, and the distance to which it is driven will show the intensity. This is also shown in plan (Fig. 3). The hole for the tube is also represented. There is also shown in section a cup of mercury, placed at the foot of the pillar G (Fig. 1), which has eight holes in its circumference just above the surface of the mercury. When a shock occurs mercury is driven out into the hole corresponding to the direction of the shock. The quantity of mercury determines the intensity. The battery is shown in Fig. 4, and needs no explanation.

In the same room there is apparatus for detecting and measuring atmospheric electricity. A gold leaf electroscope and a bifilar electrometer are observed regularly. These are successively put in connection with the conductor. This consists of a disc of metal above the roof of the house connected with an insulated metallic rod, supported vertically, and capable of being rapidly raised by means of a cord passing over a pulley. When not in use this rod is in connection with the ground. In making an observation the rod with the disc attached is quickly raised, thereby disconnecting it from the ground. The electricity of the atmosphere at the point where the disc is fixed affects the electroscope and electrometer. Prof. Palmieri prefers the conductor above-described to a conducting point or a flame, because he considers that these do not give comparable results, an objection which is not supported by all observers. He considers the same to be true of the method of dropping water.

After having made careful observations on atmospheric electricity for about a quarter of a century in a country where meteorological changes are more regular and less capricious than in our own island, there is no one whose deductions are more deserving of our attention; the more so as he considers that he has combined his researches into a definite law. His first fact is this:—*If within a distance of about fifty miles there is no shower of rain, hail, or snow, the electricity is always positive.* The single exception is during the projection of ashes from the crater of Vesuvius. During a shower he finds the following law universally to hold good:—At the place of the shower there is a strong development of positive electricity; round this there is a zone of negative, and beyond this again positive. The nature of the electricity observed depends upon the position of the observer with respect to the shower, and the phenomena will change according to the direction in which the shower is moving. Sometimes negative electricity may be observed during a shower; but this is always due to a more powerful shower farther off. These conclusions have been supported by means of telegraphic communication with neighbouring districts. It appears, then, that except when the moisture of the air is being condensed, there is no unusual development of electricity. These results are in accordance with the experiments of Palmieri and others, which show that

FIG. 1



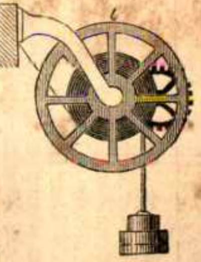
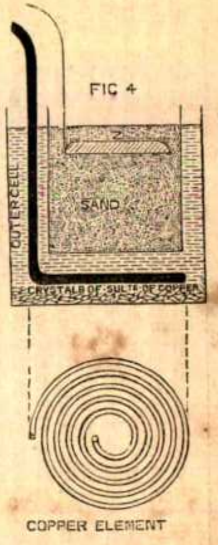
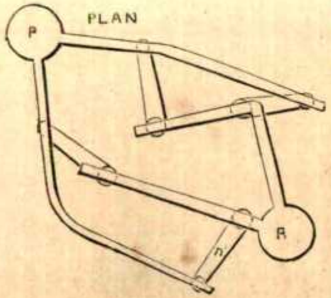
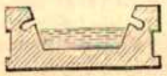


FIG 4



COPPER ELEMENT





SECTION

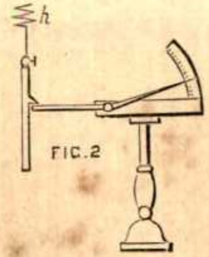


FIG. 2

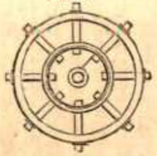


FIG. 3

HOLE FOR TUBE

aqueous vapour in condensing developes positive electricity. No unusual development of electricity has ever been detected by him in a cloud when no rain is falling.

The above results, though falling short of what has to be done to complete the theory, are yet definite, and hence valuable, the more so if supported by other observers placed in equally favourable situations. But of the variations *in intensity* of positive or negative electricity nothing has been said.

Besides the fixed instruments at the Observatory others are used on the mountain. Gases are collected from cracks in the earth's crust, tubes being let down into them, and the gas sucked up by a kind of bellows to be examined at leisure. A portable spectroscope is also used during eruptions, and there is a larger one by Hoffman in the Observatory. From this Observatory we have received valuable information, and it is much to be regretted that equally efficient observatories have not been established in different parts of the world. Many portable and cheap instruments have been invented, most of which are described by Mr. Mallet in the "Admiralty Manual of Scientific Inquiry;" but there ought to be three or four as delicate as that on Mount Vesuvius. It is a pity that no observatory has ever replaced the ancient one of Empedocles near the summit of Etna, or even at Nicolosi, where the valuable services of Dr. Gemellaro might have been obtained. This would have been the more interesting, as Palmieri can detect shocks caused by that volcano, though the distance is enormous. With a third observatory, say in the Philippine Islands, we could not fail to increase our knowledge enormously.

From long practice Palmieri is able to predict eruptions. We remember well when we were enjoying his hospitality at the beginning of last year how he said, "This is a small eruption, but there is going to be a great one; I do not say it will be soon, it may be a year, but it will come." In almost exactly a year the great eruption did come.

GEORGE FORBES

ON THE DISINTEGRATION OF COMETS

THE main design of the following paper is to present at one view the historical evidence of the gradual disintegration of periodic comets. A few preliminary remarks, however, in regard to the received theory of comets and meteors, may not be destitute of interest.

The fact that in several instances meteoric streams move in orbits identical with those of certain comets was first fully established by the researches of Signor Schiaparelli. The theory, however, of an intimate relationship between comets and meteors was proposed and advocated by the writer several years previous to the publication of Schiaparelli's memoirs. In an article written in July 1861, and published in the "Danville Quarterly Review" for December of that year, it was maintained—

1. That meteors and meteoric rings "are the *débris* of ancient, but now disintegrated comets whose matter has become distributed around their orbits."[*]
2. That the separation of Biela's comet as it approached the sun in December 1845 was but one in a series of similar processes which would probably continue until the individual fragments would become invisible.
3. That certain luminous meteors have entered the solar system from the interstellar spaces.[†]
4. That the orbits of some meteors and periodic comets have been transformed into ellipses by planetary perturbation. And—
5. That numerous facts—some observed in ancient and some in modern times—have been decidedly indicative of cometary disintegration.

What was thus proposed as theory has been since confirmed as undoubted facts. When the hypothesis was originally advanced, the data required for its mathematical demonstration were entirely wanting. The evidence, however, by which it was sustained was sufficient to give it a high degree of probability.

The existence of a divellent force by which comets near their perihelia have been separated into parts, is clearly shown by the facts enumerated in the following lines. Whether this force, as suggested by Schiaparelli, is simply the unequal attraction of the sun on different parts of the nebulous mass, or whether, in accordance with the views of other astronomers, it is to be regarded as a cosmical force of repulsion, is a question left for future discussion.

1. Seneca informs us that Ephoras, a Greek writer of the fourth century B.C., had recorded the singular fact of a comet's separation into two distinct parts.[*] This statement was deemed incredible by the Roman philosopher, inasmuch as the occurrence was then without a parallel. More recent observations of similar phenomena leave no room to question the historian's veracity.

2. The head of the great comet of 389 A.D., according to the writers of that period, was "composed of several small stars" (Hind's "Comets," p. 103).

3. On June 27, A.D. 416, two comets appeared in the constellation Hercules, and pursued nearly the same apparent path. Probably at a former epoch the pair had constituted a single comet.[†]

4. On Aug. 4, 813, "a comet was seen which resembled two moons joined together." They subsequently separated, the fragments assuming different forms.[‡]

5. The Chinese annals record the appearance of three comets—one large and two smaller ones—at the same time in the year 896 of our era. "They travelled together for three days. The little ones disappeared first, and then the large one."[§] The bodies were probably fragments of a large comet which, on approaching the sun, had been separated into parts a short time previous to the date of their discovery.

6. The third comet of 1618.—The great comet of 1618 exhibited decided symptoms of disintegration. When first observed (on November 30), its appearance was that of a lucid and nearly spherical mass. On the eighth day the process of division was distinctly noticed, and on the 20th of December it resembled a cluster of small stars.[‖]

7. The comet of 1661.—The elements of the comets of 1532 and 1661 have a remarkable resemblance, and previous to the year 1790 astronomers regarded the bodies as identical. The similarity of the elements is seen at a glance in the following table:—

	Comet of 1532.	Comet of 1661.
Longitude of Perihelion	111° 48′	115° 16′
Longitude of Asc. Node	87 23	81 54
Inclination	32 36	33 1
Perihelion Distance	0·5192	0·4427
Motion	Direct	Direct

The elements of the former are by Olbers; those of the latter by Mechain. The return of the comet about 1790, though generally expected, was looked for in vain. As a possible explanation of this fact it is interesting to recur to an almost forgotten statement of Hevelius. This astronomer observed in the comet of 1661 an apparent breaking up of the body into separate fragments.[¶] The case may be analogous to that of Briela's comet.

8. The identity of the comets of 1866 and 1366, first suggested by Prof. H. A. Newton, is now unquestioned. The existence, then, of a meteoric swarm, moving in the same track, is not the only evidence of the original comet's partial dissolution. The comet of 1866 was invisible to the naked eye; that of 1366, seen under nearly similar

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[*] The name of *cometoids* was accordingly proposed for luminous meteors.

[†] Others, it was supposed, might have originated within the system—a view which the writer has not wholly abandoned.

[*] "Quæst., Nat.," lib. vii., cap. xvi.

[†] Chambers's "Descr. Astr.," p. 374.

[‡] Ibid. p. 383.

[§] Ibid. p. 388.

[‖] Hevelius, "Cometographia," p. 341. See also Grant's "History of Physical Astronomy," p. 302.

[¶] "Cometographia," p. 417.

circumstances, was a conspicuous object. The statement of the Chinese historian that "it appeared nearly as large as a Tow measure,"[*] though somewhat indefinite, certainly justifies the conclusion that its magnitude has greatly diminished during the last 500 years. The meteors moving in the same orbit are doubtless the products of this gradual separation.

9. The bipartition of Biela's comet in 1845, as well as the non-appearance of the two fragments in 1865, when the circumstances were favourable for observation, are too well known to require more than a passing notice.

The comet of Halley, if we may credit the descriptions given by ancient writers, has been decreasing in brilliancy from age to age. The same is true in regard to several others believed to be periodic. The comet of A.D. 1097 had a tail 50° long. At its return, in March 1840, the length of its tail was only 5°. The third comet of 1790 and the first of 1825 are supposed, from the similarity of their elements, to be identical. Each perihelion passage occured in May, yet the tail at the former appearance was 4° in length, at the latter but 2½°. In short, instances are not wanting of this apparent gradual dissolution. It would seem, indeed, extremely improbable that the particles driven off from comets in their approach to the sun, forming tails extending millions of miles from the principal mass, should again be collected around the same nuclei.

The fact, then, that comets and meteors move in the same orbits is but a consequence of that disruptive process so clearly indicated by the phenomena described. In this view of the subject, comets—even such as move in elliptic orbits—are not to be regarded as permanent members of the solar system. Their *débris*, however, thus scattered through space, and subject more or less to planetary perturbation, may casually penetrate the atmosphere, producing the phenomena of sporadic meteors.

DANIEL KIRKWOOD

NEWTON'S MANUSCRIPTS AND BIRTHPLACE

ALL Trinity men will, like myself, regret that Lord Portsmouth's gift, recorded in NATURE of June 6, should have been made to the library of Newton's University instead of to that of Newton's College. Surely for many reasons Trinity library is the most fitting depository for the Newton manuscripts. A catalogue of these papers is given in Collet's "Relics of Literature, 1823," pp. 190-194, consisting of eighty-two manuscripts, said to cover nearly eight thousand pages, mostly quarto or folio, besides six note-books, and many letters to Newton in English, French, and Latin. Unfortunately many of these papers relate to biblical or theological subjects.

When Dr. Pellet, by request of Newton's executors, examined these papers with a view to publication, he condemned all but five. These were :—

α 56 half sheets in folio, *De Motu Corporum.*

β 31 half-sheets in folio, being paradoxical questions concerning Athanasius (*sic*).

γ 12 half-sheets folio, an abstract of chronology, and 92 half-sheets folio, the chronology.

δ 144 quarter-sheets, and 95 half-sheets folio, being loose mathematical papers.

ε 40 half-sheets folio, the "History of the Prophecies," in ten chapters, and part of eleventh unfinished.

Of these γ was to have been printed, and α, β, and parts of δ and ε were to be reconsidered.

While on this subject, permit me to add an account of the present state of Newton's birthplace sent me by a lady at Stoke Rochford, where Newton attended a dame's school before going to the free school at Grantham :—

"Woolsthorpe, the birthplace of Sir Isaac Newton, is about half-a-mile westward from Colsterworth, and nine miles from Grantham. It has been thoroughly repaired by its present owner. At the top of the staircase, in a room to the left, Newton was born on Dec. 25, 1642. Over the fireplace is a small white marble slab recording the fact, with the well-known lines parodying Genesis i. 3. The only things in this room which remain unchanged since Newton's time are the door, which is massive, and rather ornamental in its workmanship, and a small cupboard close to the fireplace, the door of which is curiously carved. In another room a singular piece of furniture, made of wainscoting, stands in one of the corners, which looks like a small apartment taken from the main room. It is said to have contained Newton's books, instruments, &c. Above the door in front there is a shield with cross-bones, and a few words to denote that the house was the birthplace of Newton. The sun-dial which Newton made and put upon the south side of his house was sent to the British Museum some thirteen or fourteen years ago."

C. M. INGLEBY

NOTES

WE are informed that Dr. Sharpey, who has for so many years filled with such great advantage to Science and personal distinction the post of Biological Secretary to the Royal Society, has recently sent in his resignation of that appointment. There is a very general hope among Fellows of the Royal Society that Prof. Huxley may allow himself to be nominated as his successor.

AT the meeting of Convocation of the University of Oxford, held last week, the honorary degree of D.C.L. was conferred on the following gentlemen :—Samuel David Gross, M.D. and LL.D., Professor of Surgery in the Jeaffreson Medical College of Philadelphia; Sir Benj. Collins Brodie, Bart., M.A., F.R.S., late Waynflete Professor of Chemistry; George Burrows, M.D., of Caius College, Cambridge, F.R.S., President of the Royal College of Physicians of London, and formerly President of the General Medical Council.

THE choice of the electors of the Waynflete Professorship of Chemisty at Oxford, vacant by the resignation of Sir B. C. Brodie, has fallen on Prof. Odling, F.R.S., who at present holds the position of Fullerian Professor of Chemistry to the Royal Institution, and Examiner in Chemistry to the University of London.

MR. EDWIN RAY LANKESTER, B.A., late junior student, Christ Church, has been elected to a Natural Science Scholarship at Exeter College, Oxford. Mr. Lankester was elected to the Burdett Coutts Scholarship in 1869, and to the Radcliffe Travelling Fellowship in 1870. There were four candidates.

IN accordance with the intimation which we gave last week, Mr. G. B. Airy has been gazetted a K.C.B., a graceful acknowledgment of the claims of representative men of science to recognition by the State.

WE have to record the death, on the 16th inst., in his 82nd year, of Colonel W. H. Sykes, F.R.S., M.P. for Aberdeen. He was a distinguished officer of the East India Company, and occupied the post of chairman of its Board of Directors at the time of the surrender of its Imperial functions. Colonel Sykes was always a firm friend to scientific research, and was himself possessed of no mean scientific attainments.

MESSRS. C. F. J. YULE and W. J. Sollas have been *elected* to Foundation Scholarships for proficiency in Natural Sciences at St. John's College, Cambridge. Each has been twice placed in the first class in the College Examination in Natural Sciences, and Mr. Sollas obtained in 1870 the exhibition of 50*l.* per annum offered by the College for competition to students in Natural Science not yet members of the University.

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[*] Williams's "Chinese Observations of Comets," p. 73.

The following is the list of those who have passed the special examinations in Natural Sciences for the ordinary B.A. degree at the University of Cambridge:—Chemistry—First Class: Porter, Clare; Loder, Trinity; Dent, Trinity; Corbet, St. John's. Second Class (in alphabetical order): Flood, Jesus; Savary, Trinity; Thomas, St. John's; Winder, Christ's. Geology—First Class: Jesson, Trinity. Botany—First Class: Smith, Christ's; Gibb, Down; Standert, Corpus; Norcock, Corpus. Second Class (in alphabetical order): C. W. H. Evans, Caius; Gibson, Christ's; Hamilton, St. Peter's; Hughes, Clare; Norton, Clare; Tamberlain, Trinity. Ægrotat: Moore, Corpus. Zoology—First Class: Bird, Trinity. Second Class (in alphabetical order): Campbell, Caius; Clutton, Clare; Leatham, Trinity.

Science has sustained a great loss in the West of England by the death of Mr. J. S. Enys, of Enys, near Penryn. In conjunction with Sir Charles Lemon and Mr. Davies Gilbert, he assisted science in every way that one of the largest landed gentlemen in the county could do. The Falmouth Polytechnic, and most others of the Cornwall scientific institutions, will miss the support which Mr. Enys so largely and constantly afforded them. Many of his geological and kindred works have been printed in the "Proceedings of the Royal Cornwall Institution," and by liberal subscription he assisted scientific periodical literature. He died, an ardent nature-worshipper, in his 76th year.

Mr. Gregory, the new Governor of Ceylon, we are happy to hear, not only takes great interest in the wonderful archæological treasures of the island, but also intends to do his best to promote the course of science there. A regular curator, a zoologist, will probably be appointed to the Colombo Museum, and if he does half as much work amongst the fauna of Ceylon as Mr. Thwaites has done amongst the flora, biological science will profit in no small degree thereby. We wish Mr. Gregory all success, and hope he may secure a good man for the post.

The great provincial meeting of the Royal Horticultural Society will be held next week at Aston, near Birmingham, and promises to be a brilliant one. The proceedings will be opened on Tuesday, the 25th inst., by H.R.H. Prince Arthur.

Prof. Humphry commenced his course of three lectures on Human Myology at the Royal College of Surgeons on Monday last. He spoke of the various provisions, such as obliquity of the direction of fibres, insertion near the centre of motion, passage through loops, &c., which are for the purpose of lessening the range of muscular action required, and so of shortening the fibres of muscles. The mechanical disadvantage resulting from this is, he observed, more than compensated for by the greater number of fibres brought to bear by means of tendons upon given points, and by the convenience of massing the fibres in certain positions, as well as by greater strength in the muscle itself. He next entered into the morphology of the muscles of the trunk, neck, and head. All of these he regarded as modifications of, or derivatives from, that structure which in the fish forms the great lateral muscle; and he showed how the parts corresponding with the septa of the lateral muscle sometimes form inscriptions in the muscles, and sometimes become converted into tendons, as well as into osseous and cartilaginous structures. This subject was discussed at considerable length. In speaking of the intercostal muscles, he gave mechanical reasons for believing that both the external and the internal intercostal fibres are, in their whole extent, agents in inspiration, and that the internal intercostals do not, as they have been supposed to do by some authors, act as depressors of the ribs at parts of the intercostal spaces.

A paper will be read at the Horological Institute on Tuesday, June 25, on "The Compensation and Adjustment of the Hemispherical Cup Arms of Velocity Anemometers," by Mr. John James Hall, F.M.S.

The Franklin Society of Mobile was organised in the year 1835, under charter of the State of Alabama, for the purpose of promoting intellectual culture, literary taste, and other kindred objects. Its operations were suspended a little before the outbreak of the late American war, in consequence of the destruction of its hall, furniture, and a part of its library by fire, and have only recently been resumed. A suitable building has been purchased and remodelled, and arrangements are in progress for the inauguration of lectures and for the extension of the privileges of the Society's library beyond its own membership to the public at large. The library, however, is as yet but limited, and the ability of the members of the Society has been well nigh exhausted in the expenses incident to the purchase and fitting up of a new building. At a recent meeting it was resolved that a committee be appointed to correspond with the officers of similar societies in the several States, and with such other persons as the committee may think proper to address, and to request donations or loans of books, manuscripts, paintings, engravings, or other works of art. We commend the movement to those who have the means and inclination to contribute to objects of this nature. Shipments may be made to the "Franklin Society, Mobile, Alabama," the freight on which will be paid by the Society; or parcels may be sent to W. E. Mickle, Secretary Mobile Franklin Society, care of E. Stock, 62, Paternoster Row.

The Austrian Government steamer *Admiral Tegethoff* sailed from Bremen on Thursday last, on its North Pole expedition. A farewell banquet, at which Count Zichy and Dr. Petermann were present, was given to the members of the expedition at Geestemunde.

It is reported that the Emperor of Russia is projecting the junction of the Black Sea with the Caspian by a short canal connecting the Manutch, an eastern tributary of the Don, with the Kerma, a river running into the Caspian. The total length of the communication will be 680 versts, or 90 German miles; but the length of the canal will be only about one German mile. The piercing of the mountain which separates these rivers will, however, be an engineering work of gigantic magnitude, and is calculated to require the labours of 32,000 workmen for six years, and to cost 81,000,000 roubles.

Prof. Dawson, F.R.S., delivered the Annual Address to the Natural History Society of Montreal on May 18. In it he strongly attacked the Darwinian theory of Evolution, which, in its extreme form, he considered had a tendency to "prostitute natural history to the service of a shallow philosophy," and to lead to "the destruction of science, and a return to semi-barbarism." He held that his researches on the shells of the Gulf of St. Lawrence and the coasts of Labrador and Greenland, showed that it was impossible that any changes of the nature of evolution were in progress; but that all these species had remained the same, even in their varietal changes, from the post-pliocene period till now. Principal Dawson then referred to the controversy raised by Dr. Sterry Hunt with regard to the use of the names Cambrian and Silurian in geology; and concluded with a sketch of the recent operations of the society in dredging the Gulf of St. Lawrence.

The members of the Australian Eclipse Expedition, if they were unsuccessful in the primary object of their voyage, saw some strange things along the shores to the north of the great continent of Australia. Mr. Foord tells a wonderful story, "amply attested by witnesses," of a fish with four hands. This extraordinary creature was found crawling on a piece of coral dredged up from the bottom of the sea. "The body was that of a fish," says Mr. Foord before the Royal Society on January 22, "but wonderful to relate, it had in the place of fins four legs terminated by what you might call hands, by means of which it made its way rapidly over the coral reef. When placed on the sky-light of the steamer, the fish stood up on its four legs, a sight to behold! It was small, and something like a lizard, but with the body of a fish!" It is to be hoped that a full and scientific description of this latest marvel of deep-sea dredging may soon be published, as the specimen appears to have been brought back to Melbourne. Mr. White, too, of the same Expedition, tells strange tales about the rats. "The little island," he said, "upon which we pitched our tent was overun with them, and what was most extraordinary, they were of every colour from black to yellow, and some tortoise shell!"

Among other collections made by Prof. Marsh during his explorations in 1871 were additional specimens of the pterodactyl, first obtained in 1870. Portions of five individuals were procured; and among them nearly all the bones of the right wing of one, which exhibited the pterodactyl structure in its perfection. The teeth found with the other remains were somewhat similar to those of the pterodactyls of the Cretaceous of England, being smooth, compressed, elliptical, and somewhat curved. A second species, still larger than the other, was obtained in the Upper Cretaceous, near the Smoky River, in Western Kansas. The expanse between the tips of the fully-extended wings was probably as much as twenty-two feet. In all, Prof. Marsh has determined the existence of three species from the same region, which he characterises in the April number of the *American Journal of Science*. In the same journal Prof. Marsh refers to the interesting discovery that the body of mosasauroid reptiles was probably covered with plates, as in some crocodiles, the head itself being smooth. This fact has been ascertained in regard to specimens of all the American genera, so that probably all the species possessed it.

The second Report of the Geological Survey of Indiana, made during the year 1870, under the direction of Mr. E. T. Cox, State geologist, has just made its appearance, and, like its predecessor, appear to be a work of much scientific value. In addition to the series of reports upon the geology of the counties, it embraces a paper upon the Western coal measures and Indiana coal, and a paper upon palæozoic zoology, and closes with an extended manual of the botany of Jefferson County, Indiana, prepared by Prof. A. H. Young, of Hanover College. In this the total number of indigenous species is given at 537, those introduced numbering 72.

Another book of excellent typographical execution has just appeared from the public printing-office of the United States, in the form of the astronomical and meteorological observations made at the United States Naval Observatory during the year 1869, under the direction of the superintendent, Admiral B. F. Sands. This volume, forming a stately quarto of over 900 pages, is prefaced by a detailed account of the transit circle, the meridian transit instrument, the mural circle, and the equatorial of the observatory, and followed by a statement of observations made with these instruments. The volume also contains the meteorological observations for 1869, the positions of the sun, moon, and planets during that year, as made with different instruments, &c. The report of the total eclipse of December 22, 1870, which has already appeared as a separate memoir, is included in this volume, as also an appendix embracing the zones of stars observed with the mural circle in the years 1846, 1847, 1848, and 1849. The observatory is now in excellent condition, and includes in its working force some of the best astronomers and mathematicians of the country; among them Profs. Newcomb, Hall, Harkness, Eastman, &c. The completion of the gigantic telescope now in process of construction by Alvan Clark will constitute an important addition to the means of research, and will doubtless be turned to good advantage.

ON THE SOUND MADE BY THE DEATH'S HEAD MOTH, "ACHERONTIA ATROPOS"

The singular cry produced by the Death's Head Moth has for a long time been known to naturalists, and the question of the exact method of its production has given rise to much discussion. To judge, however, from the latest writings on the subject, the matter is considered even now as being far from definitely elucidated. In the autumn, about six years ago, I was lucky enough to rear over a hundred imagos of *Acherontia atropos* from pupæ obtained from potato diggers in the neighbourhood of Bristol. I made then some observations on the production of the sound in question, but I did not consider them as sufficiently perfect for publication. I got no more specimens until last autumn, when I obtained a single imago from ten pupæ, but on this I made an experiment which I believe to be crucial in the matter. Absence from England, however, on the Government Eclipse Expedition, has prevented my giving an account of my experiment until now.

On looking into the literature of the subject, in which task I have been kindly assisted by Prof. Westwood and Prof. Rolleston, I found it in its extent far exceeding my expectations. The number of theories which have been invented to account for this apparently simple phenomenon is astonishing; and as the history of the question is really very interesting, I shall commence by giving as complete an account of what has been written on the subject as I have been able to obtain by reference to works in the Radcliffe, Bodleian, and Linnean Society's libraries, and in that of Prof. Westwood.

The earliest writer on the subject was Reaumur (Mémoire pour servir à une Histoire des Insectes, 1734-1742, vol. i. pl. 14), who suggests that the noise is most probably due to the same cause as in certain scarabei, which produce a sound by the rubbing together of certain of their scaly parts. Later on (*loc. cit.* vol. ii. p. 24) Reaumur states that he has made further experiments, and concludes that the sound is produced by the rubbing of the proboscis against the palps. He held the palps aside from contact with the trunk, and the sound ceased. But he is of opinion that air may have something to do with the matter, and makes his statement with caution. There is a membrane stretched at the base of the trunk, he says, which may have something to do with it; and finally, "Je ne me lasserai point de répéter que nous dévons nous attendre, que dans les plus petits sujets il restera toujours quelque chose que nous ignorons." It would have been well if some of the many subsequent writers or the subject had profited by this sage remark. The next authois Roesel (Insecten Belustigung: Nürnberg, 1755, § 16), whose observations, according to Wagner, were very superficial; and who considers the sound due to friction between the opposed surfaces of the abdomen and thorax. Next comes Rossi (Istoria della Farfalla a testa di Morto, Opuscoli di Milano, Ann. 1782) who is the first to arrive at a correct result, and says the sound is due to expiration of air through the trunk. Schröten (Der Naturförscher, xxi. Stück: Halle, 1785) gives as a cause the rubbing of the trunk against the head. Engramelle, as quoted by Passerini, without special reference, makes the sound come from the part of the insect called the spallette.

There is now a considerable chronological interval, and then Godart and Dupronchel (Hist. Nat. des Lépidoptères de France. par M. Godart. tom. iii. pp. 18 et 19) report a letter from M. Lorey, retired army surgeon-major, who describes a peculiar pair of organs, situate on the sides of the abdomen, surrounded by long hairs, which, when the animal squeaks, may be seen to elevate themselves, and form a conical cavity leading to an opening. He considers the sound to be produced by the passage of the air through this opening. It will be seen further on that this peculiar pair of organs was subsequently described as a discovery by two writers ignorant of the literature of the subject.

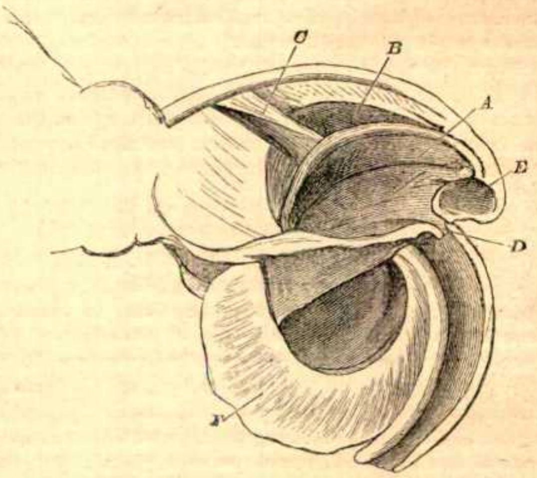
Next comes Passerini, who went to the very root of the matter, and explained the whole thing correctly and clearly in a monograph entitled, "Osservazioni sopra la Sphinx Atropos o Farfalla a testa di Morto," del Dottore Carlo Passerini: Pisa, 1828. He commenced his experiments in 1824. He first disposes of Lorey's theory by showing that his peculiar organs exist *only* in the male insect, whereas both male and female Death's Heads produce the sound; and further, that the same organs exists in the males of other moths, as *Macroglossa stellatarum*, and *Sphinx convolvuli*, which produce no sound. Next he records this startling experiment:—A moth may be divided in two through the middle of the thorax, and the anterior extremities will still continue to speak. This experiment disproves absolutely all theories which connect the sound with the abdomen in any way, by friction or otherwise. He next removed successively from a moth the palps, the trunk, and the spallette, and the insect nevertheless continued to squeak. He then cut away carefully with a sharp knife the horny top of the head of another specimen, and observed certain muscles rising and falling in rapid motion when the animal squeaked, but remaining quiescent as soon as the sound ceased. As long as these muscles were left intact the insect might be mutilated in almost any manner without the sound being stopped. If these muscles were divided longitudinally, or transversely, the power to emit sound was lost. In quite a fresh specimen, he says, in examination after death, the upper part of the head is found filled with an indurated cellular structure, and beneath this are found the elevating and depressing muscles. Beneath these is an inclined shining horny surface of triangular form leading to a narrow transverse aperture corresponding with the opening of the proboscis tube. At the back of the incline is a very fine aperture, leading into the body of the *Sphinx*. He concludes that the air enters the cavity of the head by the fine aperture, and is driven out through the narrow transverse one by the action of the muscles just described, and that thus the sound is produced.

Chavannes wrote on the subject (Act. Soc. Helv. Sc. Nat. 17; Isis: Genève, 1832, pp. 93-94), but I have not been able to refer to his memoir, nor to that of Rochebrune (Act. Soc. Scient. Bordeaux, 1832, t. 5, pp. 120-122, tab. 1.) Then we have H. Burmeister (Handbuch der Entomologie, Berlin, 1832), who after citing Reaumur and Rossi says, "This much is certain, the organ of voice is seated in the head." Then Vallot (*L'Institut*, 1834, II., No. 34, p. 7), who demolishes to his own satisfaction all previous theories, and adopts that of Johet, which considers the sound to be produced by the striking of the wings in rapid movement against certain parts. Wagner (Müller's Archiv für Physiologie, III., 1836, pp. 60-62), after satisfying himself that the sound came from the head, unrolled the trunk, and found that the sound became feebler, but did not cease. When he held the two halves of the trunk apart, or cut off one or both up to the base, the sound ceased. He found just in front of the true stomach a crop very large, distended with air, and filling up the whole anterior part of the abdomen, and opening into the posterior extremity of the œsophagus. He could not find any special apparatus in the trunk, but he says that there appears to be a fine slit-like opening at the basis of this organ on its inferior surface, formed by the imperfect closure of the two halves of the trunk. This may have something to do with the matter. He could not find Passerini's cephalic cavity. The proboscis has strong, transversely-striated muscles. Duges (Traité de Physiologie comparée, par A. Duges: Montpellier, 1838, vol. ii. p. 226) ascribes the noise to the rubbing together of the opposed edges of the two halves of the proboscis.

We have theories already in abundance, and there are more yet to come; but we now come on repetitions of previous theories by persons who either had not access to, or were too lazy to consult, the writings of the original inventors of them. Dr. Alex. V. Nordman (Bull. Acad. St. Petersburg, 1838, t. 3, pp. 164-193) says the sound is seated neither in head nor proboscis, but in the abdomen; and he then proceeds to give an elaborate description of the peculiar organs already discovered by M. Lorey, and he congratulates himself not a little on his having been able, after the failure of so many previous investigators, to make this discovery, and to set the matter finally at rest. We next have observations and experiments made by MM. Duponchel and Guerin (Ann. Soc. Ent. France, 1839, t. 8, pp. 59-65). To show that Passerini was wrong, they compressed the trunk at the base sharply with forceps, and also stopped the end of the proboscis, but apparently in a very insufficient manner. The noise did not cease. They had only one individual on which to operate, and therefore could not afford to mutilate it and repeat Passerini's experiments. Their conclusion is that the noise has the nearest analogy to that emitted by Longicorn beetles, which is produced by the rubbing of the prothorax against the smooth portion of the scutellum. Goureau (Ann. Soc. Ent. France, 1840, t. 9, pp. 125-128) says, "Reaumur, Passerini, Lorey, all are wrong. There is no special organ for the sound. The sound is of a double nature. There is one shrill part of the sound which proceeds from the vibrations of the thoracic rings; in another part a grating sound is caused by the rubbing of the shoulders against the thorax." Abicot (Ann. Soc. Ent. France, ser. 2, 1843, t. 1, Bull. p. 50) says that amputation of the trunk stops the noise, therefore Goureau is wrong.

Ghiliani (Ann. Soc. Ent. Fr., ser. 2, 1844, t. 2, Bull. pp. 72, 75) comfirms Passerini. He cut off the head and the sound stopped. He held the trunk horizontally, and at various inclinations, the sound continued; he removed the palps, the sound continued; he dipped the head in oil, the sound ceased. He then amputated the trunk at its root. Three apertures were formed by the operation, one corresponding to each half of the proboscis, the other probably to the prolongation of the buccal cavity situate just above the trunk, and opened by the knife when amputating the trunk. A green liquid flowed from the third opening over the cut root of the trunk, in which liquid large bubbles were seen to be formed by the expiration accompanying each sound. On closing the apertures the sound ceased. He is unable to explain how inspiration and expiration are managed. Paris (Ann. Soc. Ent. France, ser. 2, 1846, t. 4, Bull. pp. 96, 142) thinks the noise due to a mucous fluid which the insect forces by aspiration, with the assistance of the palps up and down inside the proboscis, comparing the process to the flow and reflow of a liquid in a suction and force-pump.

J. Vander Hoeven (Tydschr. Entom. Neder Vereen, 1859, t. 2, Stück 4, pp. 117-122) says the source of the sound is certainly in the head. In an Indian species of *Acherontia* he has found tuberosities all over the basal portion of the trunk. The exterior surface is beset with stiff bristles. On rubbing this surface against the edge of a piece of paper or scalpel, a sound like that of the moth is produced. He considers the moth's cry to be due to friction of this kind.



Vertical section through the median line of the head of *A. atropos*, from a specimen preserved in absolute alcohol. *A*, dome-shaped cavity of Passerini. *B* and *C*, depressing and elevating muscles of this cavity. *D*, narrow orifice, leading from the cavity to the tube of the proboscis. *E*, palpus.

Westmaas (Tydschr. Entom. Neder Vereen, 1860, t. 3, pp. 120-124) extended the trunk on a pin, and at the same time pulled the palps aside. He still heard the sound, though it became feeble. He cut off the palps without effect on the sound. He cut off the trunk bit by bit, as it was gradually shortened the sound diminished. A fluid exudation covered the end of the trunk, and he saw bubbles of air formed in this as each sound was emitted. He confirms Passerini. He notices that the insect produces a louder sound when the trunk is coiled up. The moth when emitting a sound elevates the front of its body, and uses an evident effort. He stopped the aperture of the trunk with grease, the crying ceased; he kept one specimen half a day with the trunk aperture thus closed, it emitted no sound, but on the grease at last being removed squeaked at once. In order to test Wagner's theory, he squashed out the abdomen quite flat, so as to destroy any air cavity it might contain; the animal still squeaked. Chapronnier (Ann. Soc. Ent. Belg. *Comptes Rendus*, pp. 16, 17) says that the noise produced by the larva is due to the snapping together of the mandibles. An imago, which he bred, which had a deformed head, emitted no sound; therefore the sound organ is situate in the head.

Finally, we find some discussion of the matter in the *Entomologist.* Edward Newman (*Entomologist*, August 1865, p. 284), says, "With regard to the sound produced by the pupa and imago, A vast deal has been written, but nothing worth repeating." But he surely cannot have sufficient grounds for this sweeping demolition of previous authors, and certainly cannot have consulted this "vast deal which has been written;" for he proceeds to enunciate the very theory first put forth in the year 1755 by Rossel, and subsequently revised by Duponchel in 1839.

E. A. Johnson (*Entomologist*, Nov. 1865, p. 325, *Field* newspaper, Oct. 24, 1865) describes the organs discovered by M. Lorey before the year 1828, and subsequently by Nordman, in 1838. W. H. Taylor (*Entomologist*, Nov. 20, 1865) brings forward Passerini's objection to Lorey's theory, viz., that males only have these organs, while both sexes squeak, and that other species which produce no sound possess these organs. Thus does history repeat itself. In the same page of the same journal we have some remarks on the subject from the Rev. A. Preston, who, when sticking his pin-point into an insect in killing it, and moving the pin up and down against a muscle, heard a sound just like that of the mouth. He does not, however, from this observation draw any very definite conclusion.

It will be found that there are eleven distinct theories in all which have been put forward to account for the cry of this moth. By far the greater number attribute the phenomenon to friction of various parts, two to an expiratory effort, one to rapidly repeated percussion, and one to the intervention of a fluid.

The foregoing contains the pith of all the writings on the subject which I have been able to meet with. I have no doubt there are many others in existence, but I hardly expect that I have missed any of importance.

I am not aware whether all known species of *Acherontia* emit a sound; but a closely allied species in Ceylon is described by Sir Emerson Tennant, in his "Natural History" of that island, as doing so; and I was lately, whilst in Ceylon, informed by several residents that such was the case. This Ceylon species is probably *Sphinx Acherontia Lethe*, described by Prof. Westwood in his "Oriental Entomology," p. 87.

I now pass to my own observations on the subject. I was killing a specimen out of the large number which, as stated above, I bred about six years ago, by means of a solution of cyanide of potassium, which I was using with a pen in the ordinary manner, the animal squeaking loudly under the operation. A drop of the fluid happened to fall on the extremity of the proboscis. I noticed that at each squeak a large bubble was formed, showing a forcible expiration from the organ. I repeated this experiment constantly with water, and always with the same result. I further extended the trunk with a pin during the emission of the sound, and noticed a modification in the tone of the cry, which varied with the amount of extension. These experiments convinced me that the sound came from the proboscis, and was produced by an expiration. I at that time knew nothing of the literature of the subject, and very little of anatomy, and I unfortunately made no further observation or experiments in the matter; and it was not till last autumn that I was able to pursue the subject further on a single specimen which I was fortunate enough to rear. This specimen was a very lively one, and it squeaked freely. I placed a tight ligature on the extremity of the proboscis whilst it was in the act of emitting its cry. The noise stopped instantly. I kept this specimen two days, and handled it constantly, bullying it in all sorts of ways to try and get it to squeak, but without result. After the proboscis had been thus ligatured for two days, I amputated the lower portion of the trunk just above the ligature. The animal instantly began squeaking, and continued to do so at intervals for two days more, when I killed it in order to examine the anatomy of its head. I fancied my experiments at the time quite novel, and it was only the other day that I found that similar ones had been made by Ghiliani and Westmaas; but the method of ligaturing the proboscis, with subsequent amputation above the ligature, is, I think, more satisfactory than Westmaas's use of wax.

After these experiments, taken in confirmation of what has already been done in the matter, I think there can be no doubt that the sound is produced by expiration through the proboscis. We have now to consider—How is this expiration effected? Whence comes the air expired? and Whereabouts in the proboscis or head is the actual spot where the sound is formed? I think it will be found that Passerini's explanation is in almost every respect satisfactory. I had no time to dissect my Death's Head whilst in the fresh state last autumn, but preserved it in absolute alcohol. The accompanying drawing of a preparation made from it may therefore need slight modification on further investigation; but in the main it will be found correct. The figure represents a magnified view of a vertical section along the median line of the head. A is the large dome-shaped cavity, evidently the one described by Passerini, and which R. Wagner could not find. This cavity has a hard chitinous floor, which is prolonged forward so as to project over the proximal extremity of the proboscis (seen here in section with its end amputated), and there ends in a sharp edge, which forms, with the anterior wall of the cavity, a narrow transverse slit, leading to the proboscis tube, just as described by Passerini. Resting on the roof of the dome-shaped cavity are Passerini's muscles, B and C, with some cancellar-like tissue between them and the external wall of the head. It would obviously be easy to expose these muscles as Passerini did with great ease and very little disturbance to the insect's functions, and I cannot see any reason to doubt that they would be found in action just as he describes.

Passerini does not figure the muscles or describe them accurately. He merely calls them elevating and depressing muscles. The muscle C must obviously on contraction raise the dome-shaped cavity, whilst B must depress it. An alternate action of the two muscles would cause the cavity to act as a bellows, and inhale and expire air through that aperture which allowed it to pass and repass most freely. Passerini believed that the air entered the cavity by the œsophageal opening at its hinder part, which he describes as very narrow (*loc. cit.*, p. 6) ("Da tutto cio che ha esposto credo poter concludere che l'aria dall'nterno della sfinge vien portata alla cavita muscolare della testa per mezzo dell' esilissima tuba"), and was expelled then by the proboscis; but that this is really the correct explanation is scarcely probable. First of all, the posterior opening into the cavity must be very small indeed. I cannot find it at all in the present specimen, and it is highly probable that it may often be aborted altogether, as is the case in most butterflies. Moreover, how should the air get into the œsophagus? Certainly not from the abdomen as supposed by Wagner, for Westmaas showed that the animal could squeak after the abdomen was squashed out flat, and Passerini himself showed that an insect would emit the sound after it had been divided in two through the middle of the thorax, an experiment which of itself is sufficient to overthrow his own view. The opening into the proboscis being by far the largest leading into the dome-shaped cavity, the air probably finds ingress as well as egress by this aperture. And if I remember rightly the bubbles formed on the end of the proboscis in my experiments always collapsed between the squeaks, showing this to be the case. I think that there can further be no doubt that the note is formed at the narrow slit-like opening, the sound being modified by passage through the proboscis tube, and by vibrations therein set up, this latter to account for modifications produced by straightening the trunk or by gradually removing it bit by bit from below (Westmaas).

I further think it probable that there is a movement of the proboscis concerned in the production of the sound. If the base of the proboscis were drawn a slight distance directly forward at each inspiration, the upper opening of that organ would be brought more immediately beneath the narrow passage communicating with the dome-shaped cavity, and the ingress of air would be rendered more free. Then if at expiration the base of the proboscis were retracted again, the aperture of egress would be very much contracted, and the formation of the sound facilitated. It would be interesting to observe whether such a motion of the proboscis takes place. I think I remember to have noticed a slight movement of the trunk during the emission of the sound.

It is most extraordinary that the seat of the sound should ever have been imagined to be anywhere but in the head. One has merely to listen to the animal to detect at once where the sound comes from. It would seem as if many writers on the subject had commenced their observations with a determination to find some other seat for the cry. The animal is a large one, and one could as easily persuade oneself that a mouse's cry proceeded from the tip of its tail as that of a Death's Head from its abdomen. Should I obtain specimens of *A. atropos* this autumn, I hope to repeat Passerini's experiments, and also make certain whether inspiration as well as expiration takes place through the proboscis; I think also that Wagner's narrow slit on the under surface of the proboscis should be experimented on.

H. N. Moseley

SCIENTIFIC SERIALS

THE *Geological Magazine* for June (No. 96) opens with an interesting article by Mr. Dyer on some fossil wood from the Lower Eocene of Herne Bay and the Isle of Thanet, in which the author describes and figures the microscopic structure of the wood of a Dicotyledonous tree, showing the peculiar phenomenon known under the name of "tylose."—Mr. G. Poulett Scrope communicates some notes on the late eruption of Vesuvius.—From Mr. T. McKenny Hughes we have a note entitled "Man in the Crag," in which the writer discusses the interpretation to be given to certain crag sharks' teeth with holes bored in their substance, and sometimes through them from side to side, which have been supposed to be the work of human hands. Mr. Hughes is of opinion that there is no evidence to support this opinion, and that the cavities in question have been produced by boring mollusca.—Mr. A. R. C. Selwyn, Director of the Canadian Geological Survey, notices the occurrence of some fine fossil footprints in a stratum of dark shale belonging to the Carboniferous series of Nova Scotia, and these footprints are described and figured by Principal Dawson The latter writer states that the principal footprints are of two kinds—a large one resembling the form described by him as *Sauropus sydnensis*, but having a strong claw on the fifth toe of the hind foot, which has left its mark strongly impressed upon the slab containing the prints, and a smaller impression, sometimes trifid, but occasionally showing the marks of four or five toes. The former (which he names *Sauropus unguifer*) he thinks may have been made by *Baphetes planiceps*; the latter perhaps by a species of *Dendrerpeton.*—Mr. James Geikie concludes his valuable series of papers on changes of climate during the Glacial epoch, and gives an important tabular view of the Quaternary deposits of the British Islands, with their equivalents in some other countries. Mr. G. H. Kinahan notices the supposed middle gravels of the drift of Ireland. The Rev. O. Fisher describes the occurrence of a worked flint in the brick-earth of Crayford. The Rev. T. G. Bonney has a paper on supposed Ice scratches in Derbyshire, which he regards as slickensides; and Prof. Traquair furnishes a supplementary note on *Phaneropleuron* and *Uronemus.*—Among the notices we may mention an account of the human skeleton lately discovered in a cavern at Mentone.

Revue Scientifique, Nos. 43-50.—No. 43 commences with an article by M. Wolf on the Transit of Venus in 1874, illustrated by five diagrams. Mr. Keith Johnston's paper read before the Royal Society of Edinburgh on the Lake-basin of Eastern Africa is translated. In subsequent numbers we find a continuation of M. Claude Bernard's course of lectures on Animal Heat. A paper presented by M. Ch. Grad to the Geographical Society of Paris on the resources of Alsace. Dr. Gunther's paper on *Ceratodus Forsteri* is translated from NATURE. M. Dumas contributes an article on the higher instruction in Agriculture at the Central School of Arts and Manufactures in Paris. M. G. de Morillet on Cave-man; epoch of the Madeleine. M. Grandidier contributes a most interesting series of papers on his scientific voyage to Madagascar. Translation of the chapter on the evolution of religious ideas among savages, from Sir John Lubbock's "Origin of Civilisation." Translation of Captain Noble's lecture delivered at the Royal Institution on the Explosive Force of Gunpowder. Report of the meeting of the Congress of German Naturalists and Physicians at Rostock in Sept. 1871, department of Geography and Chemistry.—In No. 44 is a history of the Observatory of Paris. Biography of M. Pictet by Soret. We have besides in each number abstracts of the proceedings of the various scientific societies: the Académie des Sciences, Académie de Médecine, Société de Biologie, Société Chimique, Société Géologique, Société Botanique, Société d'Anthropologie, and of the foreign scientific societies at Vienna, Berlin, London, Palermo, &c.

THE *American Naturalist* for June does not contain so many original articles as usual. The longest is by Dr. R H. Ward, on "Students' Microscopes," with particulars of the relative advantages offered by the instruments furnished by different makers.—Mr. J. A. Allen continues his "Ornithological Notes from the West," discoursing this time on the birds of Colorado. —There are two interesting shorter articles: by Mr. B. Pickman Mann, on the "White Coffee-leaf Miner" (*Cemiostoma coffeellum*), so destructive to the coffee culture of Brazil, with a plate; and by Prof. Sanborn Tenney, on the Remarkable Simulation of Death presented by the Hibernation of the Jumping Mouse (*Jaculus Hudsonius*) of the Western States.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 13.—"Further Experiments on the Effect of Alcohol and Exercise on the Elimination of Nitrogen, and on the Pulse and Temperature of the Body." By E. A. Parkes, F.R.S.

1. The elimination of nitrogen during exercise was unaffected by brandy; and since the experiments led to the same result in the former series during comparative rest, it seems certain that in healthy men on uniform good diet alcohol does not interfere with the disintegration of nitrogenous tissues.

2. The heat of the body, as judged of by the axilla and rectum temperatures, was unaffected by the amount given.

3. The pulse was increased in frequency by four ounces of brandy, and palpitation and breathlessness were brought on by larger doses, to such an extent as to greatly lessen the amount of work the man could do, and to render quick movements impossible. As the effect of labour alone is to augment the strength and frequency of the heart's action, it would appear obviously improper to act on the heart still more by alcohol. In this effect on the heart, and through it on the lungs, is perhaps to be found the explanation of the trainer's rule, which prohibits alcohol during exertion. Whether in a heart exhausted by exertion alcohol would be good or bad is not shown by these experiments; but it can hardly be supposed that to urge a heart which requires rest, as would then be the case, can be proper.

4. It seems clear, from the suddenness with which marked narcotic symptoms came on after the third dose was taken on each day, that the eight hours from 10 to 6 o'clock were not sufficient to get rid of the brandy taken at 10 and 2, and that in fact the body must have been still saturated at 6 o'clock.

The exact amount of brandy which commenced to lessen the labour the man could perform is not shown by these observations, and would require more careful modes of investigation. It was evidently some quantity more than 4 ounces which produced effects sufficiently marked to attract his attention, but I should not wish to affirm that every 4 ounces produced no effect in this direction. The man himself was of opinion that 4 ounces had no influence either way. He was quite certain it did not aid his work, but he could not see that it injured it. The second 4 ounces decidedly produced a bad effect.

5. That neither exercise on water nor on alcohol produced any effect on the phosphoric acid of the urine. The result is in accordance with that of the experiments recorded in No. 89 of the "Proceedings of the Royal Society."

The effect on the free acidity of the urine was also inappreciable. The free acidity may have been a little increased in the brandy period, but the change is so slight as to fall within the limits of normal variation.

The effect on the chlorine was not certain, as its ingress was not sufficiently constant.

As the action of alcohol in dietetic doses on the elimination of nitrogen and on the bodily temperature is so entirely negative, it seems reasonable to doubt if alcohol can have the depressing effect on the excretion of pulmonary carbon which is commonly attributed to it. It can hardly depress, one would think, the metamorphosis of tissues, or substances furnishing carbon, without affecting either the changes of the nitrogenous structures or bodily heat. It seems most important that fresh experiments should be made with respect to its effect on carbon elimination, as without a perfect knowledge on that point the use of alcohol as an article of diet in health cannot be fairly discussed.

Royal Geographical Society, June 10.—Major-General Sir Henry C. Rawlinson, president, in the chair. "On the New Hebrides and Santa Cruz Islands in the South-west Pacific;" by Lieut. A. H. Markham. The paper described the topography, volcanic phenomena, and ethnology of these groups of islands, visited by him during the cruise of H.M.S. *Rosario*, under his command, between October 1871, and February 1872. He gave a history of the progress of discovery in this part of the Pacific, commencing from the voyage of Mendaña in 1568. All the various expeditions for three centuries did little more than sail through the groups and have deadly encounters with the natives. The islands lie in N.N.W. and S.S.E. direction, and contain some of the most continuously active volcanoes on the surface of the globe. The volcanic cones may be traced in a linear direction for 600 miles. The islands are remarkable for the absence of coral reefs around them, which is attributed by Dana to the destruction of the zoophytes by the heat produced by submarine eruptions. Lieut. Markham ascended the volcano Gasowa, in the island of Tanna, and watched an eruption from the edge of the crater. During the intervals between the explosions (sounding like broadsides from a line-of-battle ship) the sheets of liquid fire seemed to flow back to three distant openings in the bottom of the funnel-shaped crater; masses of scoriæ were hurled up vertically to a height of 1,000 feet. The Melanesian (black, curly-haired) and Polynesian (straight-haired) races appeared to be curiously dovetailed in their distribution throughout the northern portion of these archipelagos. This was explained, in the discussion which followed, by the Bishop of Lichfield, who gave to the meeting a most interesting account of his own experiences in these islands, and who showed that the wandering Polynesians, who peopled the greater portion of the Pacific area (including New Zealand), had been driven in their canoes by winds on some of the smaller islands of the group.

Geological Society, June 5.—J. Gwyn Jeffreys, F.R.S., in the chair.—1. "Notes on Sand-pits, Mud-volcanoes, and Brine-pits, met with during the Yarkand Expedition of 1870." By Dr. George Henderson. The author described some very remarkable circular pits which occurred chiefly in the valley of the Karakash river. These pits varied in diameter from six to eight feet, and were between two and three feet deep, the distances between the pits being about the same as the diameters. He accounted for the formation of the pits by supposing that the water, which sinks into the gravel at the head of the valley, flows under a stratum of clay, which prevents it from rising; the water in course of time, however, flowing in very varying quantities at different periods, gradually washes away small portions of the clayey band, when the sand above runs through into the cavity thus formed, leaving the pits described by the author. The mud-volcanoes at Tarl Dab he accounted for by supposing that after a fall of rain or snow the air contained in the water-bearing stratum would get churned up with water and mud, and be ejected as a frothy mud, sometimes to a height of 3 ft.; while the brine-pits in the Karakash valley he believed to be formed by the excessive rise and fall in the level of that river at various times, which alternately fills and empties the bottoms of the pits, and the water left in the pits gets gradually concentrated by evaporation until a strong brine remains. Mr. Prestwich pointed out that the pits seemed due to quite another cause than the pipes in the chalk and other calcareous rocks, as they did not appear to arise from erosion by carbonic acid. Mr. Thorp suggested an analogy between the phenomena in Yarkand and those at Nantwich, and thought that the pits might be due to solution of rock-salt below the surface.—2. "On the Cervidæ of the Forest-bed of Norfolk and Suffolk," by W. Boyd Dawkins, F.R.S. The author described a new form of *Cervus* from the Forest-bed of Norfolk, which he based on a series of antlers, and named *C. verticornis*. The base of the antler is set on the head very obliquely; immediately above it springs the cylindrical brow-tyne, which suddenly curves downwards and inwards; immediately above the brow-tyne the beam is more or less cylindrical, becoming gradually flattened. A third flattened tyne springs on the anterior side of the beam, and immediately above it the broad crown terminated in two or more points. No tyne is thrown off on the posterior side of the antler, and the sweep is uninterrupted from the antler base to the first point of the crown. The antlers differ in curvature and otherwise from those of *Cervus megaceros*, but there is a general resemblance between the two animals; and the *verticornis* must have rivalled the Irish elk in size. A second species of deer, the *Cervus carnutorum*, which had been furnished by the strata of St. Prest near Chartres, must be added to the fauna of the forest-bed. The Cervidæ of the forest-bed present a remarkable mixture of forms such as the *Cervus polignacus*, *C. Sedgwickii*, *C. megaceros*, *C. carnutorum*, *C. elaphus*, and *C. capreolus*, seeming to indicate that in classification the forest-bed belongs rather to an early stage of the Pleistocene than to the Pliocene age. This inference is strongly corroborated by the presence of the mammoth, which is so characteristic of the Pleistocene age.—3. "The Classification of the Pleistocene Strata of Britain and the Continent by means of the Mammalia." By W. Boyd Dawkins, F.R.S. The Pleistocene deposits may be divided into three groups—1st, that in which the Pleistocene immigrants lived, with some of the southern and Pliocene animals in Britain, France, and Germany, and in which no arctic mammalia had arrived; 2nd, that in which the characteristic Pliocene Cervidæ had disappeared, and the *Elephas meridionalis* and *Rhinoceros etruscus* had been driven south; 3rd, that in which the true arctic mammalia were the chief inhabitants. This third, or late Pleistocene division, must be far older than any prehistoric deposits, as the latter often rest on the former, and are composed of different materials; but the difference offered by the fauna is the most striking. In the Pleistocene river-deposits twenty-eight species have been found, the remains of man being associated with the lion, hippopotamus, mammoth, wolf, and reindeer. On examining the fauna from the ossiferous caves, we find the same group of animals, with the exception of the musk-sheep; and it is therefore evident that the cave-fauna is identical with that or the river strata, and must be referred to the same period. Some few animals, however, which would naturally haunt caves, are peculiar to them, as the cave-bear, wild cat, leopard, &c. The magnitude of the break in time between the prehistoric and late Pleistocene period may be gathered also from the disappearance in the interval of no less than nineteen species. The middle division of the Pleistocene mammalia, or that from which the Pliocene Cervidæ had disappeared, and been replaced by invading temperate forms, is represented in Great Britain by the deposits of the Lower Brick-earths of the Thames Valley, and the older deposits in Kent's Hole and Oreston. The discovery, by the Rev. O. Fisher, of a flint-flake in the undisturbed Lower Brick-earth at Crayford, proves that man must have been living at this time. The mammalia from these deposits are linked to the Pliocene by the *Rh. megarhinus*, and to the late Pleistocene by the *Ovibos moschatus*. The presence of *Machærodus latidens* in Kent's Hole, and of the *Rh. megarhinus* in the cave at Oreston, tends to the conclusion that some of the caves in the south of England contain a fauna that was living before the late Pleistocene age. The whole assemblage of Pleistocene animals evinces a less severe climate than in the late Pleistocene times. The fossil bones from the forest-bed of Norfolk and Suffolk show that in the early Pleistocene mammalia there was a great mixture of Pleistocene and Pliocene species. It is probable also that the period was one of long duration, for in it we find two animals which are unknown on the Continent, implying that the lapse of time was sufficiently great to allow of the evolution of forms of animal life hitherto unknown, and which disappeared before the middle and late Pleistocene stages. The author criticised M. Lartet's classification of the late Pleistocene or Quaternary period by means of the cave-bear, mammoth, reindeer, and aurochs, and urged that, since the remains of all these animals were intimately associated in the caves of France, Germany, and Britain, and, so far as we know, the first two appeared and disappeared together, and the last two lived on into the Prehistoric age, they did not afford a basis for a chronology. The latest of the three divisions of the British Pleistocene fauna is widely spread through France, Germany, and Russia, from the English Channel to the shores of the Mediterranean. The Middle Pleistocene is represented by a river-deposit in Auvergne, and by a cave in the Jura, in which the presence of the *Machærodus latidens*, and a non-tichorine rhinoceros, and the absence of the characteristic arctic group of the late Pleistocene and of all the peculiar animals of the early Forest-bed stage, prove that that era must be Middle Pleistocene. The early Pleistocene division is represented in France by the river-deposit at Chartres, being characterised by the presence of two non-Pliocene animals, *Trogontherium* and *Cervus carnutorum*. The Pleistocene mammalia of the regions south of the Alps and Pyrenees present no trace of arctic species, the mammoth being viewed as an animal fitted for the climatal conditions both of Northern Siberia and of the Southern States of America. It contains *Elephas africanus* and *Hyæna striata*. The fauna of Sicily, Malta, and Crete differ considerably from that described above, possessing some peculiar forms, such as *Hippopotamus pentlandi*, *Myoxus melitensis*, and *Elephas melitensis*. The Pleistocene mammalia may be divided into five groups, each marking a difference in the climate, the first embracing those which now live in hot countries; the second those which inhabit northern regions, or high mountains, where the cold is severe; the third those which inhabit temperate region; a fourth those which are found alike in hot and cold; and a fifth which are extinct. There were three climatal *zones*, marked by the varying range of animals. The northern, into which the southern forms never penetrated, the latitude of Yorkshire being the boundary of the advance of the southern animals; the southern, into which the northern species never passed, a line passing through the Alps and Pyrenees being the limit of the range of the northern animals, and an intermediate area in which the two are found mingled together. Two out of the three zones are proved by the physical evidence of the Pleistocene strata.

We see by the discoveries of Dr. Bryce, Mr. Jameson, and others that the Pleistocene mammalia must have invaded Europe during the first Glacial period before the submergence, for the reindeer and the mammoth have been found in Scotland under the deposits of Boulder-clay. Dr. Falconer and others have also discovered the latter animal in the preglacial forest-bed. The Glacial period can therefore no longer be looked on as a hard and fast barrier separating one fauna from another. If man be treated as a Pleistocene animal, there is reason to believe that he formed one of the North Asiatic group, which was certainly in possession of Northern and Central Europe in Pre-glacial times. The Pleistocene mammalia may again be divided into three groups, those which came from Northern and Central Asia, those from Africa, and those which were living in the same area in the Pliocene age. Had not the animals which lived in Europe during the Pliocene age been insulated from those which invaded Europe from Asia by some impassable barrier, the latter would occur in our Pliocene strata as well as the former. Such a barrier is offered by the northern extension of the Caspian up the valley of the Obi to the Arctic Sea. The animals of Northern and Central Asia could not pass westwards until the barrier was removed by the elevation of the sea-bottom between the Caspian and the Urals. The same argument holds good as to the African mammalia, which could not have passed into Sicily, Spain, or Britain, without a northward extension of the African mainland. The relation of the Pleistocene to the Pliocene fauna is a question of great difficulty. If the Pliocene fauna be compared with that of the Forest-bed, it will be seen that the difference between them is very great. The Pliocene mastodon and tapir, and most of the Cervidæ, are replaced by forms such as the roe and red-deer, unknown until then; but many of the Pliocene animals were able to hold their ground against the Pleistocene invaders, although they were ultimately beaten in the struggle for existence by the new comers. The fauna which the author adopted as typically Pliocene is that furnished by the lacustrine strata of Auvergne, the marine sands of Montpellier, and the older fluviatile strata of the Val d'Arno. Mr. Prestwich was hardly prepared to accept the proposed division of the Pleistocene mammalia into three groups; at all events so far as Britain is concerned. Neither could he draw that distinction between the beds at Erith and Grays and those higher up the Thames, which found favour with the author. The barrier offered by the river itself might to some extent account for the absence of reindeer; and though there was a difference in the fauna in the two cases, it seemed hardly enough to mark any great distinction in time. As to the hippopotamus, which occurred over the whole of Northern Europe, associated with the musk-ox and large boulders, he could not see how the conclusion was to be escaped of its having been able to withstand greater cold than its present representative. Though the winters might have been colder, there was evidence in favour of the summers having been warmer; and the flora seems to have been much like that of the present day. The probable migrations of the different animal groups had already been pointed out by M. Lartet, though Mr. Dawkins had carried his investigation of the subject further. He called attention to the fact of the mammoth having been found in Italy. Mr. Boyd Dawkins, in reply, stated that in forming his conclusions, he had not left out of view the evidence afforded by the classes of remains other than those of mammalia, but they threw no light on the classification. With regard to the middle of his divisions of the Pleistocene mammalia, he relied to a great extent on the presence of *Rhinoceros megarhinus*, and of a large number of stags, to say nothing of the absence of reindeer. He did not attach so much importance to the question of the level, as such discrepancies as those pointed out appeared to him by no means impossible. He gave his reasons for not regarding the mammoth as an exclusively arctic animal. His remarks with regard to M. Lartet's classification referred rather to the expanded views of his followers than to those of M. Lartet himself. He acknowledged his obligations to Profs. Gaudry, Fraas, Rütimeyer, and Nilsson for various facts of which he had made use.

PARIS

Academy of Sciences, June 10.—M. Marie presented a memoir on the determination of the critical point at which the region of convergence of Taylor's series is situated; and M. A. Ribaucour a note on the theory of lines of curvature.—M. Yvon Villarceau exhibited and described to the meeting an isochronous regulator with vanes, constructed by M. Bréguet.—A note was read by M. E. Vial on a new mode of printing on stuffs by means of metallic precipitations, in which the author described a method of printing either by means of clichés or of copper or steel plates upon any textile fabric by the agency of nitrate of silver.—M. A. Clermont presented a note on the metallic trichloracetates, in which he described the preparation and characters of trichloracetate of ammonium, and of acid and neutral trichloracetate of thallium, and noticed the action of permanganate of potash upon hydrate of chloral in producing trichlor-acetic acid.—M. Wurtz communicated a note by M. Oré on M. O. Liebreich's experiments, from which the latter inferred that strychnine is an antidote to chloral. M. Oré shows grounds for the belief that M. Liebreich's experiments were inconclusive.—M. de Vibraye presented some further remarks on the spontaneous appearance in France of exotic plants in the track of the belligerent armies in the late war, in which he stated that the number of these plants introduced into the department of the Loir et Cher alone is 163.—In consequence of M. de Vibraye's statements, the sections of Botany and Rural Economy were instructed to prepare a scheme for the systematic introduction of Algerian forage plants suitable for the climate of France.—M. Decaisne presented a note by M. J. E. Planchon on the geographical distribution of the Ulmideæ.

BOOKS RECEIVED

English.—Contributions to Molecular Physics in the domain of Radiant Heat: J. Tyndall (Longmans).—Patterns for Turning: H. W. Elphinstone (J. Murray).—Symon's British Rainfall for 1871 (E. Stanford).—Erewhon, or Over the Range (Trubner).—The Principles of Geology, 11th edition, Vol ii: Sir C. Lyell (Murray)

American.—Astronomical and Meteorological Observation made at the U. S Naval Observatory, Washington, 1869.—The Science of Æsthetics in the Nature, kinds, laws, and uses of Beauty: H. N. Day.

Foreign.—Medizinische Jahrbucher, Heft i., 1872: S. Stricker.—Bulletin de la Société Impériale des Naturalistes de Moscou, iii and iv., 1871.—Die Darwinsche Theorie: J. W. Spengel.

DIARY

THURSDAY, June 20.

Royal Society, at 8.30.—Volcanic Energy—an attempt to Develope its True Origin and Cosmical Relations: R. Mallet, F.R.S.—Preliminary Note on the Reproduction of Diffraction Gratings by means of Photography: Hon. J. W. Strutt.—On Voltaic Standard of Electromotive Force: Latimer Clark.—Pyrology, or Fire Chemistry: Capt. Ross, R.A.

Society of Antiquaries, at 8.30.—Hungarian Origin of the word Coach: A Goldsmid.—On the Origin of the Christian Æra: G. Oppert.

Linnean Society, at 8.—On the structural peculiarities of the Bell Bird (*Chasmorhynchus*): by Dr. Murie, F.L.S.

Chemical Society, at 8.—On Deacon's Method of obtaining Chlorine, as illustrating some principles of Chemical Dynamics: H. Deacon.

MONDAY, June 24.

Royal Geographical Society, at 8.30.

WEDNESDAY, June 26.

Society of Arts, at 4.—Anniversary Meeting

Royal Society of Literature, at 8.30.—On the Extent of Ancient Libraries: W. E. A Axon.—On a Service Book of Strassburg use, containing Dramatic representations: Walter de Grey Birch.

THURSDAY, June 27.

Society of Antiquaries, at 8.30.

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THURSDAY, JUNE 27, 1872

THE TIDES AND THE TREASURY

OUR readers may have heard that England is a "sea-girt isle," and that we are a maritime nation, possessing a very powerful navy and an extensive commerce. They also know that the ocean to which we owe these peculiarities is a very restless fluid, its surface being ruffled by the wind, and its entire mass uplifted and depressed from time to time, in what we call tides, by the attractive power of the sun and moon. They know, too, that the theory of the tides has been investigated by the most profound mathematicians, particularly by Laplace, Lubbock, Airy, and Whewell. And they are, no doubt, aware that the theoretical laws deduced by these learned men, though indispensable as a foundation of our knowledge, are entirely insufficient, by themselves, for the wants of man, the conformation of the coast-line and of the sea bottom powerfully modifying tidal facts. Hence it becomes necessary to resort to observations and surveys in order to know what will be the course of the tides as to heights and times in particular localities frequented by ships, such as roadsteads, harbours, and the mouths of rivers. All this, perhaps, every one of our readers knows; but it may not, perhaps, be so generally known that the study of the tides throws light on various high cosmical, gravitational, and physico-geographical problems.

The importance of this study is unquestionable, and, indeed, unquestioned; and it has been pursued to a limited extent by ourselves and all civilised nations at the public cost. But as yet the observations have been insufficient both as to character and as to the number of localities at which they have been taken, and also as to the reduction of them, and the deductions from them, that have been made.

Sir William Thomson accordingly brought the subject before the British Association, and obtained from that body the aid of a Committee and of small sums of money from year to year, to enable him to supply, so far as might be possible, these deficiencies. The Committee was designated "for the purpose of promoting the extension, improvement, and harmonic analysis of tidal observations;" and, having regard to the object with which we now address the scientific public, we must also give its composition, namely, Sir W. Thomson, Prof. J. C. Adams, the Astronomer Royal, Mr. J. F. Bateman, C.E., Admiral Sir E. Belcher, Mr. T. G. Bunt, Staff-Commander Burwood, R.N., Mr. Warren De La Rue, Prof. Fischer, Mr. J. P. Gassiot, Prof. Haughton, Mr. J. R. Hind, Prof. Kelland, Staff-Captain Moriarty, Mr. J. Oldham, C.E., Mr. W. Parkes, C.E., Prof. Bartholomew Price, Prof. Rev. C. Pritchard, Prof. Rankine, Captain Richards, Hydrographer to the Navy, Dr. Robinson, Sir E. Sabine, Mr. W. Sissons, Prof. Stokes, General Strachey, Mr. T. Webster, Profs. Fuller and Iselin (secretaries), and Sir W. Thomson (reporter). Every gentleman here named is favourably known, and the majority are highly distinguished, in those branches of the sciences with which the tides are connected.

The Committee has made three reports, namely, in 1868, 1870, and 1871, the two first prepared by Sir W. Thomson, and the third by Mr. E. Roberts of the Nautical Alliance Office, under whose able superintendence the computations and deductions were placed. The three reports have been published *in extenso* by the British Association in the volumes of the above-mentioned years. They bring fully under view the theoretical basis of the investigation, an account of observations made by the Committee and by the authorities, some of the conclusions deduced therefrom, and a statement of the measures recommended in order to extend and perfect our knowledge of the subject. It is impossible to exaggerate the value of these documents. They clearly define the present position of the problem, and the course which any future researches must take.

At the meeting of the Council of the Association on on November 11, 1871, the following resolutions were passed:—

> (1.) That it is desirable that the British Association apply to the Treasury for funds to enable the Tidal Committee to continue their calculations and observations.
>
> (2.) That it is desirable that the British Association should urge upon the Government of India the importance for navigation and other practical purposes and for science, of making accurate and continued observations on the tides at several points on the Coast of India.

The second of these resolutions has already been productive of fruit. Colonel Walker, R.E., the distinguished superintendent of the Great Trigonometrical Survey of India, has, under the authority of the Indian Government, established self-registering tide-gauges at several points in India, and has made adequate arrangements for the reduction of the results.

It is with Resolution (1) that we are concerned to-day, the official correspondence relating to it having been placed at our disposal for review. This consists of only two documents—a Memorial of the British Association to the Lords of the Treasury, signed by the President, and dated May 21, 1872; and the reply thereto.

The main points dwelt on in the memorial may be thus summarised:—That the primary object which the Committee have uniformly kept in view is "the practical application of their results to Physical Geography, Meteorology, Coast and Harbour Engineering, and Navigation;" that they have undertaken the reduction of twenty years' observations made with self-registering tide-gauges—"a most laborious work;" that 600*l.* has been granted by the British Association in four successive annual sums of 100*l.* and one of 200*l.*, "to pay the calculators, and to print and prepare tables, forms for calculations, &c.; that the last grant barely sufficed for the work actually in hand;" and that they now apply to the Government for the sum of 150*l.*, "to secure the continuance of the investigation."

The reply to this memorial is such that, unless printed *in extenso*, many persons would, we feel certain, refuse to believe that such a document could have been issued with the sanction of a civilised Government. We therefore now append it:—

"Treasury Chambers, June 3, 1872

"Sir,—The Chancellor of the Exchequer has referred to the Lords Commissioners of Her Majesty's Treasury the memorial of the British Association for the Advancement of Science, forwarded to him with your letter of the 21st ult., praying for Government assistance in connection with tidal observations.

"I am to state that their Lordships have given their anxious attention to the memorial, and that they are fully sensible of the interesting nature of such investigations; but that they feel that if they acceded to this request it would be impossible to refuse to contribute towards the numerous other objects which men of eminence may desire to treat scientifically.

"Their Lordships must, therefore, though with regret, decline to make a promise of assistance towards the present object out of public funds.

"I am, Sir, your obedient servant,
"(Signed) William Law

"Sir W. Thomson, Athenæum Club."

Nothing would be easier than to be sarcastically indignant on such a theme as this. The picture of the Lords Commissioners of H M. Treasury giving their "anxious attention" to the tides, and expressing "regret" that they cannot grant so large a sum as 150*l.* for investigations which they really think "interesting," lest eminent men should avail themselves of so imprudent a precedent, in order to make further demands for "scientifically" treating other objects of the same character—this picture is one which requires but a touch, it hardly, indeed, needs a touch, to make it a far-fetched caricature of civilised governing.

To apply the lash, however, to narrow stupidity, can only gratify temporary spleen; and we must resist the temptation in order to attain the higher object of illustrating, by this pointed example, the present condition of State science in England, and of showing what we require in order to prevent the mischief which its existing condition must cause.

To begin with the British Association. Here is a body carrying on operations by means of privately contributed funds, of very limited amount, about 2,000*l.* a year; not for the first, tenth, or hundredth time, quietly accepting as a fact that certain scientific objects of national importance will not be recognised or pursued by the Government, and, therefore, stepping in to contribute as far as they can towards their accomplishment. The Kew Observatory, the map of the moon, the utilisation of sewage, are other examples of the same kind. They have all been commenced on a necessarily miserable scale—a little advance has been made, and then the thing has dropped through for want of funds. Now, according to our apprehension the British Association, though acting with the very best intentions and motives, have greatly erred in these matters. It is absurd to suppose that any one of the numerous large national scientific problems they have taken up could be properly dealt with even if their whole income of 2,000*l.* a year had been devoted exclusively to it. The small contributions to each which they have been able to afford, if not sometimes quite wasted, have almost invariably produced results quite inadequate even to the small expenditure, simply because it was so small as to forbid really efficient measures. This is an evil, but as some good results, however slight and imperfect, have been achieved, it might be submitted to if it were all. A far greater evil, however, has been caused by the measures we allude to. An obscurity has been thrown round the great question which England must soon solve. "What is the scientific work which the Government is bound to perform for the benefit of the community at large; and what is the scientific work which cannot be performed by State agency so well as by private enterprise?"

So long as individuals, and bodies of individuals, without discrimination, attempt to do what should properly devolve on the State, so long will a Government, destitute, like ours, of a particle of the scientific element, neglect its legitimate duties. We therefore strongly counsel the British Association, at their next meeting, to take measures for classifying science under the two great heads of Public and Private, to supply the Government with a full statement of all comprehended under the first head, and to refuse a single penny of its funds to any object not distinctly appertaining to the second. This will bring matters to a crisis—and we want a crisis.

As to the Government, what can we say? Poor Mr. Law's letter speaks volumes. It plaintively confesses its total inability to grasp any State scientific problem lest it should have to deal with all. We have no heart to spurn a prostrate form so lowly and humble; but can we not raise it? Can we not introduce into our Administration a source of knowledge on which they can rely to guide them in the choice of scientific objects really profitable to the nation, and officials able to insure a proper system for the attainment of such objects?

Many minds are busy on this very question; and the fact that a maritime Government will not give 150*l.* towards investigating the tides is not likely to weaken their determination to bring it to a decisive issue.

PUBLIC HEALTH IN AMERICA

Third Annual Report of the State Board of Health of Massachusetts. (Jan. 1872.)

PUBLIC health problems in New England are very much of the same character as they are in Old England. The countries and climates are both healthy, and there is plenty of preventible disease notwithstanding. In both countries bad habits have much to do with the causation of disease. In both countries civilisation takes but small account of natural laws, and as a consequence makes one step forwards where two might be made. One reason of this is partly want of knowledge, but the report before us shows that another not unimportant cause is attempting to gain present advantages by discounting the future. It is an old story told in a new country. There is a small present profit to a small minority of the community, at the cost of the remainder; but Nature, as has been well said, "just goes on levying her own cess in her own way," *i.e.*, she sends in her account, not only to the perpetrators of the damage, but to the whole community which tacitly submits to it.

The Report consists of two portions, one part giving a brief account of the Board's proceedings, the other containing an interesting series of reports by different writers on the effects of arsenical colours on health, on mill dams and water obstructions as causes of disease, on the use and abuse of intoxicating drinks, with reference to a cosmic law of intemperance, on provision for the insane, on the use and abuse of opium. There is a curious paper on the effects on health of the use of the feet in working sewing machines. There are others on slaughtering and bone boiling, vegetable parasites and the diseases produced by them, on small-pox, and on health of towns generally, with special reference to the occurrence of typhoid fever.

Our space will only admit of a cursory glance at the chief questions dealt with in these papers as illustrations of the discounting process alluded to.

Somebody, for example, discovers that papers coloured with arsenic are fair to look on, and may possibly become a source of profit. He makes such papers, and people hang their rooms with them. The maker flourishes, and the purchasers find to their cost that they are poisoned; but not always. If they were always poisoned they would cease to buy, but this not being the case, the law assumes the manufacture to be legitimate, and people take their chance.

The State of Massachusetts was in former times almost entirely exempted from intermittent and remittent fevers. But, unfortunately, the State has numerous "water privileges," which an industrious people may take advantage of. They erect dams and backwater large areas of land, many of which became built on, and now Massachusetts has its fair quota of periodic fevers passing into typhoid fever when the streams dry up in summer.

We next approach the *pons asinorum* of social legislation, viz., the drink traffic, which we, in this country, appear disposed to deal with by reversing the principles of political economy, which teach that demand will ensure supply. We, on the contrary, propose to cut short the supply in hope that the demand may become less in consequence. In a report on this subject, the Chairman of the Board, Mr. Bowditch, endeavours to raise intemperance causes to the dignity of a science, but then he also states that "open dram shops are an unmitigated evil." Whoever wishes to master the question of intoxicating drinks, and to learn something of the cost to a community at which the profit of vending them is purchased, will find much to instruct him in this report. The remedies suggested are shutting up drunkards until they are cured, and using beer and wine instead of spirits. Might we suggest for the consideration of our Transatlantic cousins and also of our own national temperance societies, that the amounts of crime, lunacy, and pauperism produced by drink are possibly ascertainable quantities, and that while we charge railway casualties on companies under whose administration they occur, we charge the costs of crime, lunacy, and pauperism, not on the parties who, for their own profit, are accessory to their production, but on the public at large. If we do the one why do we do the other? Why should railway shareholders be made to refund part of their profits, and publicans be allowed to pocket all theirs? And may not the cure for drunkenness be found after all in leaving supply and demand to themselves, and charging all the damage accruing to the State on the liquor retailers? Might not such a course help to reduce rates and taxes, and convert the publicans into an efficient unpaid police? At all events, it is worth while to ask these questions.

Another kindred subject is the abuse of opium. It appears that the domestic consumption of opium in the United States has increased tenfold in thirty years, for a population little more than doubled. We are sorry to say that teetotalism is blamed for this result. The reporter states that in countries where vine culture prevails drunkenness and opium eating are comparatively unknown, and he argues in favour of domestic wine manufacture as a remedy for both evils.

We learn from the paper on sewing machines, that while making a shirt requires 14 hours 26 minutes by hand, it can be put together by the machine in 1 hour and 16 minutes. A coat requires 16 hours and 35 minutes hand sewing, and only 2 hours 38 minutes by machine sewing. A silk dress can be made by machine in 1 hour 13 minutes, but requires 8 hours 27 minutes of hand labour. The work is mainly done by the feet acting on treadles, which, if imperfectly applied, make a great call on certain sets only of muscles and nerves, and the result is a development of various nervous and constitutional affections peculiar to the female sex.

The best remedy is, of course, applying a motive power to the machine, and next to this to do away with the heel and toe movement of the treadle, and to substitute a swinging backward and forward movement of the feet and legs, or by other improved adaptations of leverage.

The only other paper we can notice is the one on the effect of vegetable parasites on man, which contains a good digest of the present state of knowledge on the subject. The moral of the paper is that, if people will keep their skins dirty and thus allow their vitality to fall below par, nature will kindly step in and supply fungal spores to convert the dirt into some product which is sure to call attention to the fact.

It appears that Boston young men are apt to contract a peculiar kind of ringworm by being shaved in barber's shops, the cure for which is, of course, to learn to shave themselves at home.

These Reports will do much good by enlightening public opinion, and so leading to better habits of life and to greater consideration of the interests of others, while people are looking after their own interests, results which there is small chance of arriving at by any mere legislative enactments.

OUR BOOK SHELF

Note sur les Singes fossils trouvés en Italie, précédé d'un aperçu sur les quadrumanes fossiles en général. Forsyth Major, M.D. (Reprinted from the Proceedings of the Italian Society of Natural Sciences.)

THE primary object of this paper, which was read last month, was to describe certain fossil Simian remains which have lately for the first time been discovered in Italy. One which was found in the valley of the Arno, and presented by the Marquis Ermes-Visconti to the Museum of the city of Milan, consisted of a fragment of a maxilla with the last three molars. It is referred by the writer to a species closely related to the Barbary ape (*Macacus innus*, Linn.), still found at Gibraltar. It appears to have been somewhat smaller than the *M. priscus* of Montpellier, described by Gervais. A second fossil, part of a mandible, belonging to the same species, has been found by M. Cocchi in the Upper Arno valley. A third, also a mandible and also discovered in Tuscany, at Monte Bamboli, has been assigned by the last-named zoologist to a species of *Cercopithecus*. Lastly, some Simian teeth from Mugello, now in the museum of Pisa, are supposed by Dr. Major to belong to a species of *Macacus*.

After discussing the characters of the soil in which the first of these fossils was found, and the other mammalian remains of the same formation—*Rhinocerus Etruscus*, *R. leptorrhinus*, *R. hemitocchus* (Falconer), *Elephas meridionalis*, *Hippopotamus major*(?), *Bos Etruscus*, *Mastodon Arvernensis*, the last being probably somewhat earlier—the author concludes that the maxilla above mentioned belongs to the later Pliocene period.

The following is a list of fossil quadrumana as yet discovered:—

EOCENE

1839 Lyell and Owen, *Eopithecus*(?), Woodbridge, Suffolk.
1862 Rubimeyer, *Cænopithecus lemuroides*, Swiss Jura.

MIOCENE

1836 Cantley and Falconer, *Semnopithecus* sp., Sewalik, N.W. India.
1836 Baker and Durand, *Semnopithecus* sp., Sewalik, N.W. India.
1837 Cantley and Falconer, *Semnopithecus* sp., Sewalik, N.W. India.
1837 Cantley and Falconer, *Macacus erythræus v. rhesus*, Sewalik, N.W. India.
1837 Cantley and Falconer, *Pithecia* sp., Sewalik, N.W. India.
1837 Lartet, *Pliopithecus*, S. of France.
1856 Lartet, *Dryopithecus Fontani*, S. of France.
1863 Biedermann and Heer, *Pliopithecus platyodon*, Zurich.
1870 Fraas, *Colobus grandævus*, Wurtemberg.
1862 Gacedry, *Mesopithecus Pentelici*, Greece.

PLIOCENE AND QUATERNARY

1836 Lund, *Propithecus*, *Iacchus*, *Callithrix*, *Cebus*, Brazil.*
1845 V. Claussen, *Mycetes*(?) Brazil.
1845 Owen, *Macacus pliocænus*, Gray's Thurrock, Essex.
1859 Gervais, *Semnopithecus Monspessulanus*, Montpellier.
1859 Gervais, *Macacus priscus*, Montpellier.
1871 Gervais, *Cercopithecus*, Monte Bamboli.
1872 F. Major, *Macacus inuus*(?) Valley of the Arno.

From the restricted geographical distribution of the *Lemuridæ*, it is not surprising that no remains of this suborder have yet been discovered. The fossil monkeys as yet found in S. America belong to the *Hapalidæ* or *Platyrrhini*, still peculiar to the Neotropical region. All the rest belong to the *Catarrhini*, and some to the anthropomorphous genera. They all belong to the old world, but while some have been found in India, others inhabited Greece, France, Germany, and England.

P. S.

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* Ann. Nat. Hist., vol. iii. p. 426.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Total Eclipse in Java

[Extracted from a letter from Prof. Oudemans by Mr. J. N. Lockyer.]

I WAS not fortunate on the occasion of this eclipse. I had a spectroscope of Merz, sent by the Minister of the Colonies on the advice of the Academy of Sciences at Amsterdam; but the telescope to which it was to be adapted had no clock-motion. I took it with me to the Island Lawoengan in the Pepperbay, whither a Government steamer brought me and three other gentlemen. On the day of the eclipse we had three showers before totality, sky wholly overcast; but now and then the sun broke through between the clouds. Happily the clouds opened just before totality. My observations were therefore confined, like that of the other gentlemen of my party, to the general observations, following therein principally the suggestions and indications published in NATURE. I have already sent the results to the Academy at Amsterdam; and take the liberty of offering you herewith a copy of the general report, which I made up from the several partial ones for the Government.

My observations and those of my party have given me the conviction of the existence of an optical phenomenon, besides the purely solar phenomenon; not of an atmospheric origin (there is no question whatever of this), but of rays, variable during the totality, too variable to attribute them to luminous solar matter emerging from the body of the sun itself. I could follow the rays and some rifts as far as the moon's limb.

At Bintensory, the residence of the Governor-General, they were more successful, the weather being beautiful; there, as well as at Batavia, Mr. Bergsma caused observations of the declination of the magnet to be made during the whole morning, several days before, the day of, and several days after, the eclipse, at intervals of five minutes. The observations are now reduced for the influence of the moon, and he will propose to the Government to publish these observations and their reduction apart. The result of the observations is, that the movements of the magnet-needle during the eclipse have not deviated considerably from the common diurnal movement of the declination at this time of the year.

The "flying shadows" were very remarkable at Buitensorg, they were observed by persons wholly unacquainted with the phenomenon.

They were seen by Mr. Bergsma on a white wall directed E. 13° 30′ N. to W. 12° 30′ S., and on a sheet of white paper lying on a table. On the wall the shadows were inclined to the west, making with the horizontal line an angle according to one observer's measurement of 40°, and according to another's of 45°. They moved from E. to W. On the white paper they made an angle of 45° with the edges, which were perpendicular to the wall; they moved on the paper from S.E. to N.W. The phenomenon did not show itself as it is represented in "Secchi's Le Soleil," p. 158.

The shadows had a breadth of 5 to 6 centimetres; they were limited by lines with small irregular undulations; they were separated by regularly illuminated bands; the distance of the shadows was, according to Dr. Scheffer (the botanist), 1½ decimetres, and, according to Mr. Lang, about 3 decimetres or a foot. They moved parallel to themselves slowly; their velocity over the wall was about that of a horse in a moderate trotting pace. Mr. Bergsma saw the shadows from about three minutes before totality.

During totality they were not visible according to Mr. Lang, whom Mr. Bergsma had requested to pay particular attention to this point, only Mr. Lang saw now and then a slight change in the intensity of the light on the paper.

Immediately after totality the shadows appeared again, increasing and diminishing alternatively in strength, but growing gradually less and less distinct, although Mr. Bergsma continued to see them till about 5 minutes after totality.

Mr. Bergsma now describes the means proper to obtain more reliable observations on future occasions.

By construction and calculation I have deduced from Mr. Bergsma's data as to the direction of the shadows on the wall and the paper the following:—

I assumed the inclination of the lines on the wall to be 42½° with respect to a horizontal line, taking the mean between the computations of Messrs. Lang and Scheffe. That the shadow-lines made an angle of 45° with the edges of the paper, could be understood on two different theories—viz., that their azimuth was 121½° and 211½° (N.E.) Mr. Bergsma declared that 211½ was meant.

Now, if we pass a plane through a shadow-line on the wall and its prolongation on the paper, this plane intersects the horizon along a line directed in an azimuth of 31½° (N.E.), whereas the same plane has an inclination of 52⅓° to the west.

The normal on this plane meets the sky in a point having an azimuth of 121½°, and an altitude of 37⅓°. At the middle of totality the sun had an azimuth of 131°·4, and an altitude of 54°. Accordingly there is a difference of 10° in azimuth, and 16° in altitude. As regards the rough computation of the direction of the shadow-lines, this error may easily have been made, the more so as the observers were not prepared for an accurate observation of the phenomenon.

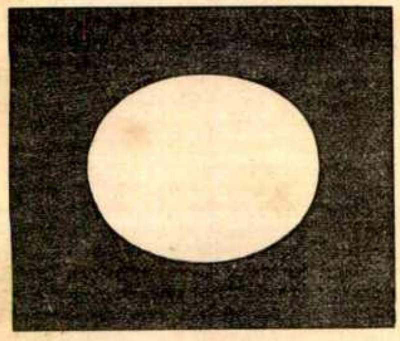
Thus it appears, without anticipating more accurate observations on the occasion of late eclipses, that the shadow-lines were situated in planes perpendicular to the sun's rays. They moved *from* the sun.

Singularly enough, neither at Tjilentap nor at the island Lawoengan, was anything of the phenomenon seen. At the island circumstances were very unfavourable, but at Tjilentap the sky was clear. OUDEMANS

Batavia, April 28

The Great Storm of June 18

OUT of a large quantity of hailstones collected here after the storm had subsided, and which were therefore partly melted, I selected one of the largest. The subjoined boundary line is the measure of a section of this hailstone through the poles, the form of it being a prolate spheroid, flattened on two sides, like a confectionary lozenge, if I may employ so vulgar a comparison. Many of the hailstones, however, as they fell, were jagged pieces of ice, the like of which I have never seen.



Edgbaston, Birmingham, June 21 C. M. INGLEBY

Spectrum of Lightning

I HAD a good view of the spectra of lightning during the storm of yesterday. Frequently there was only one bright line visible, this being coincident with the nitrogen line. At other times there were several bright lines, sometimes with, and at other times without, the nitrogen line. Several flashes showed a continuous sprectrum without visible lines. My instrument was a small direct-vision spectroscope, but sufficiently powerful to divide the sodium line. J. P. JOULE

Broughton, Manchester, June 19

YESTERDAY this neighbourhood was visited by a most terrific thunderstorm, such as I have never before seen in England. Indeed, it is stated that the last storm of similar severity occurred exactly 33 years ago to a day—rather a singular coincidence. The storm commenced here about half-past one, though distant thunder was audible at one o'clock. It was accompanied by violent wind, rain, and hail, and lasted about an hour, during which 0·66 in. of rain fell. The wind was S.E. at the time, but the storm came from the W. The hailstones are described as being, many of them, larger than marbles, and did a good deal of damage to glass. Several fatal accidents have occurred from the lightning, which for some time was almost incessant. I examined its spectrum with a miniature spectroscope, and succeeded in observing four or five lines. Their approximate positions, which I give below, are very roughly determined, and especially so toward the red end. They were obtained by comparison with the Fraunhofer lines, and with the carbon spectrum of a Bunsen burner. The former were mostly very faint from the darkness, but the atmospheric absorption bands near D were very marked. I am strongly of opinion that the spectrum is that ordinarily given by a spark in air, but was unable to make direct comparison. I have since examined a feeble spark by the same spectroscope, and the general appearance is very similar. I also tried observation of the long zig-zag flashes with a simple prism, but without much success, though I was able to see the spectrum. H. R. PROCTER

North Shields, June 19

Lines.	Wave-length.	Remarks.
α	about 66 8th-metres	—
β	,, 59 ,,	Doubtful.
γ	,, 56 ,,	—
δ	,, 53 ,,	—
ϵ	,, 50 ,,	Bright.

Water Analysis

MY attention has been directed to an article entitled "Water Analysis, I." published in NATURE of June 6. The article is unsigned, bearing neither name nor initials, and contains strange errors and misrepresentations, some of which I beg permission to correct.

First, there is a false date. The article states that in 1868 "Messrs. Chapman, Wanklyn, and Smith proposed to determine the organic matter in water from the amonnt of ammonia evolved when the water was treated with a strongly alkaline solution of potassic permanganate, and then distilled." The truth is, that our paper, proposing the process, and giving directions how to work it, together with examples, was read before the Chemical Society on June 20, 1867, and published in the *Journal* for the year 1867 (*vide* p. 445, *et seq*). Moreover, in the year 1867, our process was extensively employed by the Rivers Commission by Mr. Way, who was at that period the chemist on the Commission.

Next, I have to notice a misrepresentation. The article describes us as having at first stated that albumen gave up the whole of its nitrogen (in the form of ammonia) when treated with alkaline permanganate, and that afterwards we said that only a certain fraction was obtainable in that way.

We have never said that distillation of albumen with alkaline permanganate converted the whole of the nitrogen of the albumen into ammonia. The assertion in the article is therefore untrue. The circumstance to which your statement was intended to refer was the following.

In our paper read on June 20, 1867, we proposed two distinct modifications of the water process. In the one modification we evaporated to dryness with potash in the oil-bath, and afterwards distilled the residue with alkaline permanganate. The quantity of ammonia got by the operation with potash in the oil-bath, *plus* the quantity of ammonia got afterwards by permanganate of potash, is equal or nearly equal to the total ammonia which the total nitrogen of the albumen will yield.

On June 20, 1867, in addition to this early form of the water process, we described and recommended a second modification, consisting in the omission of the evaporation to dryness with potash. We boiled with potash, but did not take down to dryness, and then boiled with permanganate. At that date we knew, and mentioned in the paper, that omission to take down to dryness involved some loss of ammonia which potash should evolve. We did not know that failure to get the full yield with potash involved the ultimate sacrifice of a certain quantity of ammonia. That fact was afterwards ascertained by me, and published later in the autumn of 1867, and is duly recorded in the *Journal of the Chemical Society*.

The conviction that a really serviceable process of water-analysis must be a simple one, and the perception that a definite fraction of the total nitrogen was as good a datum as the total nitrogen itself, led me to persist in recommending the second modification rather than the first. Much experience in these matters has confirmed my judgment, and I do not repent the choice that we made.

Returning to the article. After having mentioned our experiments on papaverine, sulphate of cinchonine, narcotine, strychnine, sulphate of quinine, there is the following extraordinary statement:—

"If the authors had enabled us to ascertain the absolute error on the quantity taken instead of the percentage error, by giving us the quantities from which the results were taken, it would no doubt be much more apparent: the results given above in the case of Frankland and Armstrong's paper are absolute errors."

I invite you to open the *Journal of the Chemical Society*, May 1868, which is referred to in our treatise. We did give the quantities from which the results were obtained. Quoting from our memoir, you may read that we took 10 mgrm. of papaverine, and obtained 0·22 mgrm. of ammonia; that we took 10 mgrm. and 5 mgrm. of sulphate of cinchonine, and got respectively 0·57 and

0·27 mgrm. of ammonia; that we took 5·5 mgrm. of strychnine and obtained 0·30 mgrm. of ammonia; and that we took 10 mgrm. of sulphate of quinine and obtained 0·45 mgrm. of ammonia.

The absolute errors, therefore, were—

	Milligrammes of ammonia.		
	Calculated.	Found.	Error.
Papaverine	0·25	0·22	0·03
I. Sulphate of cinchonine	0·48	0·57	0·09
II. „ „	0·24	0·27	0·03
Strychnine	0·28	0·30	0·02
Sulphate of quinine	0·456	0·45	0·006

giving a mean error of 0·035 mgrm.

I have to remark, in reference to these five examples, that they are not cases selected by me to exhibit the accuracy of our process, but cases picked out from a great number, in order to exhibit what takes place under the most unfavourable circumstances. In contrast with these are Frankland and Armstrong's six determinations, five on urea and one on hippuric acid, given by themselves as exemplifying the accuracy of their method, and showing a mean error of 0·35 mgrm. of nitrogen—just ten times as much as ours under the most unfavourable conditions.

I observe you say that the amount of ammonia obtainable from albumen by the action of alkaline permanganate is influenced by the degree of concentration of the solution, the amount of heat applied to the retort, and consequent rate of distillation, and the time to which the solution is exposed to the action of the alkaline permanganate.

It would be just as true and as much to the point to say that the amount of carbonic acid obtainable from sugar depended on the amount of oxide of copper with which it is mixed, and the length of time to which it is exposed to a red heat.

I am able to affirm most positively that there is no difference in the yield of ammonia from albumen, whether the solution be of a certain strength or six times as strong, or whether the distillation be rapid or slow; and in proof of this I refer to a set of experiments on albumen, published in 1867. If the action of the permanganate be pushed to the ultimate limit, the yield of ammonia is constant.

Your assertion that water which has been distilled from permanganate, and gives no reaction with the Nessler test, yields ammonia on being again distilled with permanganate, will not astonish persons who have had experience in the working of our process. The explanation of this fact is now, I believe, tolerably well understood, and is simply this: that when water contains so minute a quantity of ammonia as not to impart a colour when 100 cubic centimetres of it are treated with Nessler test, it may still contain sufficient ammonia to yield a perceptibly ammoniacal distillate if one litre be made to yield 100 cubic centimetres of distillate.

In conclusion, you mention some difficulties in applying our process to the effluent water from sewage farms. I will not, on this occasion, describe how these difficulties are overcome. Suffice it to say that they have been overcome by very simple and obvious means.
J. ALFRED WANKLYN

11, Harrington Street, London, June 17

Parasite of the Beaver

MAY I occupy a few lines of your valuable space for a brief note upon the singular parasite of the beaver, *Platypsylla castoris* Ritsema (*Platypsyllus castorinus* Westwood)?

On the kind application of Messrs. Wayers and Roelofs, of Brussels, Mr. Ritsema very courteously presented me, some months ago, with a pair of this insect, the remarkable characters of which seem to deserve a more extended notice than has been given by himself or by Prof. Westwood, who almost at the same time described it from specimens obtained from a different source.

The former has classed it with the so-called suborder, *Suctoria*, or *Aphaniptera*, as a family or series equal in value to the *Pulicidæ* (fleas, jiggers, &c.), while the latter considered it so peculiar as to represent a new order of insects, which he named *Achreioptera*.

After a careful study of a series of beautiful dissections made for me by my friend the Rev. A. Matthews, I have to dissent from both of these views, and to regard it, in accordance with my impressions at first sight, as *Coleopterous*.

The appearance of the insect is such as to mark it, on the most superficial inspection, as a distinct family. In the wonderful structure of the mentum, with three immense posterior lobes, it shows an affinity, though remote, with the singular genus, *Leptinus*, which is also the type of a family (*vide* Le Conte, Proc. Acad. Nat. Sciences, Philadelphia, 1866, p. 368). But the lateral lobes in *Platypsylla*, broad triangular processes, are in *Leptinus* only narrow spines, projecting in the same manner over the gular plate.

In the form of the antennæ it resembles *Gyrinus* and *Parnus*, and in other less important parts of the body it has unmistakable affinities with various members of the Clavicorn series, such as *Staphylinidæ*, *Silphidæ*, and *Corylophidæ*, though especially with *Trichopterygidæ*, in the very extraordinary genus *Limulodes*, Matthews.

A very rare character is the reception of the antennæ in cavities on the dorsal surface of the prothorax; such characters are found in *Physemus* of the *Byrrhidæ*, *Mychocerus* Er., the affinities of which are doubtful, and in *Usechus* Motsch. of the *Tenebrionidæ*. In those three genera the antennal cavities are round fossæ, while in *Platypsylla* they are grooves extending along the whole lateral margin.

My object in the present note is not so much to express an opinion on the systematic position of this wonderful animal (which I will discuss fully in an illustrated memoir now in preparation), as to call the attention of your readers to the possible occurrence of similar epizoa on other aquatic mammals, especially rodents.

The complex affinities of this genus indicate that it either was in former times, or is at present, a widely distributed type. The European beaver, the capybara, and the musk rat, may, perhaps, when examined, be found to support allied forms.

I will conclude by observing that the insect has no organs with which to perforate the substance of its patron, and cannot eat living tissues or fluids; it is, therefore, not a parasite in the strict sense of the term, but an inquiline, living upon effete material, perhaps epidermal scales. The larva should be diligently sought for by those that have the opportunity, both in the houses and on the bodies of the beavers, as a knowledge of the development and transformations will be of importance in recognising more fully its affinities.

I trust that this note may stimulate further investigation on the part of some of your readers.

Lausanne, June 19
JOHN L. LE CONTE

Vespertilio

YESTERDAY a neighbour, in cutting down a very old, wide-spreading broadleaf (*Griselinia littoralis*), came suddenly on a great crowd of bats. Whilst he was chopping he noticed that his dog seized something, which he found to be a bat. From a huge hollow limb of the tree seventy-five bats were dislodged; they fluttered into the bush, keeping just above the ground.

Ohinitahi, New Zealand, Feb. 14
T. H. POTTS

Origin of Cyclones

I HAVE to thank Mr. Whitmee for his statement about the formation of cyclones at the Samoan and neighbouring islands in the latter part of the Southern summer. It will be seen that though I was ignorant of the fact when I wrote in my former letter on cyclones, it confirms my theory that they originate "in the meeting of the trade-winds in the northern and southern hemispheres, at some distance north or south of the equator." The cyclone region in which the Samoan and Fiji islands are situated is probably an extension of that of the Southern Indian Ocean.
JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, June 17

THE POPULATION OF THE PHILIPPINE ISLANDS

ACCORDING to the latest, not yet published, statistics, the Philippine Islands are inhabited by 7,451,352 inhabitants, distributed into 43 provinces and 933 cities or villages. 1,232,544 pay tribute to the Government, and the number of 7,451,352 is calculated on the supposition that about the sixth part of the whole has to pay tribute. As there exist in all the islands, even in Luzon, independent tribes, and a large number in Mindanao, the number of 7,451,352 gives no correct idea of the real population of the Philippines. This is not known at all, and will not be known for a long time to come.

The number of 7,451,352 is composed in the following manner:—

The Island of Luzon	4,467,111	in	508	villages
,, ,, ,, Panay	1,052,586	,,	92	,,
,, ,, ,, Cebu	427,356	,,	51	,,
,, ,, ,, Leyte	285,495	,,	43	,,
,, ,, ,, Bohol	283,515	,,	36	,,
,, ,, ,, Negros	255,873	,,	43	,,
,, ,, ,, Samar	250,062	,,	35	,,
,, ,, ,, Mindanao	191,802	,,	64	,,
,, ,, ,, Mindoro	70,926	,,	18	,,

The remainder on the other small islands.

The following is the division into 43 provinces:—

Abra	37,266	in	8	villages
Albay	341,493	,,	38	,,
Antique	131,886	,,	19	,,
Basilan	600	,,	1	,,
Bataan	67,362	,,	12	,,
Batangas	432,504	,,	21	,,
Bulacan	346,317	,,	24	,,
Bohol	283,515	,,	36	,,
Burias	2,430	,,	1	,,
Cagayan	114,396	,,	19	,,
Calamianes	27,189	,,	5	,,
Camarines North	42,525	,,	9	,,
,, South	434,016	,,	34	,,
Capiz	272,292	,,	32	,,
Cavite	173,193	,,	19	,,
Cebu	427,356	,,	51	,,
Cottabato	1,200	,,	1	,,
Davao	1,860	,,	1	,,
Hoilo	648,408	,,	41	,,
Hocos North	220,038	,,	15	,,
,, South	265,233	,,	21	,,
Islas Batanes	12,000	,,	6	,,
Isla de Negros	255,873	,,	43	,,
Isabela	47,067	,,	9	,,
Laguna	216,435	,,	28	,,
Lepanto	56,088	,,	81	,,
Leyte	285,495	,,	43	,,
Manila	354,348	,,	29	,,
Mashate y Ticao	17,190	,,	9	,,
Mindoro	70,926	,,	18	,,
Misamis	100,398	,,	32	,,
Morong	73,080	,,	12	,,
N. Ecija	167,325	,,	23	,,
N. Vizcaya	21,471	,,	6	,,
Pampauga	300,567	,,	29	,,
Pangasinan	431,691	,,	30	,,
Romblon	34,137	,,	9	,,
Samar	250,062	,,	35	,,
Surigas	73,770	,,	28	,,
Tayabas	155,280	,,	17	,,
Union	133,452	,,	13	,,
Zambales	109,044	,,	23	,,
Zamboauga	14,574	,,	2	,,
	7,451,352*		933	

The following division of the Philippine Islands is proposed, but not yet introduced:—

18 Provinces in 3 divisions

1st division.—Manila, Hoilo, Cebu, Hocos, Cagayan.

2nd division.—Pangasinan, Pampauga, Laguna, Cavite, Batangas, Albay, N. Ecija.

3rd division.—Bulacan, Camarines, Capiz, Negros, Leyte, Marianas.

The Islands of Mindanao, Basilan, Tolo (Soolo), Samales, and Balabac, will have a special government.

ADOLF BERNHARD MEYER

Manila, April 15

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* The Marianas Islands belong to the Government of the Philippines with 8,000 to 9,000 inhabitants.

MINERAL SPRING OF SHANA NEAR TREBIZOND

THE mountainous and volcanic district, or, to speak more correctly, belt, which skirts the northern coast of Asia Minor, beginning from Amastri, one hundred and fifty miles east of the Bosphorus, up to the Georgian valley and the Russo-Caucasian frontier, abounds in mineral springs, varying as to temperature and constituents, but generally endowed with hygienic properties, which are, to a certain extent, known and appreciated by the natives of the land. But few of these springs have been made the subject of scientific examination and analysis; so that the ingredients whence they derive their value, where not discernible to the unassisted senses, are in most cases matter of conjecture rather than of demonstration.

In one instance, however, that of a remarkable mineral source within this district, the obligingness of a resident Italian chemist, M. Marengo by name, has lately furnished me with some scientific data, not indeed as complete as might have been desired, yet enough for interesting information. These I will now give, accompanied by my own observations made during frequent visits to the locality in question.

About six miles east of Trebizond on the sea-coast stands the little fishing village of Covata, at the entrance of the valley which, as also the stream that flows down it, bears the same name. Following the valley some way inland towards the mountains where it originates, we come on the water-course and ravine of Shána, falling into that of Covata at nearly right angles, from east to west. "Shána" is, like most names of places hereabouts, a word of Laz, that is Mingrelian, origin, and signifies "heat." This ravine is narrow and deep; the rocks on either side are volcanic, chiefly mottled tufa of dark grey substance, speckled throughout with small black fragments of irregular shape and size imbedded in it. Vegetation, wherever the steepness of the slope allows it to take root, is most luxuriant; vines, olives, walnut trees, chesnut, sycamore, maple, poplar, with a dense undergrowth of alder and hazel. Down the bottom of the gorge flows a small torrent, which joins the river of Covata not far from its sea-mouth.

Tracking the narrow path which leads up to the Shána gorge for about four hundred yards, we come on a sort of widening-out, where a horizontal sheet of porous volcanic rocks spreads to some distance alongside of, but slightly elevated above, the course of the torrent. In the middle of this rock-sheet has been formed, partly by nature, partly by art, a small circular basin, nearly three feet in diameter, and averaging a foot or rather more in depth. This is constantly full of clear, limpid-looking water, which wells up through several irregular clefts in the stone bottom of the basin, and overflows it, the waste running off down the ledge into the neighbouring torrent, and leaving everywhere on its passage a thick bright-red deposit of oxide of iron, which stains the rocks, and even discolours the main-stream to some distance. Through the clefts just mentioned bubbles of carbonic acid gas rise in sufficient abundance to give the water the appearance of boiling; but the temperature is normal. This water is strongly impregnated with free carbonic acid; its taste is pungent and ferruginous, with a distinct, but, so long as it is fresh, a not unpleasant indication of sulphur. If it is put into a bottle, corked, and exposed to the heat of the sun, the expansion of the gas soon causes an explosion, driving out the cork, and even bursting the bottle.

Near this semi-artificial basin, and placed on a line with it one after another in the axis of the valley, are two other natural rock-hollows, one of several feet in extent, the other less; whence the same description of ferruginous water, mixed with bubbles of carbonic acid gas, issues continually, but not in equal abundance, the fissures below being partially choked up, whereas those in the circular basin are carefully kept open by the peasants. Here, too, the rock around is stained with bright red streaks of iron deposit. Also, carefully observing the torrent itself, which flows in a parallel direction a few feet distant, I noticed that bubbles of gas kept rising here and there from between the stones in its bed, and that the water, though tasteless higher up, here partook to a certain extent of the mineral acidity so strongly marked in the springs.

The analysis of the ingredients of the "Shána" water, as supplied me by M. Marengo, was not quantitative, but merely qualitative. I give it as follows, apologising at the same time for any technical inexactitude in my translation from the letter of the Italian document now before me:—

INGREDIENTS

Hydrosulphuric Acid . . .	Abundant
Carbonic Acid	Abundant
Sulphuric Acid	Not much
Chlorine	Scanty
Oxide of Iron	Very abundant
Lime	Abundant
Magnesia	Abundant
Alumina	Scanty
Soda	Scanty
Potash	Trace
Silica	Trace

Iron, free carbonic acid, sulphur, and magnesia, are the chief characteristics of this spring.

Among the Mahometan and Turkish-speaking population of the neighbourhood, the source goes by the name of "'Ilijeh," or "Healing," a term which they apply to almost every mineral spring of whatever description. As for this one in particular, the natives ascribe to it almost every sanatory virtue that a Holloway's advertisement could claim. In reality it is tonic, and, if its use be persevered in, alterative; the magnesia which it contains renders it at first slightly laxative. A considerable quantity of the water is brought in jars or bottles, which are filled and closed on the spot, to Trebizond, where it is much esteemed. The supply is unaffected by change of weather or season; only in summer the water is a few degrees cooler, as in winter warmer, than that of the torrent close by, which is often, during the severity of a Black-Sea February, changed into ice; whereas the little circular basin, in spite of its shallowness, never freezes.

The so-called "Greeks," who have a small peasant colony in the neighbourhood, have christened the source, in their modern corrupt dialect, "Iäsma," the correct word being Ἁγιασμα, or "sanctification," and have erected on an over-hanging rock close by, a small chapel, dedicated to I know not what saint, the supposed patron of the waters. The Turks, on the contrary, attach to it no religious idea whatever.

The peasants report the existence of another ferruginous spring some miles farther on among the mountains, ; but the precipitous character of the paths leading to it and the density of the forests, render it practically inaccessible to all but themselves.

Trebizond, May 28 W. GIFFORD PALGRAVE

THE DISPERSION OF SEEDS BY THE WIND

IN the very interesting notice of Grisebach's "Vegetation der Erde," which appeared in a recent number of NATURE,* reference is made to a paper by Kerner of Innsbrück, "On the Influence of the Wind on the Distribution of Seeds in Mountain Regions." As this paper was presented to the German Alpine Club, and no translation, as far as I am aware, has appeared in this country, with the exception of an abstract in the *Gardener's Chronicle*, it is probably almost unknown to English readers, and a short epitome of its most interesting features may not be unacceptable.

The idea that the wind performs a very important part in the distribution of plants, by the extensive dispersion of their seeds, is a very prevalent one. Mr. Bentham has, however, pointed out in his Anniversary Address to the Linnean Society in 1869, that this popular belief rests on insufficient data. If that portion of thistle-down which has been carried to a considerable distance by a high wind is carefully observed, it will generally be found to have left its seed behind it; and in the same order of Compositæ, several species of *Eclipta*, *Elephantopus*, *Anthemis*, and *Lapsana*, which have no pappus, have a much more wide-spread distribution than the majority of *Senecios*, for instance, with their light and broad pappus. The rapid spread of our common thistle, *Carduus arvensis*, in any new country where it once gains a foothold, is probably as much due to the persistent vitality of its roots as to the dispersion of its seeds. If the individuals in the same field are examined, they will generally be found to be all of one sex, showing that they must have been propagated by the division of the same individual. Of the extraordinarily rapid power of dispersion possessed by some plants independently of their seeds we have a familiar instance in the suddenness with which the Canadian water-weed, *Elodea canadensis*, filled up all our canals and water-courses within a few years of its first introduction; and yet up to the present time the male plant is entirely unknown in this country, and indeed in Europe; and it is probable that the whole of the stock now in England may have sprung by sub-division from the first imported specimen.

M. Kerner conceived the idea that a careful examination of the plants growing on moraines, and of the seeds found on the surface of glaciers, would throw considerable light on this interesting subject, since it is evident that they could only have arrived in those localities by the agency of the wind; and the results of an elaborate series of investigations are recorded in the pamphlet alluded to. Firstly, with regard to the moraines:—A list of five of these floras, from as many different moraines, consisting of limestone, schist, and gneiss, included 124 species, the following orders being the most largely represented:—Compositæ, 23 per cent.; Caryophylleæ, 10 per cent.; Gramineæ, 8 per cent.; Mosses, Saxifrageæ, and Salicaceæ, 6 per cent.; Cruciferæ, 5 per cent.; Ferns and Rosaceæ, 4 per cent.; Scrophulariaceæ, 3 per cent. Of the smaller families, the genera *Valeriana*, *Epilobium*, and *Juncus*, occurred the most frequently. The investigation of these lists, with a view to trace the origin of the plants, shows that the larger number of those which constitute the moraine flora are species widely distributed over the higher mountain regions in immediate proximity to the glacier. Less frequent are those plants which belong to the grassy plateaux of the lower elevations; and still less common species belonging to the meadow or wood flora of the lowlands, which maintain only a short and precarious existence.

The absence of this latter class of plants might, however, be due to the inability of the seeds to germinate under such unfavourable circumstances; and in order to determine this point, M. Kerner carried his researches to the surface of the glacier itself, examining both the animal and vegetable productions found thereon, with the following results:—

The animals found were entirely dead or benumbed insects belonging to the orders Lepidoptera, Hymenoptera, Coleoptera, and Diptera, and consisted of forty-three species, a considerable portion of which are found only in the highest mountain regions in the immediate vicinity of the glaciers; more than half the species were of very wide distribution, extending from the mountain valleys and neighbouring plains to the edge of the glaciers; very few being found only in the mountain valleys, and one only, the common honey-bee, being peculiar to cultivated districts. None of the insects found belong to extra-Alpine species, none of the kinds peculiar to the warm valleys of the southern Alps are represented; and the inference is unavoidable, that all the animals found on the glaciers have either strayed voluntarily, or have been driven by the wind, from districts immediately adjacent to the glacier.

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* Vol. v. p. 458, April 11.

The task of determining the seeds found on the surface of the glacier was much more difficult. The seeds of many Alpine plants have hardly been described; and in other instances it is difficult to distinguish between those belonging to several different species of the same genus. Thirty-six species, however, were determined with tolerable certainty, the majority of which were identical with the species previously recorded as inhabitants of the moraines. Here again the same results are established: not a single seed is found on the glacier, as not a single plant on the moraine, which does not belong to a species inhabiting the immediately adjacent mountain slopes or valleys. The conclusion from these facts seems inevitable, that the conveyance of seeds, even when provided with apparatus calculated for being floated in the air by horizontal currents, takes place only within very circumscribed limits; and that the prevalent opinion that they may be thus carried for very great distances is not supported by facts.

M. Kerner thus sums up the results of his observations:—

1. Only dust-like substances, such as pollen, spores, diatom-scales, &c., can be distributed by currents of air over wide stretches of land and sea in uninterrupted flights, and thus be brought into the alpine regions.

2. Fruits and seeds of flowering plants which are provided with a web-like floating apparatus that distends itself in dry air in the form of a parachute, are carried upwards by the ascending current of air which arises on sunny days in alpine regions on the cessation of the horizontal wind; but after sunset they sink again to the ground at a short distance in a horizontal direction; and the object attained by this floating apparatus is not so much the adaptation of the seeds for long journeys, as to enable them to settle on the projections and in the crevices of steep precipices and rocks, and to clothe with vegetation these rock-walls which are not easily accessible by other seeds.

3. The presence of membranous margins and wings favours the transport of fruits and seeds by horizontal currents of air; the horizontal distance, however, over which these seeds are carried scarcely ever extends farther than from one side of a valley to the other, and the distribution of the fruits and seeds of flowering plants, in so far as this is caused by currents of air, can only proceed gradually and step by step.

4. Fruits and seeds which are deficient in any kind of appendages that facilitate flight are scarcely influenced by currents of air; it is only when these seeds are of very minute size and extremely small weight that they can be driven short distances by horizontal winds.

It appears, therefore, that the idea that seeds are distributed to great distances by the wind, if not to be treated as a popular error, at least requires a much larger foundation of fact than it at present possesses, in order to be accepted as a scientific truth. A series of observations of this nature, if carefully conducted, is a substantial gain to Science, and may assist the determination of great physiological questions in hundreds of ways. They are within reach of every intelligent resident in the country possessed of ordinary powers of observation; and yet how few interest themselves practically in carrying them out!

A. W. B.

LYELL'S PRINCIPLES OF GEOLOGY*

In our last notice, after a sketch of the methods of investigation employed by Sir Charles Lyell, and an outline of the principles deduced therefrom, we gave a few examples of the kind of proofs brought forward by him to show that the degrading and transporting forces which we see in operation are producing similar phenomena to those we observe in the sedimentary rocks, and that, given sufficient time only, effects on as great a scale must be the inevitable result.

We will now select some of the evidence adduced by him to show that the igneous forces also, the movements of upheaval and depression, are as active, and the products of eruption on as grand a scale, as we have any reason to believe they have ever been within the period over which our observations extend.

The consideration of what suggested the former greater intensity in the subterranean forces, viz., the supposed vast magnitude of the ancient igneous rocks, and the proofs of variations in climate, leads Sir Charles into an investigation of the astronomical and geographical causes of vicissitudes of climate, which involves an inquiry into the vexed questions of oceanic circulation, and the effect of various changes of conditions on the organic world in the extinction of species, and their replacement by new forms of life.

It certainly may at first seem difficult to believe that the forces which produce upheaval and eruption have not varied in intensity throughout the whole period of which we have any record, and yet that over many large tracts of country, where now the faintest vibration of the distant earthquake is exceptional and rare, we have thousands of feet of volcanic ash and lava, and great masses of matter which have apparently been injected in a molten state into the fissured rock. But this difficulty has arisen because the vastness of the ancient volcanic deposits has been assumed without sufficiently detailed observation, and the magnitude of modern igneous action has been underrated, while the most important point, the transference of paroxysmal action from one area to another, has been overlooked.

Speaking of contemporaneous volcanic deposits in the older rocks, Sir Charles Lyell says:—"If one of these igneous formations is examined in detail, we find it to be the product of many successive ejections or outpourings of volcanic matter. As we enlarge therefore our knowledge of the ancient rocks formed by subterranean heat, we find ourselves compelled to regard them as the aggregate effects of innumerable eruptions, each of which may have been comparable in violence to those now experienced in volcanic regions" (p. 114). This question, however, Sir Charles does not investigate in the "Principles," which deals with the modern changes of the earth; and we will pass on to notice some of the examples he gives to show the magnitude of modern igneous action.

First, as to the fact that changes of level are going on:—"Recent observations," says Sir Charles Lyell, "have disclosed to us the wonderful fact that not only the west coast of South America, but also other large areas, some of them several thousand miles in circumference, such as Scandinavia, and certain Archipelagos in the Pacific, are slowly and insensibly rising; while other regions, such as Greenland and parts of the Pacific and Indian Oceans, in which circular or coral islands abound, are as gradually sinking" (p. 128). The atolls are themselves a proof of oscillations of level. The coral zoophytes live only within certain distances from the surface, and, having commenced nearly all round an island, keep building up as the island goes down till they have formed a ring of coral. The accompanying ideal section across such an island enables one to understand the mode of growth. A channel is kept open through one side, probably at first by the stream, which drains the island, and carries down mud and fresh water, and afterwards by the scour of the tide. Whenever an area covered by such islands is upheaved, and the reefs lifted up above the breakers, or the waves and wind have heaped up broken coral rock and shell around, the surface soon gets weathered, and forms a soil on which plants and animals settle and live. Sometimes the top of the island around which the coral was built is still seen; sometimes it has disappeared altogether beneath the sea. We subjoin a sketch of one of these circular reefs.

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* "The Principles of Geology, or the Modern Changes of the Earth and its Inhabitants considered as Illustrative of Geology." By Sir Charles Lyell, Bart. Eleventh and entirely revised edition. (London: J. Murray, 1872.) (The Second Volume has been issued since the appearance of our last notice; see NATURE v. p. 456.)

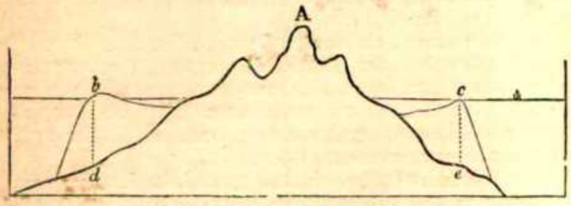

FIG. 1.—SUPPOSED SECTION OF AN ISLAND WITH AN ENCIRCLING REEF OF CORAL

A The Island: *b*, *c* highest points of the encircling reef between which and the coast is seen a space occupied by still water.

But besides such indirect evidence of gradual change of level, it is a matter of observation that as an accompaniment of volcanic action we frequently have sudden movements of small extent. For instance, in the destructive earthquake which visited Chili in 1822, the coast was raised from 2 ft. to 4 ft., while farther inland the rise was estimated at from 5 ft. to 7 ft., and off the port of Penco, if the reports of the inhabitants are to be believed, there was a rise of 24 ft. during the single earthquake of 1751. In New Zealand, during the earthquake of 1855, a fault 10 miles long, with a displacement of 9 ft., was produced.

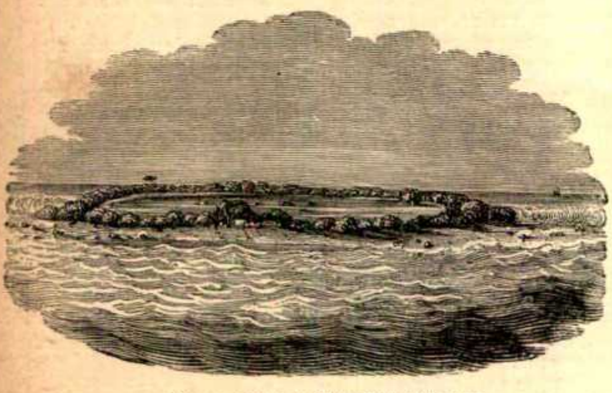

FIG. 2.—VIEW OF WHITSUNDAY ISLAND

Supposing an elevation of 7 ft. occurred only once every century, it would require less than 150,000 years to form a chain as high as the Pyrenees, and if repeated three times in a century would be sufficient to account for the Andes in the same time.

"It may be instructive," says Sir Charles, "to consider these results in connection with others already obtained from a different source, and to compare the working of two antagonistic forces—the levelling power of running water, and the expansive energy of subterranean heat. How long, it may be asked, would the Ganges require . . . . to transport to the sea a quantity of solid matter equal to that which may have been added to the land by the Chilian earthquake? The discharge of mud in one year by the Ganges at its mouth was estimated at 20,000,000,000 cubic feet. According to that estimate it would require about four centuries before the river could bear down from the continent into the sea a mass equal to that gained by the Chilian earthquake" (p. 97).

In volcanic districts especially we may expect evidence of recent upheaval and depression, and so we often have marine beds forming the base of a volcano, or submerged volcanos, whose leading features seem to be due to subaerial action. We may, for instance, mention the case of Etna, and refer our readers to the interesting line of reasoning by which our author works out the history of that mountain, showing that it was formed by degrees, of matter heaped up upon marine beds of comparatively recent age, which have now been lifted up to a considerable height above the sea, and further proves that at one time there were two principal craters from which matter was ejected, but that now, owing to subsequent explosions and denudation, an enormous valley occupies what was the top of the mountain.

As an example of a submerged volcano we may mention Santorin, with regard to which Sir Charles Lyell says:—

"We may conceive, therefore, if at some former time the whole mass of Santorin stood at a higher level by 1,200 feet, that this single ravine or narrow valley, now forming the northern entrance, was the passage by which the sea entered a circular bay. But at a still earlier period, when the ancient volcanic cone—of which the outer islands are the remains—was still more elevated above the level of the sea, there may have been a deep valley of subaerial erosion cut by the principal river which then drained Santorin, which may have consisted of one lofty volcanic cone, afterwards truncated by a paroxysmal explosion such as we have already spoken of in the case of Galongoon" (p. 72).

We subjoin Sir Charles Lyell's sketch (Fig. 3), which it will be interesting to compare with that of the unsubmerged summit of Etna.

We select also his ideal section across Barren Island (Fig. 4), to help to realise its manner of formation.

It may be worth calling attention to the similarity between the submerged crater, with its deep channel leading into it on one side, and the coral Atoll of which we have given figures above (Figs. 1 and 2). Nature has many ways of arriving at apparently analogous results; but close examination shows how varied are her methods.

Sir Charles also points out that, in the quantity of matter ejected, modern eruptions will bear comparison with any we know of in ancient times. In order to help us to realise the enormous volume of the lava poured out from Skaptar Jokul in 1783, he considers "how striking a feature" the two streams of lava then poured out "would now form in the geology of England, had they been poured out on the bottom of the sea after the deposition and before the elevation of our secondary and tertiary rocks." From one we should have a mass 100 ft. thick and 10 to 15 miles broad on the oolitic hills overlooking the vale of Gloucester. It would be traced for a distance of about 90 miles to the neighbourhood of London, where, "crowning the highest sands of Highgate and Hampstead, we might behold some remnants of the current some 500 ft. or 600 ft. in thickness, causing those hills to rival or even to surpass in height Salisbury Craigs and Arthur's Seat" (p. 52); while the other stream might be traced from London to the coasts of Devon and Dorset. The description given (pp. 104-106) of the volcanic outburst in the island of Sumbawa can hardly be read without our feeling that we know of no one ancient volcanic rock comparable in extent to the deposit of ash which must have resulted from the single eruption of 1815.

After having shown that these tremendous effects of volcanic action on the surface are "insignificant when contrasted with the products of heat in the nether regions," Sir Charles says (p. 211):—"The continual transfer of the points of chief development of the earthquake and volcano from one part of the earth's crust to another is established as a general law by the clearest geological evidence. We have also seen that volcanic operations are now in progress on the grandest scale, and also that single currents of lava of modern date are as voluminous as any which can be shown to have ever poured out in the earliest eras to which our geological retrospect can be carried."

The doctrine of the former greater intensity of the igneous forces, connected as it generally was with the hypothesis of the primæval igneous fusion and gradual cooling down of the planet, of course involved the theory of the former higher temperature of the surface of our earth; and therefore all indications of a warmer climate over any area in the ancient seas were supposed to point to a universal higher temperature over the globe. But Sir Charles Lyell points out that "the climate of the extra-tropical regions has been by no means always hotter than now; but on the contrary, there has been at least one period when the temperature of those regions was much lower than at present" (p. 173).

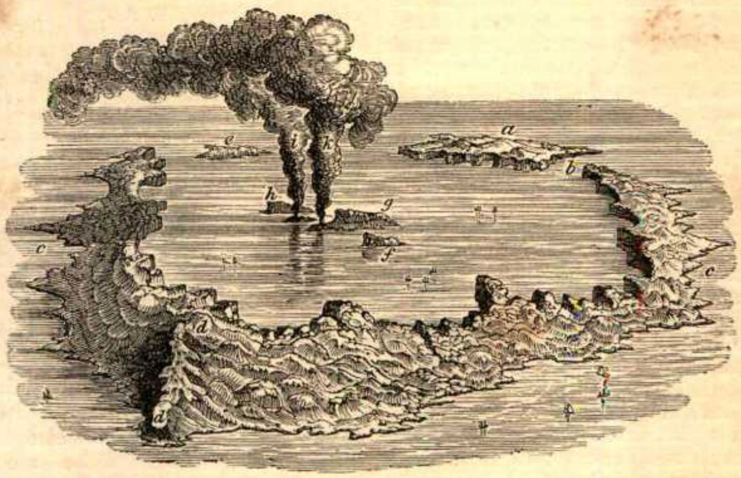

FIG. 3.—BIRD'S-EYE VIEW OF THE GULF OF SANTORIN DURING THE VOLCANIC ERUPTION OF FEBRUARY 1866

a Therasia; *b* the northern entrance, 1,068 feet deep; *c* Thera; *d* Mount St. Elias, rising 1,887 feet above the sea, composed of granular limestone and clay-slate, the only non-volcanic rock in Santorin; *e* Aspronisi; *f* Little Kaimeni; *g* New Kaimeni; *h* Old Kaimeni; *i* Aphroessa; *k* George.

Space will not allow us to follow our author while he proves, from an examination of the circumstances under which we find similar and dissimilar climates at the present day, that geographical conditions produce far greater effect upon climate than we have reason to believe would result from any astronomical combinations. Dependent to a great extent upon geographical conditions we have prevalent winds, which materially influence climate, and moreover give rise to most of the great ocean currents.

About the origin of these, however, there has been some controversy. Sir Charles considers the various theories very fully, and shows that the great currents are due to prevalent winds. "That movements," he writes, "of no inconsiderable magnitude should be impressed on a wide expanse of ocean by winds blowing for many months in one direction may easily be conceived, when we observe the effects produced in our own seas by the temporary action of the same cause. It is well known that a strong south-west or north-west wind invariably raises the tide to an unusual height along the west coast of England and in the Channel; and that a north-west wind of any continuance causes the Baltic to rise 2 ft. and upwards above its ordinary level" (vol. i. p. 492).

It is clear that when the surface water is being thus driven continuously for a long time in one direction against a shore or into a *cul de sac*, there must be an undercurrent formed by the head of water thus produced. But in deep basins there is no reason why the water should not remain embayed for ages, and, having been at any time cold, should never receive sufficient from above or below to raise its temperature.

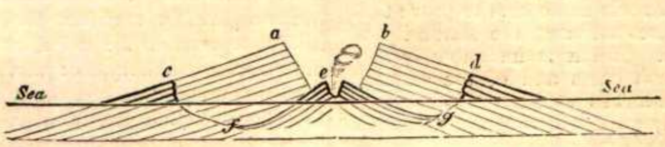

FIG. 4.—SUPPOSED SECTION OF BARREN ISLAND, IN THE BAY OF BENGAL

In the Mediterranean and the smaller seas connected with it there seems to be a great complication of current-producing causes, which have proved a fertile source of speculation and controversy ever since Aristotle puzzled over the currents of the Euripus. That land-locked sea is too small to have any considerable tide generated within itself, but the Atlantic tide rushes in and out with great force. The vast surface-current of the Atlantic, and the prevailing westerly winds, increase the in-going tide, and check, and generally altogether overpower, the surface part of the out-going tide, so as to give rise to an upper and a lower current in opposite directions through the Straits of Gibraltar.

In addition to these causes, there is the enormous evaporation during the hot season and the excess of fresh water poured in during the rainy season, which must produce a great effect. But as each tide would, when there was a deficiency of water, bring a little more in, and when an excess of water, take a little more out, this adjustment being made twice every day it does not seem likely that either evaporation or rain would make any appreciable difference in the currents at the Straits. Some authors have referred oceanic circulation to difference of specific gravity, due to difference of temperature or amount of salinity; but, though this is a *vera causa* which might in some cases explain similar phenomena, Sir Charles shows, by reference to the observations of Captain Spratt and others, that the currents of the Mediterranean, and, indeed, all observed currents, are due to other causes.

The question of the dependence of climate, both subaerial and sub-aqueous, upon geographical conditions, is very important in its bearing upon the changes in the inorganic world. For those who believe that in the history of the crust of the earth we have evidence of alternate periods of universal catastrophic action and repose would be quite prepared to believe that in the world of life also there were alternations of destruction and creation; but to those who hold that the face of the globe has been, and is for ever being, modified by the gradual action of forces always in operation, it seems *a priori* probable that Nature should have provided the organisms which inhabit this ever-shifting earth with modifiability somewhat commensurate with the changes of the world in which they live.

A mountain has been raised and chiselled out into its present form by operations extending over a period so vast that no one can have witnessed them. A species has been changed into something quite different by processes requiring a length of time so great that no one can have watched them.

Whatever may have been the chief *causes* of the movements of upheaval, it is a *fact* that movements are going on which bring different parts of the crust within reach of denudation, and that, given sufficient time, mountain ranges must be the result.

So, whatever may be the *origin* of the variations, it is a thing definitely known that variations of the same kind as those which are considered to form specific differences do occur; it is a matter of experiment that these variations can be accumulated and perpetuated by selection; it is a matter of observation that Nature does select. The burden of proof that there are any limits to variation or natural selection rests with those who hold it.

It has been objected to the doctrine of the origin of species by natural selection that some totally distinct classes of animals have corresponding organs, so similar that it is difficult to suppose that they can "'have been brought about in two independent instances by merely indefinite and minute accidental variations.' Yet these organs in the two types must have been developed in entire and complete independence one of the other; for it would be impossible to find a common ancestor without going back to some very simple form not yet provided with even the rudiments of vision" (p. 498).

Sir Charles quotes Mr. Darwin and others to prove that in some at least of the cases adduced the similarity of structure was exaggerated. Still it is undoubtedly very great, and the study of such cases and of the mimetic forms which Mr. Wallace has so well described, makes us feel that what we chiefly want to know more about, is the law which governs the first appearance of varieties. Such facts do not so much furnish arguments against the doctrine of the origin of species by natural selection, as in favour of the existence of some law according to which external conditions and the requirements of the individuals may tend to produce variation in a given direction.

How vast and how perplexing are the questions raised by the study of the modern changes of the earth and its inhabitants; but the calm philosophic spirit which pervades the "Principles of Geology" leads us to hope that it may promote in no small degree that education which will render it "possible to welcome new truths," although they may at first appear to be "out of harmony with cherished associations of thought."

T. McK. HUGHES

NOTES

WE are informed that the Directorship of the National Observatory at Marseilles has been offered to Dr. Janssen.

25,000 rupees have already been subscribed towards the Archdeacon Pratt Memorial Fund.

WE are glad to hear that the local committee at Brighton are forming a temporary museum, to be opened during the Meeting of the British Association.

THE prizes in the Faculties of Art, Science, and Fine Arts, of University College, London, were distributed by the Right Hon. S. Cave, M.P., in the Botanical Theatre of that institution on Tuesday last. The attendance was very small, and several even of the professors absented themselves; but, notwithstanding this bad management on the part of the authorities, the proceedings were exceedingly animated and highly interesting to those engaged in the advancement of education. The report of the Dean, Prof. Croom Robertson, showed a very marked improvement in the condition of the College as well as of the School, the number of students during the past session having been greater than in any previous year. Amongst those who distinguished themselves the most notable were four ladies: Miss Orme, who was presented with the first prize and the first certificate for Political Economy, the only class in the Faculty of Arts which has as yet been opened to ladies; Miss Lupton, Miss Malden, and Miss Wylde, who received medals in the Fine Arts Faculty. The third certificate in this class was also taken by a lady; the number in the class being about thirty gentlemen and six ladies. The genuine and enthusiastic applause of the students at these successes leaves nothing to be wished for, except the continuation of that liberal policy for which University College has always been remarkable. Miss Orme had previously greatly distinguished herself at the examinations of the University of London.

THE following telegram has been received at the Admiralty from Aden, dated June 17:—"Dawson and party have returned to Zanzibar, Mr. Stanley having arrived with despatches from Livingstone: alive and well." Letters of that date from Aden are now due.

BESIDES the Minor Scholarships or Exhibitions at St. John's College, Cambridge, there will be offered for competition this year an Exhibition of 50*l.* per annum for proficiency in Natural Science, the Exhibition to be tenable for three years in case the Exhibitioner have passed within two years the previous xamination as required for candidates for honours; otherwise the Exhibition to cease at the end of two years. The Examination will commence on Friday, the 13th of December; in (1) Chemistry, including practical work in the laboratory; (2) Physics, viz., Electricity, Heat, Light; (3) Physiology; they will also have the opportunity of being examined in one or more of the following subjects: (4) Geology, (5) Anatomy, (6) Botany, provided that they give notice of the subjects in which they wish to be examined four weeks prior to the Examination. No candidate will be examined in more than three of these six subjects, whereof one at least must be chosen from the former group. It is the wish of the master and seniors that excellence in some single department should be specially regarded by the candidates. They may also, if they think fit, offer themselves for examination in any of the Classical or Mathematical subjects. Candidates must send their names to one of the tutors, Dr. Parkinson, Mr. Bonney, or Mr. Sandys, before the commencement of the Examination. The Minor Scholarships are open to all persons under twenty years of age, whether students in the University or not, who have not yet commenced residence in the University or who are in the first term of their residence. A Minor Scholarship is tenable for two years, or until the scholar is elected to one of the Foundation Scholarships. The Exhibitions are not limited in respect to the age of candidates. It is understood that minor scholars or exhibitioners may be candidates for Sizarships.

The announcement last week that Mr. E. R. Lankester had gained a Natural Science Scholarship at Exeter College, Oxford, should have read "Fellowship."

The East London Museum at Bethnal Green was formally opened by the Prince and Princess of Wales on Monday last.

The Devonshire Association for the Advancement of Literature, Science, and Art, will shortly hold its annual meeting at Exeter, under the presidency of the Bishop of the Diocese.

Prof. Humphry gave his second lecture on "Human Myology" at the College of Surgeons on Wednesday, the 19th. In it he traced the elements of the lateral muscle upon the limbs, forming muscular expansions over them which, he said, were most clearly marked in the rudimentary claw-like limbs of some snakes, but sufficiently distinct in ourselves. In the upper limb, for instance, they form a superficial ventro-appendicular cone, which is divided, sectorially, into the pectoralis, the latissimus dorsi, and the trapezio-deltoid. In the lower limb the corresponding sheet is divided into the gracilis, the gluteus magnus, and the sartorius, with the tensor vaginæ femoris. The deeper layers of the ventro-appendicular cones more closely invest the shoulder and hip-joints. That in the upper limb he divided into the infra-spinatus and teres minor, the coraco-brachialis and subscapularis, and the supra-spinatus; and that in the lower limb he divided into the gluteus medius and minimus, the adductors and obturator, the iliacus and the pyriformis. The professor remarked that the developmental processes are so freely modified in accordance with the special requirements of each limb that an exact homological comparison of the muscles is out of the question; and he showed that the difference in the direction of the rotation of the upper and of the lower limbs upon their respective girdles has been attended with considerable modification of the attachment, especially of the insertion of the several muscles. He spoke of the clavicle as an ossification in one of the intermuscular septa of the ventral muscle, and as, therefore, corresponding serially with the epicostal or "intermuscular" bones developed in the abdominal wall of some lizards. Poupart's ligament belongs to the same series, and spans the crural arch as the clavicle spans the brachial arch. Various points in the disposition of the muscles of the upper parts of the limbs and the purposes served by them were discussed.

Signor G. A. Pasquale, in a paper presented to the "Accademia delle Scienze fisiche e matematiche" of Naples, attributes the injury done to vegetation by the recent eruption of Vesuvius neither to scorching nor to the mechanical action of the ashes in closing the pores of the leaves, the effect being much more sudden than if due to the latter cause; but to the injurious effects of the chloride of sodium which falls in considerable quantities with the ashes.

The following account of the recent thunderstorm at Birmingham, by Mr. T. L. Plant, is taken from the *Gardener's Chronicle*:—"Birmingham and vicinity were visited on June 18 by a thunderstorm, accompanied by the most tremendous quantity of rain and enormous pieces of ice, ever registered within my records. The sudden heat after the low temperature in the early part of last week caused the air on the 17th and 18th to become highly surcharged with electricity. On Monday night the heat was intense, lowest thermometer 62°. The rapid increase of temperature will be understood by the following copy of my daily readings from the 12th:—

Highest Temperature in the Shade.

June 12	62°	June 17	86°
„ 13	69°	„ 18	88° (highest temperature recorded in June since 1858
„ 14	77°		
„ 15	80°		
„ 16	82°		

At 12.45 the storm commenced, and lasted three hours and a half. For fully half an hour the rolling thunder was incessant. The depositions of ice began about two o'clock, and during a period of 20 minutes to half an hour there was a fall of large frozen bodies, mingled with tremendous rain, to an extent that finds no parallel in these annals. Some of the pieces of ice (which were of most irregular formation) measured quite an inch in length. During the height of the storm the wind was high and calm in alternate succession, and changing to various points of the compass, and ultimately south, as at first. The fall of rain in this great tempest was 2·47 inches. This is the largest quantity that has been registered in Birmingham, even exceeding the great storm on the evening of July 6, 1845. Most of the rain (which is equal to 250 tons of water to the acre) fell in 45 minutes."

The School of Science in connection with the Albert Memorial Museum at Exeter shows the zeal with which science is being cultivated in that city. The number of individual students under instruction during the current session has been 67, viz.:—7 in elementary mathematics, 13 in theoretical mechanics, 36 in inorganic chemistry, 9 in vegetable anatomy and physiology, 9 in systematic and economic botany, 7 in physical geography, 4 in machine construction and drawing, and 9 in building construction. The Museum has made considerable progress during the year.

We have again to notice the increasing success of artizan students at the Oldham School of Science and Art, in practical Inorganic Chemistry; nineteen have passed out of twenty-one examined by the Department.

From the Report of the Free Libraries Committee of Birmingham for 1871, we are glad to see that, although the number of volumes in the library bearing on science is small, the demand for them shows considerable interest in these subjects among the frequenters of the library.

The Committee of Trustees of the Industrial and Technological Museum of Victoria have issued their report for the year 1871. The progress of the Institution is spoken of as having been satisfactory, the number of visitors having greatly increased as well as of objects in the Museum. Illustrations of the mineralogical wealth of our colonies must always be of the highest importance, and there is now in the Museum a collection of the rocks of Victoria, classified and labelled, as well as a large series of fossils from different parts of the country which have not yet been classified. The models of mining machinery, formerly in the University Museum, are now also exhibited in this Museum, forming a most important and complete collection, which has excited great interest among the visitors. The exhibition of vegetable products, illustrative of their industrial uses, is rapidly increasing, and is now being systematically arranged, but is not yet catalogued. The collection of animal products, illustrative of their industrial uses, is very useful and complete. Courses of lectures have been delivered at the Museum, the primary object of which has been to make science in its relation to industry known among the artisan and mechanical classes, and the committee hope that this object has been in some measure attained.

Class lectures for practical instruction and laboratories have also been in operation.

THE circular of the University of Washington for the current year contains a catalogue of the officers and students, and a programme of the course of studies required in the different departments. In these the Physical and Natural Sciences hold a conspicuous place.

THE Deutscher Universitäts-Kalender, by Dr. F. Aschersen, published half-yearly, contains a mass of valuable information respecting all the German universities, including the names of the professors in the several faculties, the subjects for the academical prizes, &c.

WE have on our table the Reports of the Mining Surveyors and Registrars for the Colony of Victoria for the quarters ending June 30, Sept. 30, and Dec. 31, 1871.

THE Report of the Chief Commissioner of Mines for the Province of Nova Scotia for the year 1871 is printed. A report is appended of the Provincial Museum, recently established, and designed to be a permanent exhibition of the industrial resources of the province, combined with a Museum of Science and Art. The mineralogical and zoological departments appear to be well represented.

WE have received the Monthly Record for January of Results of Observations in Meteorology, Terrestrial Magnetism, &c., taken at the Melbourne Observatory, under the superintendence of Mr. R. J. Ellery.

THE American Palestine Exploration Society has, we learn from *Harper's Weekly*, lately received paper squeezes of two basaltic stones inscribed with Phœnician characters similar to, and perhaps companions of, the celebrated Moabite stone of which we have heard so much. The acquisition of the stones themselves has been a subject of much rivalry between the British and American societies; in consequence of which the Arabs, believing them to be extremely valuable, have hidden them, although it is hoped without destroying them, as was done with the Moabite stone. These squeezes were obtained by two well-known Americans, the Rev. D. Stuart Dodge and Frederick S. Winston, and have been forwarded by them to New York. Pen-and-ink copies have already been received, and have lately been lithographed and distributed among American scholars. It is not certain that the stones from which these squeezes were taken are genuine antiquities, the Orientals being unfortunately too well versed in the art of manufacturing such objects, so as to meet any demand. There is, however, a strong probability that they are what they profess to be. At any rate, they will probably before long be subjected to such an examination by experts as will determine their true character.

THE Report just issued of the Proceedings of the Geologists' Association includes detailed accounts of the visits and excursions made by the association during March and April 1871, and Messrs. R. and A. Bell's paper on "The English Crags and their Stratigraphical Divisions."

THE ninth Annual Report of the Wigan Field Naturalists' and Scientific Society contains an important paper by Mr. J. Perrins, on "The Duration of the Wigan Coalfields," illustrated by a coloured section. The Report is otherwise chiefly occupied by accounts of the different excursions of the society.

THE Bury Natural History Society has issued its first Report for the time from its foundation, in January 1868, to December 1871. Its object has been chiefly the investigation of the natural history of the district, which has been pursued with vigour; and the Report contains more or less complete lists of the flowering plants and ferns, lepidoptera, birds, mollusca, fishes, reptiles, and mammalia of the neighbourhood of Bury (the last four classes being somewhat oddly classified as the "animals" of the district).

THE "Verhandlungen der k.k. Zoologisch-botanischen Gesellchaft in Wien," for 1871, contains a number of valuable papers. Among the more important are:—Contributions to a knowledge of the Territelariæ, Thorell (or Mygalidæ), by A. Ausserer; Enumeration of the Cryptogams of Venetia, by Baron v. Hohenbühel-Heufler; Synopsis of the Fishes of the Red Sea, Part II., by Dr. C. B. Klunzinger; Monograph of the genus Certhiola, by Dr. O. Finsch; Monograph of the genus Hylæus, by Prof. Forster.

A PERIODICAL, called the *Economista di Roma*, is now published in Rome, and contains papers upon finance, agriculture, commerce, trades, public works, and statistics.

THE following reprints lie on our table, which we commend to the notice of those interested in the various subjects:—"How Fishes Breathe," by Mr. John C. Galton, from the *Popular Science Review*; "An Account of some Experiments relating to the Influence exercised by Colloids upon the Forms of Inorganic Matter," by Dr. W. B. Ord, from the "St. Thomas's Hospital Reports;" and "Non-existence of Projectile Forces in Nature," by Mr. J. A. Parker, a paper read before the American Institute.

IN a lecture on "The Influence of Human Progress on Medical Education," delivered at the Royal Victoria Hospital, Netley, Dr. W. Aitken gives an interesting sketch of the causes which have led to the improvements in the condition of medical education which have taken place especially within the last fifty years.

AN important ornithological work is announced from America as in the press, to be published by the Naturalists' Agency, Salem, Mass.—"A Key to North American Birds," by Dr. Elliott Coues, to be illustrated by seven steel plates and upwards of 250 woodcuts, and designed as a manual or text-book of the birds of North America.

WE learn from the *Garden* that Dr. Asa Gray, of Cambridge, Massachusetts, the author of "How Plants Grow," and of many important works and papers on botany, has lately brought out another little book, entitled "How Plants Behave," which deals with the climbing and other habits of plants, and is as likely to prove as valuable as the first-mentioned work.

MESSRS. W. and A. K. JOHNSTON have issued "The Edinburgh Sixpenny Quarto Atlas," a marvel of cheapness. It contains sixteen coloured maps, which, though small, are executed in the style with which we are so familiar in the productions of this house.

AS an illustration of the success which generally attends well conducted zoological gardens and aquaria, we may state that, although quite recently organised, the receipts from 216,000 visitors to the aquarium at Berlin for the year 1871 amounted to nearly 40,000 dollars.

WITH reference to the alligator story which we recently printed, Mr. W. C. Easton, who found the nest containing sixty-seven eggs on Eighteen Mile Island, Fitzroy River, on the 31st January last, informs the *Rockhampton Bulletin* that he at that time placed four eggs under a hen, and on visiting the hen's nest on March 14, found two young alligators had broken their shells, and were alive and doing well. They were then of slender form, about ten inches long. Mr. Easton, who is well acquainted with the habits of the alligator, expects his young saurians will be strong enough to bring into Rockhampton for exhibition in the course of a month or six weeks.

ATOMS AND MOLECULES [*]

THE atomic composition of ponderable matter is a fundamental postulate in the theory of chemical equivalency. By the application of the principles of experimental research, and by methods essentially modern, resulting in the discovery of many elementary bodies and their modes of combination, a conception of very great antiquity has been rendered more distinct and worthy of credence When this conception took definite form is not known. Indeed, it is one of the many speculations naturally elicited in discussing those subtle questions pertaining to the existence of matter and its relations to mind or spirit, the solution of which has always baffled, and will continue to baffle, the most profound thinkers. In attempting to unfold the mysteries of nature by the deductive process, the ancient teachers of Cosmogony were brought into direct conflict of opinion regarding the ultimate condition of matter. That it is composed of indestructible atoms which admit of no division, seems to have been the notion of some Oriental sages. Under the genius of the Greek philosophy this notion assumed the form and consistency of a theory.

Among those who held the doctrine, while immatured, were Ecphantus, Leucippus, and Democritus. Subsequently Epicurus introduced such modifications and improvements as were essential to its complete development.[†] The Latin poet Lucretius, in his "De Rerum Naturâ," has given a full exposition of the Epicurean philosophy; from this, as well as from the writings of Plutarch, it will be seen that the most prominent atomic tenets did not differ essentially from the opinions entertained by eminent scientists of modern times.[‡] Newton admitted the creation of primitive particles, extremely minute, but permanent. Descartes, on the other hand, held with Aristotle, Plato, and Pythagorae that the division of matter has no assignable limit. Leibnitz attempted to reconcile the conflicting opinions of metaphysicians and mathematicians, by supposing that matter, in its ultimate condition, consists of unextended points which he denominated monads, a term borrowed from Pythagoras. At a later day, Boscovich published his celebrated dynamic theory, in which centres of force are substituted for monads. Neither of these ingenious theories however, reaches the real points of perplexity.

It is obvious that the science which treats of the ultimate composition of bodies would lead to more correct conceptions regarding minute combinations of ponderable matter. Analysis has shown that nearly all the bodies formed in the great laboratory of nature are compounds. Thus far, sixty-three different kinds of matter have resisted every effort to resolve them into simpler constituents. These substances, distinguished as chemical elements, unite in exceedingly minute quantities according to the well-known laws of Stoichiometry. In the year 1789, Higgins, a professor in the University of Dublin, advanced the idea that certain compounds are formed by the combination of ultimate particles or atoms of different elements. Dalton, in 1803, independently arrived at a similar conclusion, which he generalised, to explain the composition of all compounds, and made it the basis of his "New System of Chemical Philosophy," published five years later. The doctrine of Dalton has undergone, since his day, such modifications as render it more acceptable; but that part of it which ascribes the union of indestructible atoms to chemical affinity may be regarded as the first successful attempt to explain that primordial action which the ancient atomists could not account for, and which the Latin poet above named describes as irregular and fortuitous.[*]

Chemists of the atomic school happily avoid the vexed question concerning the indivisibility of matter, by defining an atom as the smallest quantity of an element which can enter into the composition of a ponderable molecule; and the molecule, whether made up of one, two, or more elements, as the smallest quantity which can exist in a free state. However, a certain individuality must be assigned to the single atom, for a chemical decomposition requiring its transfer from one molecule to another involves its isolation *in transitu*. The absolute weight of the sixty-three different atoms cannot be ascertained; nevertheless, their relative weights have been determined with great care.

It is difficult to arrive at any clear notions concerning the size of an object so minute as to be for ever invisible under the most powerful magnifier. As an example of the conclusions regarding molecules, founded on microscopic scrutiny, that of the celebrated Ehrenberg may be cited.[†] Without attempting to make a close approximation towards its actual dimensions, his researches led him to infer that the diameter of an atom (the molecule of the chemist) was considerably less than six millionths of a line. Quite recently Sir W. Thomson, in a paper "On the Size of Atoms,"[‡] presented four lines of argument founded on experiments of physicists, which all lead to substantially the same estimate of the dimensions of molecular structure. He says:—

"Jointly they establish, with what we cannot but regard as a very high degree of probability, the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the hundred-millionth and greater than the two thousand-millionth of a centimetre. To form some conception of the degree of cross-grainedness indicated by this conclusion, imagine a rain-drop, or a globe of glass as large as a pea, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be coarser grained than a heap of small shot, but probably less coarse grained than a heap of cricket balls."

From these deductions of Thomson some idea may be formed of minute molecular grouping; and I venture the suggestion that, in regard to size, the smallest bullet would probably stand about mid-way between the *glomeramen minimum* and "the great globe itself."

Beyond this point of extreme tenuity, where matter first exhibits that property which is revealed in visible forms, we are forced to consider it in a still more expanded state, as the universally diffused medium of light, heat, and actinism; consequently this conception of the minute ponderable globule does not bring us very near the *minima naturæ*, for a difference in size cannot be less marked between ponderable atoms and those infinitesimal particles forming the luminiferous ether or æth which fills the interstellar spaces, and which, in a more condensed state, probably forms the interatomic medium. Assuming that all forces generating wave motions in elastic fluids follow the same law of propagation, I endeavoured some years ago to estimate the density of this inconceivably attenuated substance.[§]

In that calculation the density of air is the unit of measure. If instead hydrogen be taken as the unit, the density of luminiferous ether is expressed by the decimal ·00000000001653. Whatever may be its actual density, its reality must be admitted, until the positions established by the investigations of Huyghens,

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[*] Reprinted from the *American Chemist*, corrected by the author.

[†] Plutarch's "Morals." Edited by Prof. Goodwin, of Harvard University (Little, Brown, and Co, Boston, Mass) Vol. viii. pp, 111-112. Vol. v. p 345.

[‡] A full exposition of the ancient atomic philosophy would be foreign to the purpose of this paper Many of the prevailing erroneous impressions concerning it would, however, be corrected by an examination of the third chapter of Dr. Good's "Book of Nature," in which Epicurus is ably defended against the charge of atheism Evidently the Epicureans were opposed to Mythology; but while ignoring the power of its gods, they were naturally led to the recognition of a higher Power, an Intelligent Cause, Self-existent, and Supreme This deduction was reached by the earnest believers in the atomic doctrine. According to Stobæus, Ecphantus supposed the material world to consist of atoms, but yet to be ordered and governed by a Divine providence Ἔκφαντος ἐκ μὲν τῶν ἀτόμων συνεστάναι τὸν κοσμὸν, διοικεῖσθαι δὲ ἀπὸ προνοίας Eclog. Physic lib. 1 cap xxv. And as evidence of the belief prevalent among wise men several centuries later, Berzelius, in his paper on "Proportions, determinate," quotes from Philo, who, in his collection of the choicest philosophical ideas of his time ("Libri Sapientiæ," cap xi v 22), says:—Παντα Θεὸς μέτρῳ καὶ ἀρίθμῳ καὶ σταθμῷ διέταξε ("God made all things by measure, number, and weight'). This remarkable statement, as far as it relates to things terrestrial, modern chemical investigations have fully confirmed.

[*] Omnimodis coire, atque omnia pertentare,
Quæcunque inter se possint congressa creare.
Lucret. lib. v. *ver* 191.

[†] *Pogg Annalen.* xxiv, 35.

[‡] NATURE, No. 22, vol i, p. 551

[§] Sound would be propagated, with exactly the velocity of light through a fluid, under the standard pressure, 874,094,104,900 times rarer than air. Therefore, if the density of air be 1, the density of æth is represented by the decimal ·000,000,000,001,144+

It will not be inferred from this view that the aim has been to reach

"The first of things, quintessence pure,"

for the elastic quality of æth involves the hypothesis of a still more subtle fluid We have raised one curtain only to find another to be raised. As the unfathomed vaults of Heaven recede before the sweep of a more powerful refractor, and nebulæ resolved reveal nebulæ beyond, so the most diminutive germ that springs from the Creator's touch discloses *through the lens* of higher power new signs of more wonderful mechanism within Each nucleus has its nuclei! Each entoblast is but the boundary of a microcosm! Each particle a galaxy of atoms revolving in the all-pervading æth! Thus, before every far-reaching human advance, *Circumference* and *Centre* will for ever retreat—*Transactions of the American Institute*, 1864, p. 539. ("Clydonics," No. 1)

Young, Fresnel, Foucault, and Fizeau, are shown to be untenable. A very able American metaphysician, in meeting an objection brought by Huxley against the views of Comte, has strongly expressed his unqualified dissent;[*] nevertheless, the hypothesis that light, heat, and actinism are propagated by the undulations of a subtle all-pervading fluid, is the only one which satisfactorily accounts for a certain class of phenomena, and it is accepted by all the prominent experimental physicists of the present day.

The vast difference in density indicated cannot be apprehended, because numerical comparisons utterly fail to raise in the mind any clear conception regarding a fluid so attenuated; yet it naturally suggests the idea that there must be many intervening conditions of matter in which it exists in successive degrees of increasing density, and that these conditions form the connecting links, so to speak, between its apparently imponderable and its ponderable states. Something like this opinion seems to have been maintained in a curious work published in England many years ago.[†] The reverend author, viewing the universe as a systematic manifestation of the Divine Will, assumes that the medium of light is the mother element from which by progressive steps the chemical elements have been evolved. Proceeding from the first lines of morphology he arrives at the primitive form which cannot be isolated; then by an exceedingly ingenious synthetic process he represents by diagrams his ideal structure of different kinds of atoms, all of which are duplications of the tetrahedron. Thus he claims to reveal the unit, by multiples of which the atomic weight of all chemical elements may be expressed, and so arrives at a result which will be recognised as simply a modification of the so-called law of Prout. Although this, and other remarkable surmises by Macvicar are, for reasons which need not here be adduced, quite untenable, he seems to have led the way to an assumption which has recently met with some favour, namely, that the chemical atom, although indivisible, is a collection of smaller particles. However, in following this author towards the infinitesimal, we only realise more fully the truth that above and below the narrow zone of the visible are objects too far off and too fine for human scrutiny. Although the *seeming all* is rounded by intimation of other and brighter regions, Science can never compass them by any extension of her domain! In those unsounded depths which form the boundary and background of the known, thought grown dizzy finds no support; and even the positivist turns back bewildered when mensuration fails and computations end in surds!

On examining the numerous works on chemistry published within the last twenty years, one cannot fail to notice a gradual change in the expressions employed in describing reactions. The word "equivalent" seems to have lost the meaning originally assigned to it by Wollaston, and the terms "combining weight" and "combining proportion" are now used less frequently than "atomic weight" and "atom." This abandonment of old forms of expression doubtless indicates a gradual change of opinion among leading chemists, a change which may be ascribed partly to an accumulation of facts tending to confirm the atomic theory, and, partly, to the promptings of that mysterious intuition which, overleaping the limits of logic, often arrives at correct conclusions even before their truth has been demonstrated.

During all the discussions on "atomicity" hardly a doubt has been raised as to the actual existence of the atom. It was not, therefore, surprising that the chemical world received a sensible shock at the stand made by Brodie in 1868.[‡] However, a careful examination of his paper is likely to lead to the conclusion that the objections to the atomic theory therein enumerated are not more formidable than those which can be urged against his own ingenious, but complicated method of chemical operations. Precision in signs and definitions leads to exact results in the abstract, nevertheless a mathematical formula often requires modification to meet the varying conditions found in actual practice, and even then it only gives a near approximation to the truth.

Renewed attention to this subject was doubtless the means of drawing from the then President of the London Chemical Society a paper "on the Atomic Theory," which is generally regarded as the best exposition and defence of the doctrine yet made, and which may be consulted with profit by those desiring to obtain a clear statement of the principal results of chemical research adduced for its confirmation.[*]

A vigorous attack on the atomic theory has since been made by Mills, the real tendency of which is to raise doubts concerning the existence of matter itself.[†] He quotes with evident satisfaction from a work by Digby "on the nature of bodies" printed in 1645, wherein *quantity* is defined "as but one whole that may indeed be cut into so many several parts; but those parts are really not there till by division they are parcelled out; and then the whole (out of which they are made) ceaseth to be any longer, and the parts succeed in lieu of it, and are every one of them a new whole." From this statement proceeds a train of geometrical reasoning concerning extension and division which leads to the old dilemma regarding finite and infinite indivisibles.

Fortunately a new science, unknown to Digby, has demonstrated that matter has other than mere physical properties which are so clear and well defined as to enable its votaries to determine the ultimate composition of all bodies. The chemist affirms that, however inclined we may be to regard a body as a whole, it is in fact composed of minute parts which may be separated, and that in the great majority of bodies, which are compounds, Nature has herself made divisions by incorporating unlike parts which may be replaced by other unlike parts. On questions relating to the actual size of these parts, their form, their structure, &c., he makes no issue; he simply asserts that all these ultimate parts are permanent, and that those composed of the same kind of matter are identical in size and structure. The limits proposed for this paper will permit elucidation of this point alone.

The clearest conception of molecules and atoms will be arrived at by examining the principal phenomena attending the mechanical mixture and final chemical union of the lightest and the heaviest of the simple gases. The electro-positive element, hydrogen, is a permanently elastic gas, having a relative density expressed by 1. Its properties are in marked contrast with those of chlorine, a yellowish green gas, which may be condensed into a liquid, by a pressure of about four atmospheres. The density of this strong electro-negative element is 35·5. If two vessels of equal capacity, filled with these gases respectively, be placed in the dark, one over the other, and a communication be opened between them, a mutual diffusion of the gases will commence, the relative velocity being inversely as the square root of their densities. The action continues untraversed by the force of gravitation until minute portions of hydrogen and chlorine are equally diffused throughout both receptacles. This phenomenon cannot be accounted for, excepting on the supposition that minute parts of each gas have undergone complete isolation. If diffusion were effected only through a single stratum or extremely thin layer, it would be possible for two gaseous elements to retain their continuity by passing each other in intertwining streams, thus forming like threads, a warp and woof; but when diffusion is in every direction it is obvious that these elements must positively separate each other, and thus be divided into extremely diminutive bodies each of the same dimensions. Let *l* represent the lighter gas, *d* the denser, and *e* the dimensions or size of each isolated portion, then *el* and *ed* will denote the dissimilar parts of which the whole gaseous matter is composed. As the phenomenon of diffusion occurs under the conditions mentioned, whatever may be the quantity of gases employed, it follows that *el* and *ed* are individual volumes or molecules, invariably of the same dimensions. This diffusion of gases may therefore be defined as the uniform intermingling of dissimilar molecules.

If the molecules *el* and *ed* thus commingled while in the dark be exposed to direct sunlight, an instantaneous and complete chemical combination occurs with explosive violence but without condensation; or if exposed to diffused daylight, the union of elements will be gradual and without explosion; the resulting compound in each case being hydrochloric acid gas.

The affinity or force of chemism is generated by the action of light on the coloured gas chlorine, which, by absorbing all the rays and transmitting only the yellowish green, acquires a power which seems to be expended by the union of that element with hydrogen. Early in the present century M. Benard announced that the new properties acquired by chlorine on exposure to light were derived from the violet ray. In 1843 Draper proved by experiment the relative power of each ray in producing this change, the actinic rays being altogether the most effective.[*] Mr. E. Budde has recently described a remarkable experiment in this direction. He found that a differential thermometer filled with chlorine expanded about seven times more, in the violet than in the red ray of the solar spectrum; when the same thermometer was filled with CO_2 no action was noticed.[†]

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[*] Eleventh Harvard lecture, by Prof John Fiske. Cambridge, Mass., 1869.

[†] "Elements of the Economy of Nature." By J. G. Macvicar, D D. (London: Chapman and Hall.) 1856.

[‡] "The Calculus of Chemical Operations." By Prof. B. C. Brodie. *Journal of the Chemical Society*, London, vol. xxi. p. 367

[*] "On the Atomic Theory." By Prof. A. W. Williamson. *Jour. Chem. Soc.* London, vol. xxii. p. 328

[†] "On the Atomic Theory." By Edmund J. Mills, D.Sc. *Philosophical Magazine*, vol. xlii No. 278, p. 112.

As the combination of hydrogen and chlorine is effected without change of volume, it is obvious that the molecule *el* does not unite with the molecule *ed*, forming a compound molecule *el-ed*. The conclusion is therefore unavoidable that each molecule has been divided into two equal parts, and that by affinity, like parts have been separated, and unlike parts have been united. These parts are the smallest quantities that can be isolated, and are in fact the atoms recognised by the chemist. If this smallest combining proportion or atom be designated by *a*, the actual composition of the hydrogen molecule *el*, weighing 2, may be clearly represented by *al-al* (weight 1 + 1), and the chlorine molecule *ed* weighing 72, by *ad-ad* (weight 35·5 + 35·5). As the attraction of *al* to *al*, and of *ad* to *ad* is, after exposure to light, less than of *al* to *ad*, there is an instantaneous chemical change by which one molecule of hydrogen and one molecule of chlorine are transformed into two molecules of hydrochloric acid gas. This reaction is clearly indicated by the following equation: *al-al* + *ad-ad* = *alad* + *alad* = 2 *alad*.

The symbols here used are intended to convey to the mind an idea of the relative size of combining parts, which is not so apparent when expressed as follows:—

$$H_2 + Cl_2 = HCl + HCl = 2\,HCl.$$

From the simplest of molecular types we might proceed to the most complex; and, throughout, if we consider the combining proportion of each simple constituent as either a unit or a multiple of a unit, the composition of each molecule may be expressed by whole numbers. Thus having as many different kinds of units as there are elements, any true chemical combination may be symbolised by a combination of arithmetical ratios. This method, under the light of the atomic theory, clearly reveals the harmonic relations of molecular constituents, which, seen from the stand-point of percentage composition, appear unconnected and discordant.

It must be admitted that many of the reactions of well-known bodies have not yet been determined quantitatively; yet were they made out, we should not be able to demonstrate by experiment the truth of the atomic doctrine. It still remains a theory, in favour of which there are many facts and phenomena that collectively form an argument not easily to be outweighed. This evidence may be briefly summarised as follows:—

1. *Atomic Weights.* Elements combine in extremely minute parts, according to the law of definite and multiple proportions. The atomic weight of an element is either its equivalent weight or a multiple of it, as such multiple cannot be divided by reactions, its weight must conform with the atomic number. Whatever changes of position the combining weight of an element may undergo in a series of molecular metamorphoses, that is to say, however many times it may be displaced and replaced in chemical combinations, it invariably retains its characteristic weight. This invariability of weight is an essential property of the atom.

2. *Atomic Volume.* Gases unite in equal volumes or multiple volumes. If hydrogen be taken as unity, the density of each elementary gas is identical with the weight of its atom. The atomic volume, determined by dividing the atomic weight of a body by its specific gravity, has been the means of revealing many interesting relations among compounds of similar structure, and among many containing different components and of unlike structure.

3. *Atomic Heat.* It has been shown by experiment that quantities of each element conforming with its atomic number have the same capacity for heat, excepting only carbon, boron, and silicon; these, it is believed, will yet be found to conform to the law, that the specific heats of atoms are the same. This law is regarded as a direct confirmation of atomic weights.

4. *Molecules.* According to the atomic theory chemical forces are brought in equilibrium when atoms combine and form a molecule. Every gas and every vapour undecomposed has a density proportional to its molecular weight. All known molecular combinations and combining proportions are in accordance with the atomic doctrine. Decomposition by electrolysis affords some evidence that the constituent parts of a molecule which are simultaneously separated are proportionate to atomic weights.

5. *Atomic combining capacity.* The modern doctrine of types and substitutions is solely based on the individuality of the atom, without which the whole fabric of typical structures must fall.

6. *Isomerism.* The fact that bodies containing the same elements, and in precisely the same proportions, exhibit different properties, has been thus far accounted for, only on the supposition that atoms are differently arranged in each body. These differences in arrangement depend not only on the relative position of atoms, but also on the order as to time in which they combine; for two or more atoms having such precedence over others as to combine first, may, by that means, form a radical of such permanence as to play the part of an atom. Apart from the question of radicals, we may ascertain the number of different bodies which can be formed from the same number of different atoms, by an application of the mathematical law of permutations.

7. *Homogeneity.* The uniformity of structure and appearance of any element or chemical combination of elements furnishes the most palpable proof of the identity in size and shape of those definite parts which we designate as molecules. This homogeneity is retained under different degrees of pressure, thus making it apparent that molecules are not identical in structure, but that they approach and recede in precisely the same manner under the same conditions.

Finally.—The foregoing statement regarding the existence of atoms which are indivisible and indestructible under the present order of things does not preclude the supposition that the atom may be a cluster of smaller particles held together by a powerful affinity, which, when counteracted, would leave them free to move within a given sphere. On this assumption it is highly probable that the relative position of such particles may modify the combining capacity of the atom. Moreover, the normal motion of such particles may determine not only the peculiarities of elemental spectra, but produce other effects not dependent on the amplitude of atomic oscillations, thus favouring the inference that the atom itself is a receptacle of force.

SAMUEL D. TILLMAN

BLOOD-RELATIONSHIP [*]

I PROPOSE in this memoir to deduce by fair reasoning from acknowledged facts a more definite notion than now exists of the meaning of the word "kinship." It is my aim to analyse and describe the complicated connection that binds an individual, hereditarily, to his parents and to his brothers and sisters, and, therefore, by an extension of similar links, to his more distant kinsfolk. I hope by these means to set forth the doctrines of heredity in a more orderly and explicit manner than is otherwise practicable.

From the well-known circumstance that an individual may transmit to his descendants ancestral qualities which he does not himself possess, we are assured that they could not have been altogether destroyed in him, but must have maintained their existence in a latent form. Therefore each individual may properly be conceived as consisting of two parts, one of which is latent and only known to us by its effects on his posterity, while the other is patent and constitutes the person manifest to our senses.

The adjacent, and, in a broad sense, separate lines of growth in which the patent and latent elements are situated, diverge from a common group and converge to a common contribution, because they were both evolved out of elements contained in a structureless ovum, and they jointly contribute the elements which form the structureless ova of their offspring.

The annexed diagram illustrates my meaning, and serves to show clearly that the span of each of the links in the general chain of heredity extends from one structureless stage to another, and not from person to person.

$$\begin{array}{c}\text{Structureless}\\ \text{elements in}\\ \text{Father}\end{array}\left\{\begin{array}{l}\text{......Adult Father ......}\\ \\ \text{......Latent in Father......}\end{array}\right\}\begin{array}{c}\text{Structureless}\\ \text{elements in}\\ \text{offspring.}\end{array}$$

I will now proceed to consider the quality of the several relationships by which the above terms are connected together.

The observed facts of Reversion enable us to prove that the latent elements must be greatly more varied than those that are personal or patent. The arguments are as follows:—(1) There must be room for very great variety, because a single strain of impure blood will reassert itself after more than eight generations; (2) an individual has 256 progenitors in the eighth degree, if there have been no ancestral intermarriages, while under the ordinary conditions of social and neighbourly life, he will certainly have had a considerable, though a smaller, number of them; (3) the gradual waning of the tendency to reversion as the generations increase, conforms to what would occur if each fresh marriage contributed a competing element for the same place, thus diluting the impure strain until its relative importance was reduced to an insignificant amount. It follows from these arguments that for each place among the personal elements there may exist, and probably often does exist, a great variety of latent elements that formerly competed to fill it.

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[*] A Treatise on the Forces which produce the Organisation of Plants. By John William Draper. (New York: Harper and Brothers, 1843.)

[†] *Pogg. Annalen* for 1871, No. 10.

[*] Read before the Royal Society, June 13, by Francis Galton, F.R.S.

I have spoken of the primary elements as they exist in the newly-impregnated ovum, where they are structureless, but contain the materials out of which structure is evolved. The embryonic elements are segregated from among them. On what principle are they segregated? Clearly it is on some principle whose effects are those of "Class Representation," using that phrase in a perfectly general sense, as indicating a mere fact, and avoiding any hypothesis or affirmation on points of detail, about most, if not all, of which we are profoundly ignorant. I give as broad a meaning to the expression as a politician would give to the kindred one, a "representative assembly." By this he means to say that the assembly consists of representatives from various constituencies, which is a distinct piece of information so far as it goes, and is a useful one, although it deals with no matter of detail; it says nothing about the number of electors, their qualifications, or the motives by which they are influenced; it gives no information as to the number of seats; it does not tell us how many candidates there are usually for each seat, nor whether the same person is eligible for, or may represent at the same time, more than one place, nor whether the result of the elections at one place may or may not influence those at another (on the principle of correlation). After these explanations there can, I trust, be no difficulty in accepting my definition of the general character of the relation between the embryonic and the structureless elements, that the former are the result of election from the latter on some method of Class Representation.

The embryonic elements are *developed* into the adult person. "Development" is a word whose meaning is quite as distinct in respect to form, and as vague in respect to detail, as the phrase we have just been considering; it embraces the combined effects of growth and multiplication, as well as those of modification in quality and proportion, under both internal and external influences. If we were able to obtain an approximate knowledge of the original elements, statistical experiences would no doubt enable us to predict the average value of the form into which they would become developed, just as a knowledge of the seeds that were sown would enable us to predict in a general way the appearance of the garden when the plants had grown up. But the individual variation in each case would be great, owing to the large number of variable influences concerned in the process of development.

The latent elements in the embryonic stage must be developed by a parallel, I do not say by an identical process, into those of the adult stage. Therefore, to avoid all chance of being misapprehended when I collate them, I will call, in the diagram I am about to give, the one process "Development (*a*)" and the other "Development (*b*)."

It is not intended to affirm, in making these subdivisions, that the embryonic and adult stages are distinctly separated; they are continuous, and it is impossible but that they should overlap, some elements remaining embryonic while others are completely formed. Nevertheless the embryo, speaking broadly, may fairly be looked upon as consecutive.

Again, the two processes are not wholly distinct; on the contrary, the embryo, and even the adult in some degree, must receive supplementary contributions derived from their contemporary latent elements, because ancestral qualities indicated in early life frequently disappear and yield place to others. The reverse process is doubtful; it may exist in the embryonic stage, but it certainly does not exist in a sensible degree in the adult stage, else the later children of a union would resemble their parents more nearly than the earlier ones.

Lastly, I must guard myself against the objection, that though structure is largely correlated, I have treated it too much as consisting of separate elements. To this I answer, first, that in describing how the embryonic were derived from the structureless elements, I expressly left room for a small degree of correlation; secondly, that in the development of the adult elements of the embryonic, there is a perfectly open field for natural selection, which is the agency by which correlation is mainly established; and thirdly, that correlation affects groups of elements, and not the complete person, as is proved by the frequent occurrence of small groups of persistent peculiarities, which do not affect the rest of the organism, so far as we know, in any way whatever.

The ground we have already gained may be described as follows:—

Out of the structureless ovum the embryonic elements are taken by Class Representation, and these are developed (*a*) into the visible adult individual. On the other hand, returning to our starting-point at the structureless ovum, we find, after the embryonic elements have been segregated, the large residue is developed (*b*) into the latent elements contained in the adult individual. All this is summarily expressed in the first two columns of the diagrams below. I might have inserted vertical arrows to show the minor connections between the corresponding stages in the two parallel processes, but it would have complicated the figure.

In what way do the patent and latent adult elements respectively contribute representatives towards the structureless stage of the next generation? We know that every quality they possess may be transmitted to it, but it does not follow that they are invariably transmitted. The contributions from the patent elements cannot be by "Class," because their own original elements have been themselves specialised, and therefore can contain no more than one or a few members of each class (which, it is true, must have been somewhat developed, both in numbers and variety). Their contributions may therefore be justly described as being effected on some principle that has resulted in a "Family representation," though whether in a strictly universal representation I do not profess to say.

As regards the large variety of adult latent elements, they cannot all be transmitted, for the following obvious reason; the corresponding qualities of no two parents can be considered exactly alike; therefore the accumulation of sub-varieties, if they were all preserved, as the generations rolled onwards, would exceed in multitude the wildest flights of rational theory. The heritage of peculiarities through the contributions of 1,000 consecutive generations, even supposing a great deal of ancestral intermarriage, must far exceed what could be packed into a single ovum. The contributions from the latent adult elements are therefore no more than representative; but we know they cannot be so on the broad principle of "class representation," if the word "class" be applied to the same large orders as before, and if the representatives are few in number, because it is incumbent on them to furnish all the various members of each Class whence the representatives have to be drawn. Therefore, bearing in mind what has been just argued, that it is impossible for the elements of every individual quality to be contributed, we are driven to suppose, as in the previous case, a "Family Representation," the similar elements contributed by the two parents ranking, of course, as of the same family. It is most important to bear in mind that this phrase states a fact and not an hypothesis; it does not mean that each and every Family has just one representative, for it is absolutely reticent on all matters of detail, such as those I enumerated, when speaking of Class Representation. To show the importance which I attach to this disclaimer, I may be permitted to mention what appears to me the most probable *modus operandi*, namely, that it is in reality a large selection made on a broader and not a narrower system than that of classes, and similar to that obtained by an indiscriminate conscription; thus, if a large army be drawn from the provinces of a country by a general conscription, its constitution, according to the laws of chance, will reflect with surprising precision the qualities of the population whence it was taken; each village will be found to furnish a contingent, and the composition of the army will be sensibly the same as if it had been due to a system of immediate representation from the several villages.

The following diagram expresses the whole of the foregoing results:—It begins with the structureless elements, whence the parent individual was formed, and ends with its contributions to the structureless elements, whence his offspring is formed.

I will now inquire, what are, roughly speaking, the relative proportions of the contributions to the elements of the offspring made respectively by the patent and latent elements of the adult parent? It is better not to complicate the inquiry by speaking, at first, of these elements in their entirety, but rather of some special characteristic; thus, to fix the ideas, suppose we are speaking about a peculiar skin-mark in an animal. The peculiarity in question may be conceived (1) as purely personal, without the concurrence of any latent equivalents, (2) as personal but conjoined with latent equivalents, and (3) as existent wholly in a latent form. It can be shown that, in the first case, the power of hereditary transmission is exceedingly feeble, for notwithstanding some exceptions (as in the lost power of flight in domestic birds), the effects of the use and disuse of limbs, and those of habit, are transmitted to posterity in only a very slight degree. Again, it can be fairly argued that many classes of cases which seem at first sight to fall under case (1), that is, to be purely personal, and to prove a larger hereditary influence than what I assign to it, do really belong to case (2). Thus, when individuals born with a peculiar mark are reputed to be the first of their race in whom it had ever appeared, it would be hazardous in the extreme to argue that the latent elements of that mark were wholly deficient in them. It is very remarkable (I was indebted for a knowledge of this fact to Mr. Tegetmeier) how nearly every bar or spot found in any species of an animal in its wild state may be bred into existence in the domesticated variety of that species; showing that the elements of all these bars and spots are universally present in all varieties of the species, though their manifestation may be overborne and suppressed. We therefore see that the hereditary influences of an animal with respect to any particular spot are, I will not say in every case, but certainly on the average of many cases, much more numerous than if that spot had been purely a personal characteristic, without the concurrence of any latent elements. Bearing this argument in mind, we shall more justly estimate the import of the statistical evidence to be obtained from breeders of animals. I should judge from the impression left by many scattered statistics that it is perfectly safe to affirm that breeders, when they mate two animals, each having the same unusual characteristic, not through known hereditary transmission, but by supposed variation, would consider themselves fortunate if one quarter of the progeny inherited that quality. Now these successful cases are, as I have shown, on the average, the produce of parents having the peculiarity not only in a personal, but also, to some degree, in a latent form. We may therefore reasonably conclude that, had the latter portion been non-existent, the ratio of successful cases would have been materially diminished.

I should demur on precisely the same grounds to objections based on the fact of the transmission of qualities to grandchildren being more frequent through children who possess those qualities than through children who do not; for I maintain that the personal manifestation is on the average, though it need not be so in every case, a certain proof of the existence of some latent elements.

Having proved how small is the power of hereditary transmission of the personal elements, we can easily show how large is the transmission of the purely latent elements, in the case (3) by appealing to the well-known facts of reversion; but into these it is hardly necessary for me to enter at length. The general and safe conclusion is that the contribution from the patent elements is very much less than from the latent ones.

If we now combine our results into a single diagram, showing the fainter stream of heredity by *italic lines*, and indicating those processes by asterisks (*) which were described at length in the previous figure, we shall easily recognise the complexity of hereditary problems. We see that parents are very indirectly and only partially related to their own children, and that there are two lines of connection between them, the one of large and the other of small relative importance. The former is a collateral kinship and very distant, the parent being descended through two stages (two asterisks) from a structureless source, and the child (so far as that parent is concerned) through five totally distinct stages from the same source. The other, but unimportant line of connection, is direct, and connects the child with the parent through two stages. We shall therefore wonder that, notwithstanding the fact of an average resemblance between parent and child, the amount of individual variation should not be much greater than it is, until we have realised how complete must be the harmony between every variety and its environments, in order that the variety should be permanent.

Structureless elements of parent . .	through "Class-Representation" afford	Embryonic elements . . .	which by "Development" (*a*) become	Adult elements	which by "Family Representation" contribute to .	Structureless elements of offspring.
	"Residue" after the abstracted elements are	Latent elements in embryo	which by "Development" (*b*) become	Latent elements in adult . .	which by "Family Representation" contribute to .	

Structureless elements of father	* *Embryonic*	* *Adult FATHER* *	Structureless elements of offspring . . .	* Embryonic	* Adult OFFSPRING.
	* Latent in embryo	* Latent in adult father *		. . .	. . .
Structureless elements of mother	* *Embryonic*	* *Adult MOTHER* *			
	* Latent in embryo	* Latent in adult mother *			

We also infer from the diagram how near, and yet how subject to variation, is the kinship between the children of the same parents; for only two stages are required to trace back their descent to a common origin, which, however, proceeds from four separate streams of heredity, namely, the adult patent and latent elements of each of the two parents.

An approximate notion of the nearest conceivable relationship between a parent and his child may be gained by supposing an urn containing a great number of balls, marked in various ways, and a handful to be drawn out of them at random as a sample. This sample would represent the person of a parent. Let us now suppose the sample to be examined, and a few handsful of new balls to be marked according to the patterns of those found in the sample, and to be thrown along with them back into the urn. Now let the contents of another urn, representing the influences of the other parent, to be mixed with those of the first. Lastly, suppose a second sample to be drawn out of the combined contents of the two urns, to represent the offspring. There can be no nearer connection justly conceived to subsist between the parent and child than between the two samples; on the contrary, my diagram shows the relationship to be in reality much more remote, and consisting of many consecutive stages, and therefore hardly to be expressed by such simple chances. Whenever the balls in the urns are much of the same pattern, the samples will be alike, but not otherwise. The offspring of a mongrel stock necessarily deviate in appearance from each other and from their parents.

We cannot now fail to be impressed with the fallacy of reckoning inheritance in the usual way, from parents to offspring, using those words in their popular sense of visible personalities. The span of the true hereditary link connects, as I have already insisted upon, not the parent with the offspring, but the primary elements of the two, such as they existed in the newly impregnated ova whence they were respectively developed. No valid excuse can be offered for not attending to this fact, on the ground of our ignorance of the variety and proportionate values of the primary elements. We do not mend matters in the least, but we gratuitously add confusion to our ignorance, by dealing with hereditary facts on the plan of ordinary pedigrees—namely, from the persons of the parents to those of their offspring.

It will be observed that, owing to the clearer idea we have now obtained of the meaning of kinship and of the consecutive phases of the chain of life, the various causes of individual variation can be easily and surely sorted into their proper places. I will mention a few of them, merely as examples.

In the segregation of the embryonic elements, if the structureless ones be diverse without any strongly preponderating element, it is impossible to foresee the character of the embryo, just as it is impossible to foresee the character of a handful chosen from an urn containing a mixed assemblage of variously coloured balls. But if they be not diverse, then the embryonic elements will be a true sample of the structureless ones, the conditions of purity of blood are fulfilled, and the offspring will resemble its parents.

We also see, in the process by which the embryonic elements are obtained, how the curious phenomenon may occur of inheritance occasionally skipping alternate generations. The more that has been removed from the structureless group for the supply of the embryonic (which as we have seen, in a nearly sterile destination) the less remains for the latent group, too little, it may be, to assert itself by that, the only prolific, line of transmission. In the supposed case it would recuperate itself during the succeeding generation, where the elements in question will have remained wholly latent, owing to their insignificance in the structureless stage of that generation, which would be sufficient to secure any portion of it from selection for the embryonic form.

It is in the stage of development where I presume those influences to come in, which cause domesticated animals, when turned loose, to become feral. No variety can be stable unless the conditions of development concur to maintain the structureless stages of consecutive generations in an unchanged form. It is clearly of no avail to a breeder to obtain a stock by continued and careful selection, that shall conform to a desired type, if the animals be afterwards reared under other conditions, by which the subsequent stages, both latent and patent, shall be modified.

Lastly, it is in the process of selection of elements, both latent and patent, from the adult parents for the structureless stage of the next generation, where I suppose the curious and unknown conditions usually to occur, through which a change in the habits of life, after the adult age has been reached, is apt to produce sterility. I may be permitted to remark, hypothetically, that this view appears to be corroborated by the fact, that many grains of pollen or many spermatozoa are required to fertilise each ovum, because, as it would seem, each separate one does not contain a sufficiently complete representation of the primary elements to supply the needs of an individual life, and that it is only by the accumulation of several separate consignments (so to speak) of the representative elements, that the necessary variety is ensured. I argue from this that there is a tendency to a large individual variation in the constituents of each grain of pollen, or spermatozoon, and, by analogy, that there is a similar though smaller tendency in each ovum. Also, that changes in the habits of life may increase this variation to a degree that involves sterility.

One result of this investigation is to show very clearly that large variation in individuals from their parents is not incompatible with the strict doctrine of heredity, but is a consequence of it wherever the breed is impure. I am desirous of applying these considerations to the intellectual and moral gifts of the human race, which is more mongrelised than that of any other domesticated animal. It has been thought by some that the fact of children frequently showing marked individual variation in ability from that of their parents, is a proof that intellectual and moral gifts are not strictly transmitted by inheritance. My arguments lead to exactly the opposite result. I show that their great individual variation is a necessity under present conditions, and I maintain that results derived from large averages are all that can be required, and all we could expect to obtain, to prove that intellectual and moral gifts are as strictly matters of inheritance as any purely physical qualities.

SOCIETIES AND ACADEMIES

London

Chemical Society, June 20.—Dr. Frankland, F.R.S., president, in the chair. The president announced that Mr. Hyde Hills had given ten guineas to the fund for promoting original research, and promised to further increase the donation by ten guineas for each ninety subscribed for the same purpose.—Mr. H. Deacon, on "Deacon's Method of Obtaining Chlorine, as Illustrating some Principles of Chemical Dynamics." The process consists in passing a heated mixture of air and hydrochloric acid over sulphate of copper, or over pieces of pumice or brick saturated with the same. He finds that the action is essentially a surface action, and that there is a certain comparatively small range of temperature, between the critical limits of which the percentage of hydrochloric acid decomposed varies greatly. The velocity with which the mixed gases pass over the surface of the active material also causes considerable variation in the comparative amount of chlorine produced.

BOOKS RECEIVED

English.—As Regards Protoplasm, new edition: J. H. Stirling (Longmans).

American.—The Periodic Law: Rev. G. A. Leakin.

Foreign.—Rendiconto dell'Accademia delle Scienze fisiche et matematiche, Naples, 1862-1869 (through Williams and Norgate)—Compendium der physiologischen Optik fur Mediciner u Physiker: Dr. H. Kaiser.

PAMPHLETS RECEIVED

English.—How Fishes Breathe: J. C Galton—Influence of Colloids on Inorganic matter: W. Ord.—The Edinburgh Sixpenny 4to. Atlas. W. and A. K. Johnston—The Insulation of St Michael's Mount: W. Pengelly.—The Sidereal and Solar Systems: C. C Clarke.—The Influence of Human Progress on Medical Education: W Aitken—Influence of Vaccination, &c., on Mortality from Small-Pox: R. Grieve, M.D.—London Students' Gazette, May—Annual Address to the Linnean Society: G. Bentham.—Transactions of the Norfolk and Norwich Naturalists' Society, 1872.—Meetings of the Newcastle-on-Tyne Chemical Society, 1871-2—Journal of the Iron and Steel Institute, vol. i No 2.—Report of the Astronomer Royal to the Board of Visitors—Quarterly Journal of the Meteorologieal Society, vol. i. No. 2—Journal of Anatomy and Physiology, No. 10.—Proceedings of the Geologists' Association, vol, ii No. 5.—Tenth Annual Report of the Birmingham Free Libraries Committee, 1871.—Report of Wigan Field Naturalists' Society, 1870-72—Quarterly Weather Report of the Meteorological Office, Oct. to Dec., 1870.—Devon and Exeter Albert Memorial Museum School of Science and Art; Report for 1872.—Transactions of the Institute of Engineers in Scotland.—Report of Bury Natural History Society, 1872.—On Phonic Coast Fog-Signals: A Beazeley—Examination of the recent Attack upon the Atomic Theory: R. W. Atkinson.—The Mining Review, vol. i. No 8.

American and Colonial.—The American Practitioner, May 1872—Reports of the Mining Surveyors and Registrars, Victoria.—Report on the Operations of the Trigonometrical Survey of India, 1870-71: Major Montgomerie.—Second Annual Report on the injurious and beneficial Insects of Massachusetts: A. S Packard—Historical Sketch of the *Public Ledger* of Philadelphia: E. H. Munday.—Monthly Record of Observations in Meteorology and Terrestrial Magnetism: R. J Ellery—The Projected Science Association for the Natives of India, Mahendra la'l Sircár, M.D—Report of Progress of Commission of Foreign Forests, Victoria, 1871.—Report of the Entomological Society for Ontario for 1871.—The School Laboratory, vol. ii, No 1.—The Sun and the Phenomena of the Atmosphere: Prof. C A Young.—Fourth Annual Report on the noxious and beneficial Insects of Missouri: C. V. Riley.

Foreign.—Atti della reale Accademia dei Lincei, 1871—Forme delle Protuberanze regioni del magnesio e del ferro sulla superficie del Sole. P. Tacchini.—Memorie della Società degli spettroscopisti Italiana, No 4.—Bulletins de la Société d' Anthropologie, Aug. et Sept. 1871.—Indice degli autori e delle materie della gazetta chimica Italiana, vol. i.—Contribution à une histoire générale et Encyclopédique des Sciences: T. Wechinakof.—La Belgique horticole, Mai et Juin.—Osservazione dell' Eclisse totale: Prof. L. Respighi.—Sull' ultima Eclisse del 12 Dec, 1871: L Respighi.—The Quarterly German Magazine, No 1, for 1872.

DIARY

THURSDAY, June 27.

Society of Antiquaries, at 8.30—Origin of the word Coach: A. Goldsmid.—On the Ruins of Torre Abbey. Miscellaneous Antiquities. Sir W. Tite.

FRIDAY, June 28.

Quekett Microscopical Club, at 8.

MONDAY, July 1.

Entomological Society, at 7.

TUESDAY, July 2.

Society of Biblical Archæology, at 8.30—On Israel in Egypt: Dr H. Haigh—On the Mazzaroth of Job XXXVIII: Henry Fox Talbot, F R.S.—On the Use of the Papyrus among the Accadians: Rev. A. H. Sayce—On the Economic Botany of the Bible: James Collins.

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THURSDAY, JULY 4, 1872

SENSATION AND SCIENCE

II.

THERE lies before us, as we write, a work of exceptionally high merit as a mere literary composition, entitled, *Ueber die Natur der Cometen, Beitrage zur Geschichte und Theorie der Erkenntniss.* Von J. C. F. Zöllner, Professor an der Universitat, Leipzig. The title does all it can to indicate the sensational character of the work, which deals not alone with the nature of comets, the inferiority of British to German physicists, and the grave offence of which a German is guilty when he sees anything to admire except at home; but also with the errors of Thomas Buckle, the relations of Science to Labour and Manufacture, and the analogies of development in Languages and in Religions!

It is impossible for us in a brief article to give the reader even a general glance at the numberless sources of amusement which the work affords. We will, therefore, confine our detailed remarks to a few of the parts which have most interested us, merely premising that we cannot pretend to give anything resembling a complete analysis of the contents of this astounding volume.

The skeleton, or framework, upon which the fabric is supported, consists of some scientific papers by Zollner, extracted from the Transactions of the Royal Society of Leipzig. Along with these are reprints of the descriptions given by Olbers and Bessel of the Comet of 1811 and Halley's Comet, with the speculations of these great astronomers as to the nature of the forces under which the various parts of a comet move, especially with regard to the supposed repulsive force exerted by the sun on the matter of the tails of all comets but the smallest, and by the heads of the large comets upon their tails.

We must not pause to criticise these speculations, else we should be treating of Science, extremely hypothetical no doubt, but still Science; whereas we have undertaken to give the reader some information about the progress of the *Sensational* in Science. But there is one remark which must be made. Prof. Zollner is loud in his denunciations of Sir J. Herschel's remarks on comet's tails, especially where he says in his "Outlines of Astronomy," "There is beyond question some profound secret and mystery of nature concerned." On this Zöllner remarks, "Could Sir J. Herschel have foreseen what mischief his mystical comparisons about comets were to do to the brains of his countrymen he would certainly have refrained from employing them." But with marvellous consistency he shortly afterwards quotes with approval the following passage from Olbers, as far as we can see quite as strong as anything of Herschel's:—"The particles which we see glittering in the comet's tail are thus not always the same. No; incessantly from its nucleus and from its proper atmosphere new materials develop themselves, and stream off from the comet with astounding (*erstaunenswürdig*) velocity to be lost in the vast expanse of heaven."

We must confess that the perusal of the *scientific* part of Prof. Zöllner's work has not impressed us with the highest respect for his originality or even his exactitude. "Who drives fat oxen should himself be fat;" so that when we found Sir W. Thomson, Helmholtz, and Hofmann figuring everywhere among his playthings, we eagerly looked to his original paper, the kernel of the volume, for something of the very highest order of mathematical and experimental inquiry. But, alas, in vain! We found some ordinary "second-year" mathematics (not always correct) and *one* sole experimental illustration, of a ludicrously obvious case of Newton's Third Law of Motion! Any mathematical student should surely know, that it does not require profound knowledge to calculate the distribution of gas or vapour about a spherical body if the law of attraction and that connecting pressure and density be assigned. But we must again recollect that our business at present is not with science, even the feeblest, unless accompanied by sensation.

Our difficulty now lies in selecting. Hundreds of racy passages must be omitted, else we should exceed all endurable limits. For, after the nucleus of the work, comes a long and ferocious attack upon *John* (*sic*) *Tyndall's Cometen-Theorie*, which may in the natural course of events draw down on poor Zollner a castigation he will never live to forget. In the words of Burger, who imagined his prototype in a much less perilous position—

> O Zollner! O Zöllner! Entfleuch geschwind.

It is drawn up in heads like a catechism, and is at least as metaphysical; but we have almost enough of it in the first of these heads, which we do our best to translate as "The fountain head of scientific knowledge, and its practical signification." A perfect volley of *Continuitat, Schamgefühl, Individuum, Causalität, Theorie der Erscheinung, Lust und Unlust*, &c., all in italics, fills many subsequent pages. We have not read them, save so far as to see (by a cursory examination) that Prof. Zollner employs language no doubt intended to be bitterly sarcastic, but which is so savage as to defeat its purpose, and which would be utterly unjustifiable, even if his half-made, half-implied, accusations had any foundation in fact. Section 13 has the appalling title, "Allgemeine Ursachen abnormer Erscheinungen begrundet im Zeitgeiste. Verhaltniss der Wissenschaft zur Technik und Industrie." Like a true Mephistopheles the author next confesses that he is tired of this dry tone, and must bring his victim (the reader, poor wretch) into merry company, and show him how jollily one can live. Whereupon he rises into the third heaven of vapulatory eloquence about the *Hofmann-Feier zu Berlin.* This seems to have been a sort of scientific high-jinks, perfectly harmless from every point of view. But Prof. Zollner is a realisation in the flesh of Sydney Smith's ridiculously inapt description of a Scotsman, he cannot see a joke—perhaps even the surgical operation would be thrown away upon him.

We next come to a mighty chapter, headed "Aphorismen zur Geschichte und Theorie der Erkenntniss." Here we get back again to the "trocknen Ton" and deal with *Causalität, Causalverhältniss, Lust und Unlust*, &c., more bewilderingly than before; and we find that it appears to the gifted author that "the Phenomenon of Sensation is a more fundamental fact of observation than the

Mobility of Matter." Here we confess we felt nonplussed, and as the rest of the work (pp. 321-523) seems, on a hasty glance, to be of the same sort, varied only by occasional extravagant eulogy of some philosophers and denunciation of others, we leave it unnoticed, except as regards one particular which will afterwards be referred to.

We now come to the richest part of the volume, the preface and introduction, written (as we are told) later than the rest, and therefore when the author had managed thoroughly to divest himself of all the usual amenities, as well as of regard for at least the scientific character of certain living philosophers.

From the introduction we paraphrase as follows (the passage follows some fierce remarks about Dr. Tyndall):—

> "I can assert that, when I read the addresses of Sir W. Thomson and Prof. Tait to the British Association, and when on my return to Leipzig I found on my table among the scientific novelties the German edition of their 'Natural Philosophy,' edited by Helmholtz and Wertheim (including particularly section 385), then, indeed, the appearance of my work seemed to be a *Naturprocess*; something necessary in the chain of scientific development, of which even I myself scarce knew how it had arisen, and what was my share in it. In fact, the desire to bring to light in this book what is more or less struggling to appear in German science, what is bringing out a hollow sound now from one string, anon from another; this desire, I say, has been with me to the latest scratch of my pen. I therefore doubt not that, simultaneously with mine, other heads have been working at the same problem, and perhaps in unconscious coincidence have arrived at the same solution. May then such facts ever more forcibly impress us with the conviction that the claims of personal services belong much more to the Age and to the Race than to the individual, and that no ever so clear conscious selection of means can be compared with that wonderful harmony with which Nature seeks to farther, and at the same time more surely to reach, her to us unknown ends."

To understand the bearing of the above passage, which is simply a literal assertion of

Deutschland, Deutschland über Alles,
Ueber Alles in der Welt,

the reader must refer to the preface, where he will find (along with much metaphysics) a war-dance over the mangled scientific reputation of Sir W. Thomson. The celebrated "moss-grown fragments from the ruins of another world", was a joke taken in earnest by many even in this country; so we can hardly blame Prof. Zöllner for falling into the trap; but why "bewachsene" instead of "bemooste" in translating the passage for thy countrymen, O Zöllner? Prof. Zöllner's remarks upon British philosophers as a class must be given in his own words:—"Allein die Speculation ist in der gegenwartigen Entwickelungsphase der Naturwissenschaft ein so tief empfundenes Bedürfniss, dass selbst hein eut fast nur noch *inductiv* thätiges Volk, wie die Englander, der Versuchung nicht widerstehen kann, sogar uber *mathematisch-physikalischen Hypothesen* zu speculiren."

Then we have the old question about the discovery of Spectrum Analysis, Stokes and Balfour Stewart now coming in along with Thomson for their share of the *grêle de coups*. So severely accurate a judge as Zöllner should, however, have known that Stewart has pointed out that Kirchhoff mistook his meaning when he charged him with error as to the expression for internal radiation in terms of the refractive index. A sentence from Prof. Tait's address to Section A, last August, is used as a sort of weapon against British scientific men. Prof. Zöllner here quotes the part that suits him, leaving out altogether the portion (immediately following) which turns the charge entirely the other way.

Another indefensible style of controversy we must allude to as exhibited by our author. In the above extract from his introduction it will be noticed that he specially alludes to Section 385 of Thomson's and Tait's book. In the preface we find the following condemnation of it, which is calculated to ensure notoriety. "I venture deliberately to assert that in the whole of German physical literature there cannot be found one single text-book which, in the short space of only thirty lines, contains such a plentitude of *absolute* nonsense." It is true there is a jokelet in this (now) celebrated section, something about catching a luminous corpuscle and examining it—it forms the text for a good many severe remarks; but Prof. Zöllner goes further, and accuses the authors of the work of discourtesy to Weber, which certainly no one who understands the original or the German translation (which we have taken the trouble to consult) will find in either.

Whether Thomson and Tait, with Helmholtz and Clerk-Maxwell on their side, or Weber with the assistance of Neumann (and Zollner), shall ultimately be found correct on a purely scientific question, it is not our present business to inquire; what we do object to is the sensational imputation of discourtesy, if not of something worse, especially when the object of this imagined insult is a venerable philosopher who will undoubtedly leave a permanent mark on the history of his time.

It would next be our task to show how heartily Helmholtz is pitched into for having sanctioned by his name the Geman translation of the work in question, and for his worthy recognition of Sir W. Thomson's scientific discoveries; but enough—Deutschland über Alles, and down with every Deutscher who sees aught to admire or to respect beyond the limits of Germany!

CONCRETE ARITHMETIC

Concrete Arithmetic; an Introduction to the Elements of the Abstract Science of Number. For young Children. By Temple Augustus Orme. (Groombridge and Sons, 1872.)

"SIR, according to the custom of this town, he is of age when he knows how to count up to twelve pence; and he shall answer in a writ of right when he is of that age."* The work before us will not only enable a pupil to do this, but further gives an excellent account of the first six processes enumerated in the old poem †:—

Septem sunt partes, non plures, istius artis;
Addere, subtrahere, duplareque dimidiare
Sextaque dividere est, sed quinta est multiplicare
Radicem extrahere pars septima dicitur esse.

---

* Year-book of Edward I, A D. 1292. Salop Iter., edited by Horwood, p 220.

† The *De Algorismo* quoted from Halliwell's "Rara Mathematica," by De Morgan, "Arithmetical Books," p. 15.

The passage from the one rule to the next is very gradual, but never tedious; there is copious explanation, but yet we think no superfluous verbiage. Every page is to the point, and marks the writer as one who has had practically to deal with the sort of young minds for whom his work is primarily if not solely intended. The book is a thoroughly rational one, though at first sight the reader may think he has stumbled by mistake upon an elaborate treatise on the subject of dominoes, for the method of teaching is based on a combination of what has been called "palpable" with written arithmetic. In actual teaching the palpable method is the one adopted. Mr. Orme's own words are, "Teachers are strongly recommended to take every available opportunity of using suitable blocks of wood to represent units, tens, hundreds, and thousands, and I should wish it to be distinctly understood that, without these blocks or some substitute for them, the subject will become too abstract to be comprehended by those for whose sole benefit this book has been compiled" (Preface, p. 4); and on p. 7 the resemblance of each diagram to a domino suggests the remark, "Learn to play at the game called dominoes; this will teach you how to tell the number of dots in a figure very quickly."

The main divisions are occupied with units, or single unpacked things; tens, or single unpacked deca-units; hundreds, or single unpacked hecto-units; and thousands, or single unpacked kilo-units. From some of the terms here employed, it might be supposed that the work treats principally of questions concerned with metres, decimetres, and other quantities, which require an acquaintance with the metric system; but this is not the case, though it is well fitted to serve as an introduction to the use of such a system. Special reference to this system is confined to two pages of "Directions," where the dimensions of a square centimetre are represented in a figure.

The following extract from the Preface will serve to show the spirit by which the writer is actuated:—"If the teacher proceeds in this way (*i.e.*, by the palpable method of using blocks) he will be amply rewarded by finding that children, not so stupid as they are often said to be, will frequently make out processes of their own for arriving at truths, having been taught to rely not on rules, but on reason; and occasionally the youngest pupil will unconsciously show his teacher how to teach. . . . . More good will arise from the introduction of a scientific method into the teaching of the ordinary subjects of education than will accrue from object lessons, or the freely-accepted dicta of men of science." In thus letting Mr. Orme speak for himself, we shall best put before our readers the object aimed at in his book—an object which he seems to us to have compassed; and as we think it is a right one, it is on this ground we venture to recommend the work to all who may be in search of a good elementary introduction to arithmetic. If they carefully follow out the advice given, and pursue the plan laid down, they will convey a sound and accurate view of the subject, and that without wearying the young student. There is good store of simple and varied exercises in this handy volume, which may readily be curtailed or enlarged, according to each individual case. The only typographical faults we have to point out occur on pp. 30, 104, and 105, and are readily corrected. We commend the book as the work of a "cunning" arithmetician. "It is pitie that commonlie more care is had, yea, and that emonges verie wise men, to finde out rather a cunnynge man for their horse, than a cunnyng man for their children."[*]

OUR BOOK SHELF

Contribution to the Biology and History of the Development of the Ustilagineæ. By Dr. A. Fischer von Waldheim. Translated for the Transactions of the New York State Agricultural Society for 1870. From "Pringsheim's Jahrbucher," vol. ii. part 1, 2, 1869. (Albany, New York: 1872.)

THE section of Fungi to which the Ustilagines belong has occupied considerable attention from mycologists during the past quarter of a century. Old notions of the autonomy of species have been dispersed, and at the present time all the old genera are suspected, some are condemned, and not a few amalgamated. The present condition of the classification of the Uredinous Coniomycetes is eminently transitional; so much has been written, so many observations made that await confirmation, or require further researches to render the work complete, that no one would be rash enough to predict what another twenty years may accomplish towards settling the relations of the genera in this group to each other. From the vague notions and doubts of Unger's "Exantheme" in 1833, Leveille's researches in 1839, Tulasne's first memoir in 1847, De Bary's "Brandpilze" in 1853, Tulasne's second memoir in 1854, there has been a regular advance in the accumulation of observations and the record of facts up to the publication of Fischer von Waldheim's communication in 1869. The relations of *Trichobasis* to *Puccinia*, of some species of *Lecythea* to *Melampsora*, of others to *Phragmidium*, are admitted on every hand; but whether *Trichobasis*, *Puccinia*, *Uromyces*, and *Æcidium*, shall all give way to an amalgamated genus, in which the four forms shall be recognised as four conditions of the same plant, though accepted by some, cannot yet be considered as settled beyond a doubt. The more sceptical of mycologists suspend their judgment, and await the confirmation of certain observations. Whatever the result may be, there can be but one opinion that such men as Tulasne, Leveille, De Bary, and others, deserve all commendation for the work they have accomplished. Whilst the Uredines Proper have suffered greatly in the stability of their generic distinctions, the Ustilaginous group has at present maintained its character for the autonomy of its species. Up to the present *Tilletia*, *Ustilago*, *Thecaphora*, and *Urocystis*, seem to represent comparatively stable genera. It is not impossible that this is more *seeming* than *real*, and that future workers may reveal affinities more close than as yet are suspected. The "contribution" of Fischer von Waldheim dates as far back as 1869, although now presented for the first time in an English dress, and we have to thank our Transatlantic cousins for having accomplished this fact. The New York State Agricultural Society is wise to diffuse this and all similar information amongst its members. None are more deeply interested in the development, conditions of growth, and metamorphisms (if any) of the "Smuts" than agriculturists, and to no societies should we look with more confidence for the publication of such memoirs as the present. How far they have accomplished this belongs to the past, and has become history; what they may do in the future lies within their own power. The memoir now published commences with a very good digest of the literature of the subject, after which follow the personal observations of the author on the mycelium and spore growth in *Tilletia*, *Ustilago*, and *Urocystis*. The synopsis of the Ustilagineæ in relation to their supporting plants and the place of their spore formation will be very useful to students, as will also the counter-synopsis of the supporting plants, and the Ustilagineæ occurring on them. The details of the germination of spores, direction and character of the promycelia, the effects of moisture, light, &c., measurements of threads and spores, all combine to render this a useful contribution to the literature of the Smuts, although not containing any startling discoveries. It is just what it professes to be, the record of observations on the germination of the spores of several of the Ustilagines under artificial cultivation, as a supplement to Tulasne's memoir in which this history of development was deficient. It would have been an advantage had this "Contribution" made its appearance in the trade as a separate publication with a London publisher, at a fixed price, so that all persons interested in the subject in this country might have obtained copies, and recommended the work to their mycological, horticultural, and agricultural friends. M. C. C.

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[*] Ascham, "The Schoolmaster," Book I.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Philippine Islands

Enclosed you will find :—1. A list of Earthquakes on the Philippine Islands from January to March 1872. As communication is very bad here, and meteorological observations are seldom made and noted down, I may say that most of the earthquakes do not come to our knowledge at all. I believe I do not say too much in expressing my opinion that there may be, at least, one earthquake every day at some one spot in this Archipelago. 2. Description of a Typhoon, which I witnessed at Cebu.

EARTHQUAKES ON THE PHILIPPINE ISLANDS

Since my last communication to the list of earthquakes to your journal (Feb. 5, 1872) I have noted the following :—

1872
Jan. 27, Zambales in Luzon, E.-W., many and strong shocks.
Feb. 7, Camarines on Luzon, twice.
Mar. 5, Manila, weak, 9 A.M.
6, Province Laguna in Luzon, 9 A.M. } These two were perhaps on the same day, and a mistake has been made in the letter which announced the second.
22, Manila, several very strong shocks.
,, Province Batangas in Luzon.

TYPHOON AT CEBU

April 4, I witnessed a Typhoon in the harbour of Cebu, Philippine Islands, on board H.M.S. *Nassau*, Captain Chimmo. The following gives a short description of it, hoping that the officers of that ship will publish a detailed account of this interesting storm :—

		Barometer	
8ʰ	A.M.	29·96	Strong N.W. winds, heavy rains.
2ʰ	P.M.	29·82	
3ʰ30ᵐ	,,	29·74	
6ʰ	,,	29·54	Wind, N.W. (4 to 8) rain.
7ʰ	,,	29·40	Wind and rain ceasing, till
8ʰ	,,	29·30	some heavy squalls from N.W., followed by perfect calm.
8ʰ15ᵐ	,,	29·28	Lowest marking of the barometer, till most furious squall from S.E. (11) with heavy rains and lightning. This furious storm lasted about ten minutes, and then, varying from S.E. to S.W. (hardest from S.) diminished slightly; barometer rising rapidly.
10ʰ	,,	29·76	Wind steadier, squalls less frequent, and heavy rain ceased.
12ʰ	,,	29·86	Wind steady from S.S.E. (5) dying away till daylight.

About eighteen vessels were thrown on shore, more or less damaged, many houses unroofed, and native huts blown away throughout the island Cebu, and several lives lost. At Horlo in Panay the storm did a great deal of damage too ; at Manila it was not observed at all It is said that there must have been at the same time a storm at Sargoon. Typhoons are very rare as far south as Cebu, and are said not to have been observed for twenty years. This storm proves to be a real typhoon, according to the variation of the wind and the calm betwen it, showing that the centre passed Cuba.

Manila, April 15 ADOLF BERNHARD MEYER

The Conservation of Energy not a Fact, but a Heresy of Science

Permit me a few words in reply to Mr. Brooke's strictures in your journal (No. 137) upon my article on "The Heresies of Science" in the *London Quarterly Review* of July last.

Mr. Brooke asserts that in the article "two widely different principles are oddly linked together as heretical dogmas, the doctrine of Evolution and the Conservation of Energy." Now, so far from these doctrines being oddly linked together as heresies, they are not linked at all. It is not the doctrine of Evolution, but the hypothesis of Natural Selection that I affirm to be one of the great heresies of modern science. Evolution is dealt with only so far as is found necessary to prove that the theory of Natural Selection is false. The two heresies named are connected in the article because I found so many physicists, employing them to overthrow some of the best established truths in philosophy. Of this Mr. Brooke is perfectly aware, since he expresses regret that "the principle of the Conservation of Energy has by some been misapplied in a fruitless endeavour to supersede the necessity of a creative intelligence."

"The Conservation of Energy a Fact, not a Heresy of Science," is the title of Mr. Brooke's paper. To this assertion I need only oppose some of his own admissions. He complains that the proposition—viz., "that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity—has been by some writers rather overstrained." "It may," he adds, "be taken as a postulate, and is probably true, but it is a proposition that is equally incapable of proof or of disproof, because the amount of potential energy in a body can be determined only by its development into actual energy, and cannot therefore be predicated."

Are then our judgments respecting that which Mr. Brooke asserts to be a fact of science only probably true? Surely there is contradiction here. I take it that science is knowledge, and that consequently judgments not accompanied by a conviction of certainty, but merely possessing a higher or lower degree of probability, are altogether outside the sphere of science. As Mr. Brooke accepts the doctrine of the Conservation of Energy as a truth of science, it is not competent for him to maintain that the proposition—viz., "that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity"—is equally incapable of proof or disproof, unless he can show that it expresses one of those primary convictions of the mind which constitute the very starting points of human thought. Let Mr. Brooke do this, and there is an end to all discussion on the subject. By one of the laws of thought a proposition which can neither be proved nor disproved, but by other propositions not more evident or more certain, must, by all rational minds, be accepted as true. In this region doubt becomes suicidal by self-contradiction. It is easy to show that the proposition which constitutes the scientific expression of the doctrine of the Conservation of Energy is not the symbol of a primary synthetical judgment. It is really nothing but a truism rendering to the scientific inquirer no higher service than the statement that "every effect must have a cause." In all such cases we grant the truth of the proposition when we grant the definition of either of its related terms. "That everything which begins to be has been produced, immediately or mediately by the power of an intelligent being," is the only philosophic expression of the great law of causation. Stated thus it becomes the appropriate symbol of a primary and necessary synthetical judgment of which every sane mind is conscious. No less a thinker than the late Sir John Herschel held that the doctrine of the Conservation of Energy is a mere truism. It is so as the result of the introduction of what he terms the unfortunate phrase "potential energy."

Mr. Brooke says that "energy is the power of doing work." He does not tell us what he means by work. If he means motion in any of its modes, then he confounds what he holds to be distinct realities, viz., Force and Energy. Further, he states that "the term 'potential' applied to Force or Energy means inactive, but capable of being called into action. Thus, if a weight be raised, a certain amount of energy is expended in raising it, and so long as the body is supported the energy expended in raising it remains potential in it, but when allowed to fall freely *in vacuo* to the level from which it was raised, the body acquires exactly the amount of energy that was expended in raising it." This too is the view of Tyndall and Balfour Stewart. According to this doctrine, if I throw a stone upwards, say to the height of twenty feet, the energy expended is not lost, but gradually changed in form as the stone ascends. When the stone leaves the hand its energy is actual, at its maximum height it is potential. As a form of potential energy it is a kind of power existent in the stone, but unexerted until the stone begins to descend. According to the theory of conservation this unexerted power is not a reality which abides in the stone by virtue of its constitution, but a power that may be lost, and lost as quickly as it was gained. When the stone reaches the ground it possesses no energy beyond a portion of the heat generated by the collision. The advocates of the conservation hypothesis tell us that the potential energy of the stone at its maximum height is a power to do work. We inquire what work, and are gravely assured that the stone has power to fall, which it could not do when it rested upon the ground! Let us suppose that when the stone leaves my hand I was standing on a covered coal-pit two thousand feet deep, and that I remove the cover as the stone descends, if, as Mr. Brooke affirms, the Conservation of Energy is a fact, it follows that when the stone has fallen through twenty feet it will remain suspended over the pit. By its ascent it acquired power to fall only twenty feet, not two thousand and twenty feet. The metaphysicians, so much belaboured by Prof. Tait and other physicists, have ventured to think that the force of gravity has something to do with the fall of the stone. I have certainly found myself unable, even with the aid of the scientific imagination, to form an intelligible idea of the reality supposed to be symbolised by the term "potential energy." The theory of the Conservation of Energy as now maintained by physicists is opposed in several respects to the doctrine of the conservation of force as held by Faraday. Stewart, Brooke, and others teach most explicitly that energy is not only constantly changing its form, but always shifting about from one portion of matter to another. If I mistake not, Faraday asserts the very opposite respecting force. He seems to teach that each material particle, into whatever combinations it may enter, retains all its original forces. "A particle of oxygen," he says, "is ever a particle of oxygen."

Mr. Brooke makes other admissions which are as inconsistent with the truth of the doctrine of the conservation as the one I have examined. These, however, I must leave for the present. I feel that the most satisfactory reply to Mr. Brooke's strictures would be to quote here, with two trifling exceptions, the portion of my article which relates to the conservation of energy. Those exceptions I will now name. First, I withdraw what I have said respecting Mr. Brooke's view of the nature of latent heat. My sole reason for not in this connection quoting more was that I had assumed his perfect agreement with Prof. Tait. In this it seems I was wrong, since Mr. Brooke declares that he is unable to derive any definite idea from Prof. Tait's statement. I am sorry that Mr. Brooke should have supposed that the omission of the sentence named was due to a lack of literary honesty. I wonder that it did not occur to him that another and more charitable explanation was possible. Secondly, I was in error as to the weights employed by Dr. Joule in one of his experiments for determining the mechanical equivalent of heat. But this error relates merely to the form, not to the ultimate result of the experiment; and consequently in no way invalidates my reasoning. Holding, as I do, that forces are both conserved and correlated, I feel no difficulty whatever in accepting the facts established by Dr. Joule. He avoids speculation regarding the nature of force in itself, and deals exclusively with its manifestations. Thus, his discovery of "the mechanical equivalent of heat" is the discovery of a relation between two classes of effects.

There is one misrepresentation in Mr. Brooke's review of my article I must here point out. He says, "the reviewer thus quaintly expresses the relations of force, energy, and motion:—A given motion viewed as a cause is force, while the very same motion thought as an effect is energy." But this is not my doctrine. I am here dealing with the consequences of one of Mr. Justice Grove's assumptions, viz., that if we attempt to analyse our conception of force, viewed as the cause of any perceived motion, we can get nothing beyond some antecedent motion. Mr. Brooke complains that the misapplication of the term "force" has led to great confusion in physics. His own statements are nevertheless unsatisfactory, if not contradictory. He accepts the definition of force given by Faraday. But this so-called definition by Faraday is not definition at all. It merely tells us what force does, not what force is. Mr. Brooke adds that the definition "may perhaps with advantage be thus amplified:—Force is a mutual action between the atoms or molecules of matter."

But these molecular actions or motions are the effects of force, but not force itself. In no instance whatever can force be resolved into molecular motion. Mr. Brooke says, "One finds oneself occasionally brought by circumstances into an unwelcome generalisation. Thus the reviewer, speaking of the supporters of 'conservation' in the lump, says 'they take it for granted that force is motion and nothing but motion.' This the writer entirely and absolutely denies." Will Mr. Brooke show that this denial is in harmony with his assertion that "force is a mutual action between the atoms or molecules of matter?" I cannot. My reasons for rejecting the assumptions on which the doctrine of the conservation of energy rests are not noticed by Mr. Brooke. These assumptions I have shown belong to false and exploded metaphysics." A false philosophy of causation, it is easy to prove, has greatly retarded the progress of science.

I perceive that Mr. Brooke has used for reference one of a small number of copies of my article printed for private circulation. Unfortunately the paging does not correspond with that of the review. Had I only anticipated the pleasure of an encounter with Mr. Brooke, I would gladly have sent him the review itself. As Mr. Brooke is aware of what passed at a very recent meeting of the Victoria Institute, I cannot longer withhold my name.

Sale, near Manchester, June 26 JOHN MOORE

Water Analysis

IN NATURE for June 27, 1872, Mr. Wanklyn directs attention to the facts that his paper on water analysis appeared in 1867, and that in 1868 he gave some absolute errors obtained with his process.

Mr. Wanklyn then proceeds to say:—"We have never said that distillation of albumin with alkaline permanganate converted the whole of the nitrogen of the albumin into ammonia. The assertion in your article is therefore untrue." Mr. Wanklyn's ideas of truth are probably peculiar, for if he will refer to his paper of June 20, 1867 (Chem. Soc. Jour. vol. v. N.S. p. 448), he will find the following:—"Direct experiments in which a known quantity of urea, gelatin, and albumin were taken, have shown that all the nitrogen in these substances is obtainable in the form of ammonia when they are subjected to the treatment about to be described, and has disclosed the very singular fact that boiling with a caustic alkali liberates one-third of the nitrogen, both of albumin and of gelatin, in the form of ammonia, and that a subsequent boiling with permanganate of potash liberates the other two-thirds."

Not a word is said in the paper about carrying on the permanganate treatment to dryness, and the only reference to such treatment is on page 450, where it is stated that boiling to dryness with potash alone causes the evolution of a "full third" of the nitrogen as ammonia.

THE WRITER OF THE ARTICLE

Scintillation

CAN any of your scientific correspondents tell me whether the following observation has been published, and, if so, where?

By very slight squinting, or (as suggested to me by a friend) by a slight pressure on one eye, we obtain two images of a star as viewed simultaneously from two stations a few inches apart. We made the experiment some nights ago, and could detect no relation whatever between the scintillations of the two. This seems to explain how little trace of the phenomenon remains when a telescope is used, for in that case we have a sort of integration performed over the whole aperture of the object-glass.

G. H.

To Entomologists

HOW often is it that the entomologist has to regret the want of his net? The rare butterfly, by some curious perversity, is certain to be seen when the net is not at hand. How, under such circumstances, is the butterfly to be caught? I reply, wait till it settles, and then pick it up. Perhaps most of your readers will reply "Absurd, no butterfly will sit to be caught in that way." Try and see. Whenever the butterfly settles you may walk quickly up to within a short distance from it; the distance will depend on the nature of the butterfly. Arrived at a short distance from the butterfly, the motion must now be slow and even, and as the hand is slowly and steadily advanced towards the butterfly, it will take little or no notice of it, and may be easily picked up with the fingers. In Italy this spring I picked up in this manner both varieties of swallow-tails, as many as five when walking one afternoon. I have also caught in this way, whites, red admirals, painted ladies, peacocks, and many other smaller varieties. This manner of catching butterflies does look suspiciously like the old story of catching birds by putting salt on their tails. Before anyone condemns it, I ask him to give it a fair trial, and I have no doubt he will be astonished at his success. The only thing to guard against is any jerkiness in your motions. All your motions when near the butterfly must be slow and regular. Perhaps the butterfly may not sit long enough for you to approach it by such slow motions. If the butterfly does rise take care not to change your slow and steady motions, and it will take no notice of you, and will often settle again within a few inches of your hand. Often the butterfly takes no notice of you; at other times it seems sensible of danger, but generally contents itself by folding its wings as close as possible, as if to escape notice. When the wings are in this position the butterfly is caught with least damage to its plumage. But in some cases the wings are open. When that is the case I have sometimes folded the wings before picking them up, in order to save the plumage, so tame are they under this treatment. At first I thought it was necessary to approach the butterfly from behind, and keep out of sight as much as possible, but this I find, though an advantage, is not essential. Of course I do not advocate this plan of catching butterflies when a net can be got, as it is much slower and not so certain. Yet it has its advantages. You get the butterfly without breaking its wings or ruffling a feather, and if not a good specimen you can let it away unhurt.

J. A.

ERNEST T. CHAPMAN

MR. E. T. CHAPMAN has met his death by an explosion in his laboratory at Rubeland in the Hartz. A letter containing this sad news has the following particulars:—

"As you are probably aware, Mr. Chapman's work was always in the laboratory, and it was there on the 25th inst. (June) that the accident occurred. On the morning of that day Mr. Chapman had a conversation with the gentlemen here, and shortly after this, at 11 A.M., the bomb-proof building in which he was with three workmen exploded with a tremendous crash. Mr. Chapman may perhaps have informed you that latterly he has been chiefly engaged with the production of nitric methyl-æther, with which he has been making various experiments, and we cannot explain the catastrophe otherwise than that he was not thoroughly aware of the great explosibility of this substance. The workmen present having also all perished, it is unfortunately not possible to obtain any details. The force of the explosion was so tremendous that all the surrounding buildings have been more or less injured, and about ten people seriously wounded."

The substance I presume was nitrate of methyl. If so, this lamentable accident furnishes another proof of the treacherous nature of explosives which, like nitro-glycerine and gun-cotton, contain hydrogen and carbon associated with nitrogen oxides.

Mr. Chapman was only in his twenty-seventh year when his career was thus prematurely closed. A pupil of Hoffmann and Kolbe, he was a prolific author of original researches in organic chemistry. Perhaps the best known of Mr. Chapman's researches is his study of limited oxidation. This process, in his hands and those of others, furnished chemists with a valuable method of chemical diagnosis. The little work on the Analysis of Potable Waters, by Mr. Chapman and Mr. Wanklyn, is a well-known work of reference on this important subject.

Mr. Chapman was an enthusiastic worker. His manipulative skill was of a high order, and his acquaintance with organic chemistry very extensive, his researches in this branch of science being very numerous. If he had lived, and had an opportunity of continuous scientific work, it is impossible to doubt that he would have contributed his quota towards rescuing our country from the too just reproach of rapidly becoming more and more sterile in chemical discoveries.

His intimate friends esteemed him highly, for he was a man of varied culture and singular conversational power. It was always a matter of regret to all true friends of science that a man of such proved ability and promise should have been compelled in a manner to banish himself in order to gain a livelihood. His letters show that even in the remote place of his exile his brain was busy with chemical and physical questions. He must have been killed instantly, and therefore without pain. And certainly as a brave and loyal soldier of science slain on the battle-field of the laboratory, his death, like his life, showed his unwearied devotion to science. We can ill afford to lose such men.

FREDERICK GUTHRIE

DR. WILLIAM STIMPSON[*]

DIED, at Ilchester, Maryland, May 26, of consumption, Dr. William Stimpson, Secretary of the Chicago Academy of Sciences, in the forty-second year of his age.

The announcement of the death of Dr. Stimpson will be received with profound regret, not only by a wide circle of friends here, but throughout the country. Science has lost an assiduous cultivator, the value of whose labours was recognised in both hemispheres.

For the second time in the course of a few years the Chicago Academy is called upon to mourn the loss of an accomplished secretary. Under Dr. Stimpson's energetic supervision, the collections gathered within its walls at the time of the great fire, in magnitude and importance ranked fifth in the United States, and so admirable were his arrangements that they were flowing in from every quarter of the world. Their total destruction on the morning of the 9th of October last was a terrible blow to the secretary. His private losses, too, were beyond computation, embracing as they did a choice scientific library, gathered at different intervals—many of the volumes being out of print, and many of them being presentation copies from the authors—and also all his manuscripts, including those of the Government Japan Expedition, of which he was the naturalist, which were written out for publication, and were copiously illustrated by drawings, many of which were engraved. Thus, in an hour, perished the results of twenty years' unremitting scientific labour.

To show the high estimation in which Dr. Stimpson was held by men engaged in kindred pursuits, it may be stated that, at the instigation of Agassiz, the results of the deep-sea dredgings of the United States Coast Survey were passed over to him for description—a task calling for the most exact and extensive knowledge.

After the terrible calamity to which we have referred, Dr. Stimpson remarked that he had not the heart to attempt to enter upon his life-work again, but would devote all his energies to the restoration of the Academy.

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[*] From the Chicago *Inter-Ocean*, Communicated by Mr. J Gwyn Jeffreys.

"If," said he, "I live to the ordinary age, I will gather a nobler collection than we have lost." The Smithsonian Institute, which from the first had been the generous patron of the Academy, was ready to transfer to him duplicates; the Museum of Comparative Anatomy, at Cambridge, through Agassiz, its director, had invited him to go there and select from its ample stores what he desired; and learned societies at home and abroad, in response to his appeals, were forwarding to his care copies of their Transactions. Under such auspices, the members of the Academy felt that its losses would soon be restored, and that its prestige would be more commanding than ever before. They now feel that in the death of their secretary they sustain a loss which is irreparable.

Dr. Stimpson had for some years the premonitions of the disease to which he has just succumbed. Two years ago he passed the winter on the Florida coast, making extensive collections of the flora and fauna of that region. The moist, warm breath of the ocean, he thought, invigorated him. Last autumn he repaired, in company with Dr. Veille, to the same region, embarking on board one of the Coast Survey steamers to superintend the deep-sea dredgings; but he had returns of hæmorrhage, which so far prostrated him as to defeat his purposes. For seventy days he remained on board, with nothing but ship fare to eat, at which his stomach revolted. Gaining the land, he was transferred to a hotel, but his physical powers were past the rallying point. Accompanied by Dr. Veille, he was at length placed on board a steamer, which landed him in Baltimore, from which place, by a short journey in a carriage, he was conveyed to Ilchester, where reside his wife's friends. Here he lingered a few weeks, dictating letters and pencilling short ones to his most intimate friends. And now comes the intelligence that the grave has closed over him, and that in his dying hours his thoughts centred on the Academy.

Dr. Stimpson had qualities which attracted, by the strongest ties, all who had personal relations with him. Modest and retiring in his disposition, the casual acquaintance little knew the vast range and the minute accuracy of his information—information gathered not simply from books, but from personal observation in every quarter of the globe. He has published enough already to create for his name an honourable place in the scientific opinion of the world; but if the full results of his labours could have been brought out, few scientific men in the country would have occupied a more commanding position. J. W. F.

THE CEYLON ELEPHANT AT THE OXFORD MUSEUM

THERE has just arrived at the Oxford Museum the skeleton of a full-grown male Ceylon elephant. During the visit of the Duke of Edinburgh to Ceylon there were two elephant drives, or kraals, held, of which full accounts were given in the daily papers at the time. It may be remembered that at one of these kraals a large male elephant, a rogue, was driven into the terminal enclosure, together with several other smaller wild elephants. The rogue charged the tame elephants introduced amongst the wild ones, knocking them over repeatedly, and effectually preventing the process of noosing. It was at length found necessary to shoot him, in order that the remainder of the elephants might be secured.

The Governor of the island, Sir Hercules Robinson, desired that the skeleton of the animal should be preserved, and the Hon. P. C. Layard undertook the task. The elephant was covered slightly with earth, so as to protect the bones from rapacious birds; and the mound thus formed was fenced round to keep off jackals, &c. As soon as the bones were cleaned, they were collected and transported to Colombo. Mr. Layard, with his well-known zeal for the promotion of science, took immense pains that the small bones should not be lost, and the skeleton is wonderfully perfect considering the great disadvantages under which it was prepared.

The bones were presented by Sir Hercules Robinson to the Oxford Museum, and all expenses of preparation and packing were defrayed by the colony. The skeleton was packed in two rum puncheons, under my superintendence. In packing heavy bones such as these for a long sea voyage great care must be taken, as, unless they are securely wedged into their places, they will grind against one another and get spoiled. There is nothing better than a cask for packing bones. Straw bands should first be wound round each of the large bones, and, the head having been taken out of the cask, these large bones should be jammed in as closely as possible. The interstices should be filled with smaller bones and straw, which latter should be rammed in tight with sticks. As soon as the cask is quite full the head should be put in, and that end strained up tight with its hoops. The other end of the cask should now be turned uppermost, the hoops knocked off and the bottom taken out. It will be found that a good many more bones may now be introduced from this end, which must be rammed quite full like the other. By thus packing a cask from both ends, almost absolute immobility may be secured for the contents. The elephant's skeleton is a very fine one, and I believe the only one in England of a wild specimen. The ordinary museum specimens are all from menagerie animals, the muscular ridges and tuberosities of the bones of which are always badly marked. The elephant had what is called a tush, *i. e.*, a small short tusk on the left side. This tush is in the collection of the Duke of Edinburgh.

With the elephant arrived a Dugong, also presented to the Oxford Museum by Sir Hercules Robinson. The animal, which is a young one about 5 ft. long, was procured by Mr. Twinam, Government Agent at Jaffna. It was filled with salt and packed in charcoal. It has remained more than a year thus packed up, and is unfortunately in a very pulpy condition, although its external form is remarkably well preserved. Mr. Robertson hopes, with care, to be able to make a skeleton of it.

A case containing some human skulls, and the skeletons and skulls of various smaller animals collected by me in Ceylon, arrived at the same time; and a fourth case, containing skulls of all the non-European races now inhabiting Ceylon, including some of undoubted jungle Weddo, collected by Mr. B. Hartshorne, of Panwila, and late of Pembroke College, Oxford, who has for a long time been investigating the language and habits of this very interesting race, and will shortly publish a paper on the subject.

The shipping arrangements of all the cases except the last, and their transmission through the Custom-house, were carried out by Messrs. Green and Co., of Colombo, who most liberally gave their services free of expense in the cause of science.

H. N. MOSELEY

SUPERFLUOUS DEVELOPMENTS AND HETEROGENESIS

THE remarkable and suggestive results of experimental research obtained by Prof. Charlton Bastian, together with the no less striking arguments which he has recently put forth in the pages of the *British Medical Journal* in favour of the doctrine of Evolution, appear calculated to throw light on certain phenomena in Biology which have hitherto received but scant attention. Even if Dr. Bastian's views do not gain general acceptance, so bold an enunciation of them can scarcely fail to be productive of the very best results, since (as has ever been held by philosophers) truth is only elicited and advanced by conflict of opinion.

Not merely in the lower, but also in the higher forms of animal life the microscopist is constantly encountering anomalous appearances, structures, "bodies," &c.; some of these developments being regarded as normal, whilst others are relegated to the category of pathological products. In very many cases the mysterious organisms in question have been described as "glands;" the variable character of this system of structures affording a convenient refuge for the destitute. More astute observers, however, refuse to adopt such subterfuges, and have accordingly been satisfied either merely to note their characters without forming any definite conclusions, or they have gone only a step further by placing them within the territory of superfluous developments.

To take a few familiar instances occurring within the domain of helminthology. We have the so-called Raineyan sacs, regarded as the earliest stages of Cysticerci by their discoverer. Similar bodies were previously described as the products of muscular degeneration by Hessling. Finally, they were pronounced by Leuckart to be psorosperms. In the year 1856 I detected granular bodies in the early stage of development of the eggs of *Tæniæ*, respecting the nature of which I had then no means of forming any definite opinion. These were subsequently described by Weinland and Leuckart; the latter authority looking upon them as masses cast off from the primitive yelk, and thus differentiated to form an organ concerned in the production of the chitinous envelope of the egg. Then, again, Leuckart speaks of certain "croupy masses" (resulting from the enteritis produced by *Trichinasis*) as capable of resolving themselves into psorospermiæ: and I have myself witnessed what I presumed to be the actual conversion of the granular and molecular contents of the eggs of *Fasciola hepatica* into amæboids. Leuckart, however, from prior and independent observation, had considered these bodies to be the parasitic confervoid zoospores of *Chytridium*.

Undoubtedly, appearances of this varied description are very puzzling; not so much, however, in the view of determining their actual character and nature as for the correct interpretation of their true mode of origination. Perhaps, if one were not, in a measure, dominated by the preconceived idea that these last-named bodies must have sprung from invisible germs, the apparent ocular evidence to the contrary would at once be held as a sufficient explanation. Be this as it may, I may refer in this connection to some interesting facts which have recently been recorded by Profs. F. Sommer and L. Landois. To their interesting "Beitrage zur Anatomie der Plattwurmer" I shall again have occasion to allude for other purposes, but for the present I merely quote the following short passage. Speaking of structures observed in the segments or proglottides of *Bothriocephalus latus*, they say (p. 16):—

"On a level with the folds of the seminal ducts, either close to or lying between them, we observed in the majority of instances within the sexually mature segments round or oval hollow spaces from 0·055 to 0·288m. in diameter.* Their margins were sharply defined, most of them being filled with a finely granular molecular mass; in others the contents appeared coarsely granular, highly refracting. In an especial manner carmine tinctured the finely granular contents vividly red, whilst the coarse granules effected the reduction of osmic acid (*Ueberosmiumsäure*) in a very marked degree; on the other hand the reduction by the fine granules was less marked. Further research showed that these granules consisted of very minute fat particles which, since they were little affected by the direct application of ether, appear to possess an albumenoid covering capable of being slightly tinctured by carmine. We did not observe any of these formations in immature joints; their number also varied much in the ripe segments; of such we counted from one to six in a joint; other sexually mature proglottides being altogether free. They appeared especially in the neighbourhood of the lower end of the seminal duct in front and behind, where the seminal passage approaches the cistern-like seminal reservoir. We regard these formations as detached portions of the seminal canal, namely, of the larger ducts, whose contents are in a state of fatty degeneration."

Such are the facts. Are the bodies in question really pathological products, or are they not, rather, superfluous developments? It cannot be said they are necessary constituents of the parasite; and it will, perhaps, be denied that they are in any sense heterogenetically formed organisms. At all events, these cast-off bodies have a gregariniform look about them, judging from the representations given by Drs. Sommer and Landois. The expression "hollow spaces" (*Hohlräume*) is certainly rather misleading; the more so since they are rendered somewhat opaque by the crowding of their granular contents

If it be true, as some teach, that protoplasms only beget their own kinds of protoplasm, one is at a loss to understand many of the ordinary phenomena of metamorphosis. for myself, I look upon certain of these obscure developments as the result of a law of what I term "vegetative deterioration." Under ordinary circumstances (as for example in the case of the "innovations" produced from abortive prothallia in Ferns) the law of Pangenesis ensures a repetition of parts in all respects similar to those whence the buddings have arisen; but surely it is not necessary to regard all anomalous and detached structures either as morbid products on the one hand, or as germ-begotten entities on the other. Without letting the imagination run wild, it appears to me perfectly conceivable that "bodies" of a kind more organised than Drs. Sommers' and Landois' "formations," may result from the operation of this law of degradational metamorphosis or "vegetative deterioration."

T. SPENCER COBBOLD

DR. LIVINGSTONE'S DISCOVERIES

FROM Mr. Stanley's despatches to the *New York Herald*, which, by the courtesy of the English representative of that paper, have appeared in the *Times*, we gather some important and definite information as to the exact nature of Livingstone's discoveries; and more than this, we have a full explanation of the circumstances which kept our great traveller so long out of the reach of civilisation, and of the work he still hopes to accomplish

Mr. Stanley's account of his meeting with Livingstone is a touching one. After many delays, on the 3rd of November, 1871, he came in sight of the outlying houses of Ujiji, and, anxious to enter the African town with as much *éclat* as possible, he disposed his little band in such a manner as to form a somewhat imposing procession. At the head was borne the American flag; next came the armed escort, who were directed to discharge their firearms with as much rapidity as possible; following these were the baggage men, the horses, and asses; and in the rear of all came Mr. Stanley himself. The din of the firing aroused the inhabitants of Ujiji to the fact that strangers were approaching, and they flocked out in great crowds, filling the air with deafening shouts, and beating violently on their rude musical instruments.

As the procession entered the town Mr. Stanley observed a group of Arabs on the right, in the centre of whom was a pale-looking, grey-bearded, white man, whose fair skin contrasted with the sunburnt visages of those by whom he was surrounded. Passing from the rear of the procession to the front, the American traveller noticed the white man was clad in a red woollen jacket, and wore upon his head a naval cap with a faded gilt band round it. In an instant he recognised the European as none other than Dr. Livingstone himself; and he was about to rush forward and embrace him, when the thought occurred he was in the presence of Arabs, who, being accustomed to conceal their feelings, were very likely to found their estimate of a man upon the manner in which he conceals his own. A dignified Arab chieftain, moreover, stood by, and this confirmed Mr. Stanley in his resolution to show no symptoms of rejoicing or excitement. Slowly advancing towards the great traveller, he bowed and said, "Dr. Livingstone, I presume?" to which address the latter, who was fully equal to the occasion, simply smiled and replied "Yes." It was not till some hours afterwards, when alone together, seated on a goat skin, that the two white men exchanged those congratulations which both were eager to express, and recounted their respective difficulties and adventures.

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* About $\frac{1}{500}$" to $\frac{1}{80}$" Eng. measurement.—T. S. C.

Mr. Stanley's statement is that Dr. Livingstone appeared to be in remarkably good health, stout and strong, quite undismayed by all that he had gone through, and eager only to finish the task he had imposed upon himself.

Dr. Livingstone's story of his adventure was to the following effect:—In March 1866, he started from Zanzibar. The expedition which he led consisted of twelve Sepoys, nine Johanna men, seven liberated slaves, and two Zambesi men—in all thirty persons. At first Dr. Livingstone travelled along the left bank of the Rovuma River; but, as he pursued his way, his men began to grow disaffected and frightened, and, in spite of all his efforts to manage and keep them together, most of them left him and returned to their homes, spreading everywhere the report of his death as a reason for their reappearance there. In August 1866, he arrived in the territory of Mponda, a chief who rules over a tribe living near the N'yassa Lake; and here Wikoteni, a *protégé* of the Doctor's, insisted upon being absolved from going any further. After resting for a short time in Mponda's ground, Dr. Livingstone proceeded to inspect the "heel" of the N'yassa Lake; and it was while carrying out this enterprise that the Johanna men, who had till now remained faithful, deserted him. In December 1866, having collected a number of natives, Dr. Livingstone decided upon advancing in a northerly direction; and, in pursuance of this determination, he traversed the countries of Babisa, Bobembene, and Borunga, as well as the region of Londa.

Approaching King Cazembe's territory, he crossed a thin stream called the Chambezi; and here he found himself in great difficulty, being for a long while unable to discover to what the river belonged. The confusion which he experienced was greatly increased by the fact that Portuguese travellers had previously reported the existence of such a stream, and had asserted that it was a tributary of the great Zambesi river, having no connection whatever with the Nile. These statements Dr. Livingstone was disinclined to believe, and, determined to satisfy himself as to the rise and falling of the Chambezi, he made up his mind to devote himself to the task at once. From the beginning of 1867 to the middle of March 1869, he traversed the banks of the mysterious stream, tracing it where it ran, correcting the errors of the Portuguese travellers, and proving conclusively that the Chambezi was not the head of the Zambezi river, as had been hitherto supposed. So constantly did he remain at this work, and so frequent were the inquiries which he made in every direction, that the natives, in astonishment at his persistence, supposed him to be insane; and their frequent remark was, "The man is mad; he must have water on the brain." Their ridicule had, however, no effect upon him, for he continued his work in spite of every opposition, and as the result of his labours in this region, coupled with his further researches, he has established conclusively (1) that the Portuguese Zambezi and the Chambezi are totally distinct streams; and (2) that the Chambezi is the head waters of the Nile. He found that starting from 11° south, the River Nile rolled on until it attained the extraordinary length of 2,600 miles.

In the midst of his wanderings Livingstone came upon Lake Liemba, which he discovered to be fed by Lake Tanganyika. His map of the last-mentioned lake shows that the southern portion of it resembles in shape the lower part of the kingdom of Italy. He found that it rises in 8° 42″ south, is 325 miles in length, being thus seventy-three miles longer than was supposed by Captain Burton and Captain Speke. Leaving Tanganyika, the Doctor crossed Marungua, and came in sight of a small lake, called Lake Muero, which he found to be six miles in length, and to be fed by the Chambezi. In his way he traced the Chambezi running through three degrees of latitude, and having thus satisfied himself of the total independence of the Zambezi, he returned to King Cazembe's country, and then made his way to Ujiji, where, early in 1869, he wrote letters and despatched them by messengers. A short rest was made at Ujiji, and having explored the head of the Tanganyika lake, and thus finding out that the River Rusizi flowed into the lake, and not out of it, as had been supposed, he made preparations for another, and as he then hoped, a final journey of exploration.

Leaving Ujiji in June 1869, he pushed through the Uguhba country, and after fifteen days' march he came to Mamgema, which he found to be a virgin country, the interior of which seemed utterly unknown to anybody. As he was about to proceed, however, he was seized with an illness which at one time almost threatened to put an end to his explorations. Ulcers formed in his feet, and for six weary months he was obliged to rest and wait. As soon as he had recovered he started off in a northerly direction, and came shortly afterwards to a broad river called Lualaba, which flowed in a northerly, westerly, and southerly direction. Strongly suspecting that this river was but a continuation of the Chambezi, which enters the Banguereolo, Luapula, and Muero lakes, he retraced his steps to Lake Kamolondo, and thence working his way to lat. 4° south, and after a long and difficult journey, he found the point where the Lualaba and Chambezi joined, and proved them to be both one and the same river.

He followed the course of the latter river for several hundred miles, and had come within 180 miles of that part of the Nile which has already been traced, when the men he had with him mutinied, and deserted him. Having now neither stores nor followers, he was obliged to retire to Ujiji, weary and destitute. It was soon after this that Mr. Stanley found him. In fact, the English explorer arrived at Ujiji on the 16th of October, 1871, and it was, as already stated, no later than the 3rd of November when the American searcher made his entrance into Ujiji.

On Nov. 20 Dr. Livingstone and Mr. Stanley left Ujiji in company, and explored the northern end of Lake Tanganyika, confirming by a second inspection the observations which Dr. Livingstone had previously made; and after 28 days thus pleasantly spent, they returned to Ujiji, and there passed Christmas Day together. On Dec. 26 they left for Unyanyembe, and, arriving there, stayed together till March 14, when Mr. Stanley, intrusted with letters from Dr. Livingstone, started for the coast, leaving the explorer to continue his searches for some time longer.

Dr. Livingstone states that he considers he has yet two problems to solve in connection with the Nile. The first, the complete exploration of the remaining 180 miles which lie between the spot where he was compelled to turn back and the part already traced; and he should investigate the truth of a report which has several times reached him respecting four fountains, which he has been told, supply a large volume of water to the Lualaba. To complete this task, Livingstone estimates that he will require sixteen or eighteen months. Mr. Stanley, however, is of opinion that it will occupy a longer period.

MY GARDEN[*]

WITHIN about twelve miles of London Bridge as the crow flies, at Beddington, in Surrey, Mr. Smee has a garden, and the description and history of this garden are the subject of as pretty and entertaining a book as we have met with for a long time. In thus describing the book, we advisedly use terms which do not imply that it has any great scientific value in the sense of being the medium of publication of new facts ; nor, indeed, does it put forward any such pretensions. Mr. Smee, whose reputation as an original investigator in electrical science, and as the inventor of the galvanic battery which bears his name, is thirty years old, is in the domain of natural history essentially an amateur, and the work which he now publishes is an amateur's book. To rank it in the same class as Gilbert White's "Natural History of Selborne" is very high praise, but in some respects it certainly deserves it. Faults the book undoubtedly has ; some would call it egotistic, but it is a kindly sort of egotism, which interests the reader in the author and everything connected with him ; and here and there the critical reader will detect a slip betraying want of accurate scientific knowledge ; but these are very few compared with the amount of information contained in its pages.

Mr. Smee's garden consists of about eight acres, bounded on one side by an artificial lake, and watered by the river Wandle. When first brought into cultivation the land was a peat-morass ; but is now made to produce every variety of scenery that ornamental gardening can display. Here we have umbrageous forest-trees, and here a rustic bridge ; here a fern-glen, and here a nightingale bower ; here a rockery of Alpine plants, and here a glimpse of beautiful water scenery. Indeed, in looking at the exquisite drawings with which the book is embellished, it is difficult to realise that the scenes they depict can be reached from London in half-an-hour. The following passage, in Mr. Smee's own words, gives his ideas on the proper mode of laying out a garden :—

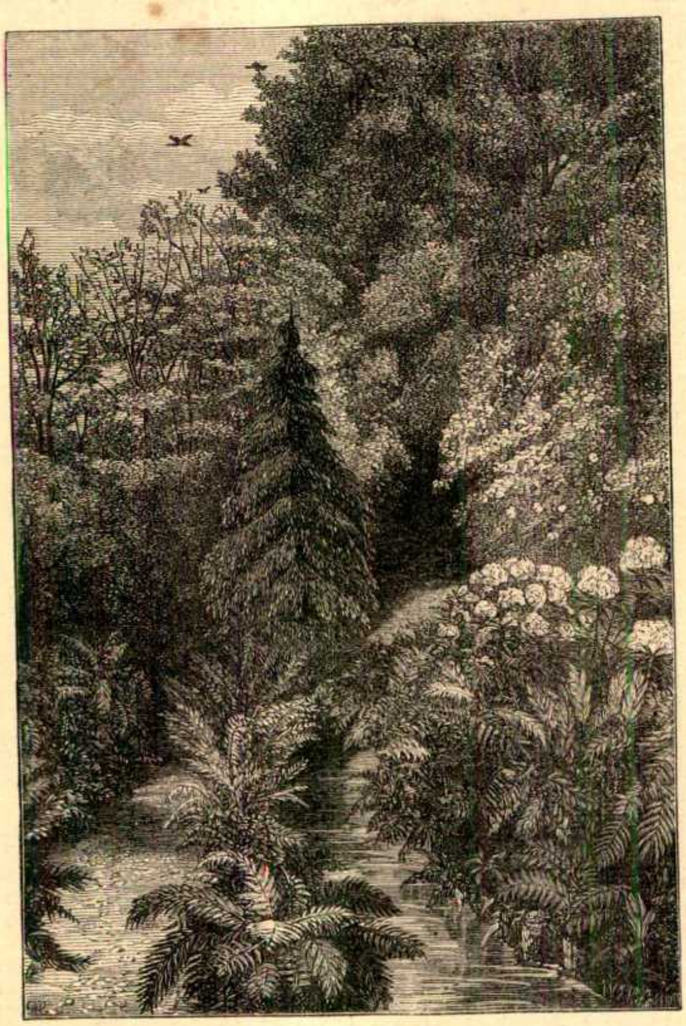

FIG. 1.—VALLEY OF FERNS

"It is a common notion that gardens should be laid out for one general effect ; but the result of such a plan is to produce a single view, and the whole can be seen at a glance. This is, however, monotonous, and my liking is to have many pictures ; so that my visitors have to walk a long way before they can see the many beautiful views which my garden affords ; and little spots of cultivated wildness, or of special cultivation, are found when they are least expected.

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[*] "My Garden: its Plan and Culture, together with a General Description of its Geology, Botany, and Natural History." By Alfred Smee, F.R.S. Illustrated with 1,250 engravings. (London: Bell and Daldy. 1872.)

"In all my designs, I have tried to suggest to the mind that it must be so; and even when my arrangements are most artificial—as when a walk doubles upon itself—it looks that the arrangement has been made because no other plan was really practicable; and when this idea is carried out, the garden looks natural.

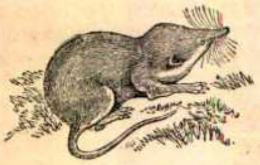
FIG. 2.—SHREW MOUSE

FIG. 3.—THE GOLDFINCH

FIG. 4.—HARVEST MOUSE

"Throughout my garden my vegetables, flowers, and fruit-trees are blended together in one harmonious whole: a plot of carrots and a row of flowering peas are beautiful objects in themselves, and hence plots of vegetables and fruit-trees alternate with rosaries, ferneries, alpineries, and flower-beds. . . .

FIG. 5.—ROAD NEAR BEDDINGTON

"A long straight line is, in a proper place, very pleasing, and my pear-tree walk is about 150 yards long, parallel with the Park palings. This walk is overarched, at intervals, with climbing roses; and planted on one side with pyramid pear-trees. The general effect is in the highest degree charming, when we come upon it from paths of curved lines, and view the chequered shade [upon the ath.

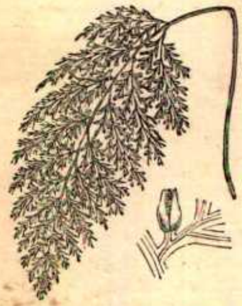
FIG. 6.—HYMENOPHYLLUM DEMISSUM

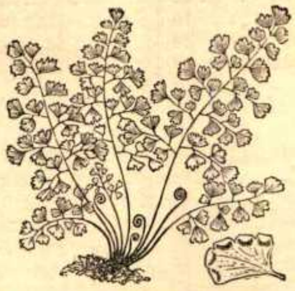
FIG. 7.—ADIANTUM CAPILLUS-VENERIS

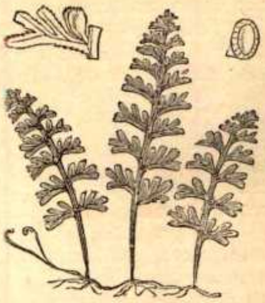
FIG. 8.—HYMENOPHYLLUM TUNBRIDGENSE

"Again, my fern glade is straight, and has a straight grass walk by its side. The nut bushes, on one side, are parallel with the stream; and the grass walk and rows of apple-trees, on the other side, are also straight. In this case also it would have looked unnatural to have had curved lines; and, although I once was tempted to try it, and had made preparations for curving the river, I ordered the materials away, so satisfied was I that straight lines alone would look natural for the occasion.

"With the exception of places where straight lines look natural, I eschew them, and also geometric figures, such as ovals, circles, octagons, as not suitable to the horticulturist, and unpleasing to the eye.

"In using curved lines beauty appears to consist in an ever-varying amount of curve. They should be parts of no regular figure, and the Indians have supplied us, in the patterns of their shawls, with forms which ever please us. In arranging them we must be guided by the eye, and frequently a variation of an inch or two makes an important difference in the effect which is produced."

But the charm of the book is the minuteness with which every detail of the garden is described; not only the flowers and fruits grown in it, the varieties which are found to answer best, and the best mode of cultivating them, but the animals, birds, beasts, and insects, which frequent it, or which have been occasionally seen in it. Mr. Smee thus gossips pleasantly about his ferneries:—

"For some years past ferns and ferneries have been much admired, and have received great attention from amateur cultivators; and with good reason, as their graceful forms are most attractive, their mode of growth interesting, and the colour of their fronds enchanting. Ferns should be grown by themselves, and not mixed with other plants, for several reasons, the principal being the necessity of a special situation for them, and their dislike to be interfered with. However, the rhododendron, and especially the scarlet varieties of it, may be planted along with ferns as a fitting accompaniment; a climbing rose growing wild, or a single-flowering scarlet thorn, may also be employed with advantage. Before the fronds shoot out in spring I like to see the ground, in large patches, covered with masses of primroses at one spot, masses of snowdrops at another, masses of the wild oxalis at a third, and at other places carpeted with the wild hyacinth. It is not usual for me to mix these flowers together, as masses of colour, such as these flowers afford in their native woods, give variety to the scenery of the garden. . . . ."

"Experience has taught me that ferns like an abundance of light, although it is necessary to screen them from cold winds. For this reason I always contrive that a belt of trees, or of rootwork or rockwork, shall surround my ferneries, and at the same time that the light of the sky may fall upon them from above without their being directly exposed to the fiery rays of the sun.

"My Fern Glade is placed on one bank of the backwater, and is screened from the sun by a row of nutbushes to the south. Here many of the larger varieties of lady-ferns, interspersed with polystichums, broad ferns, mountain ferns, and scolopendriums, are grown. The royal fern flourishes near the river, but it is advisable to keep the crowns well above the water, as their roots like damp soil rather than wet. In the driest spots we grow polypody (*Polypodium vulgare*), and in the wettest the marsh fern (*Lastræa Thelypteris*).

"The Fern Glen is a more elaborate artistic production, affording many delightful little views and growing fine ferns. The whole is well sunk into the ground, with little rivulets running through, affording one or two boggy places. It is protected on the north by a bank, with a hedge interspersed with trees, and on the south by trees. A large willow-tree (*Salix alba*) on the south-west shades the sun's rays, but still there is ample sky light overhead, which I find so desirable for the growth of all ferns. . . .

"My Fern Glade has given me so much pleasure, that I strongly advise every one who has a waste piece of land near his garden to make a fern glen. It will be a pastime in the winter evenings to design it; the construction of it—the transforming of the ideal conception of the mind into a living reality—will afford much pleasure; many a country trip in the woods will be required to furnish it; and when furnished it will afford a spot for contemplation and enjoyment, in which the designer may fancy that the robins, warblers, and nightingales, which never fail to dwell there, are pouring forth their gratitude for the construction of such a delightful retreat.

"My Valley of Ferns is another spot in which I greatly delight. It has a stream through the centre, and it is well surrounded by trees. Here two or three varieties of male ferns and of polystichums attain their highest perfection. The magnificent *Struthiopteris* raises its graceful and delicate fronds in the early spring, and shows its finely-coloured foliage when dying down in the early autumn.

"In the heat of summer the beauty of a great mass of ferny foliage, such as this place affords, cannot be surpassed. The success of this valley of ferns appears to be due to the protection afforded from cold winds by surrounding trees, whilst the plants themselves luxuriate under light and sunshine, with free exposure to air without draught . . .

"At this moment I have nearly every British fern growing out of doors, but I could never succeed in cultivating the *Asplenium marinum* in that situation. This fern grows wild by the sea-coast as far north as Aberdeen; nevertheless I have never been able to grow one in any of my outdoor ferneries. It is a remarkable fact that the *Adiantum Capillus-Veneris* (Fig. 9) has never proved to be hardy with me, although I have it now growing well in the Fern cave. I have seen it along the Mediterranean coast beyond Mentone, but only in particular situations, such as on a bed of sandstone, which is permeable by water; in this situation the fronds were severely frosted in winter. I saw a plant growing at the top of the Cathedral at Genoa, at a time when all the fountains in the city were frozen. I noticed it again to be plentiful at Pompeii and at Herculaneum, and also in the ruins of Nero's Palace at Rome. But nowhere was the maidenhair seen in such perfection as in the ruined amphitheatre at Posilippo, near Naples. The underground rooms and passages formerly used by the gladiators, and for the working machinery of the amphitheatre (which is the most perfect of any now existing), forms a series of caves, through the walls of which moisture continually oozes, and here the maidenhair luxuriates in all its glory. Some of the fronds were eighteen or more inches in length, and the earthen walls were covered with sheets of this lovely fern, standing out at right angles from the wall or hanging down from the roof. I must confess that, when I beheld this great and glorious sight, I was more impressed with it than with the thought that I was present on a spot where dramas of blood were enacted centuries before. I speedily collected a number of plants, to the no small disgust of the *cicerone* who could not do the amphitheatre at his usual gallop, and who shrugged his shoulders at my utter want of taste in gathering useless weeds. Some of these plants now grow at my garden in the Fern cave. The *Adiantum* is said to luxuriate in the orange groves in Spain, in which country the fronds are used to make the syrup of capillaire, a pleasant beverage drunk mixed with water in hot weather."

Probably few are aware that in the clefts of that remarkable formation known as the "limestone pavement," in the west of Co. Clare, Ireland, the maidenhair fern may be gathered flourishing with a luxuriance of growth fully equal to that here described in Italy.

Mr. Smee's "My Garden" is, indeed, a book which ought to be in the hands of every one who is fortunate enough to possess a garden of his own; he is certain to find some things in it from which he may profit. The mode in which the work is got out—the paper, the printing, and the binding—leaves nothing to be desired. The style of the larger illustrations may be judged from the sample we have given; the smaller figures are in most instances equally good, the botanical ones from the pencil of Mr. Worthington Smith, and they abound on almost every page. Some of them might certainly have been spared, as, for instance, the drawings of Mr. Smee's garden roller and water-pot, which do not appear to differ essentially from similar instruments which might be found in any other garden. But these are small defects, which scarcely depreciate from the value of the work in helping to promote among the inhabitants of our great cities a healthy love of country pursuits, and of the study of Nature herself. A. W. B.

NOTES

SIR JOHN LUBBOCK, Bart., M.P., F.R.S., has been elected Vice-Chancellor of the University of London in the room of Sir Edward Ryan, who accepted the office for a year only on the death of the late Mr. Grote. We may congratulate ourselves that by this appointment the interests of Science will be well looked after in the future career of the metropolitan University.

DR. H. W. ACLAND, Regius Professor of Medicine, and Dr. G. Rolleston, Linacre Professor of Physiology, have been nominated to represent the University of Oxford on the Committee of Reference for the Medical Examining Board for England.

AT the examination for the degree of D.Sc. just concluded at the University of London, the following gentlemen passed to the satisfaction of the examiners:—In Branch VI.—Electricity, Alexander Muirhead; in Branch IX.—Animal Physiology, Henry Newell Martin, M.B.

THE following despatch from Dr. Kirk respecting Dr. Livingstone, dated Zanzibar, May 2, is published in the *Times of India*:—"Letters have been received from Unyanyembe, and Mr. Stanley is now within a few days of the coast on his return, having with him a large box full of correspondence and papers. Arabs state that Dr. Livingstone has visited Uvira, and found the River Rusiri flowing into the lake; but on this point we must await Dr. Livingstone's own report. He then returned to Ujiji, and was met by Mr. Stanley. Dr. Livingstone has gone to Unyanyembe, where he remains. He is in good health, and intends further explorations south after arrival of additional stores from the coast. Two of the Nassick boys who started some years ago with him are in his company, and he is said to be still using the sextant, and taking observations regularly. All his letters are in Mr. Stanley's care." Another despatch, published in the *Bombay Gazette*, dated Zanzibar, May 3, says:—"Dr. Livingstone has reached Unyanyembe. At Ujiji he was met by Mr. Stanley, who is expected daily at Zanzibar, having in his hands a large case of correspondence that will fully explain Dr. Livingstone's recent travels, embracing the north end of the Tanganyika Lake, and solving the Nile problem. Dr. Livingstone is said to remain still in the Unyamwazi country, and to intend further discoveries to the southward on the receipt from Zanzibar of additional supplies and stores. The Livingstone Relief Expedition is now on the African coast ready with all the necessaries for African travel, and Dr. Livingstone's son forms one of its members."

IT is with regret that we have to record the death, on the 27th ult., of Mr. Charles Hill, at his residence, Cotham Grove, Bristol. The deceased gentleman was in his 78th year, and for a long time had taken much interest in astronomical matters. Mr. Hill was in the possession of an excellent observatory, and his scientific instruments were of the first order.

AT the provincial meeting of the Horticultural Society, held at Birmingham last week, Prof. Thiselton Dyer read an address on the bearing of recent scientific investigations on horticulture.

IN his third and concluding lecture at the College of Surgeons, Prof. Humphry continued his exposition of the morphology and homology of the muscles of the limbs in man. Though arranged in accordance with the movements of the several joints of the limbs upon the flexor and extensor aspects, yet the extensor muscles not unfrequently incline on the sides of the joints, from the extensor to the flexor aspects, and so, irrespective of their nerve-supply, acquire a flexor action. This he believed to be the case with the biceps flexis cruris, which he has shown, from the anatomy of the Cryptobranch in the last number of the *Journal of Anatomy*, to be a derivative from the extensor mass in the thigh. Occasionally, also, as in the case of the *lumbricales*, flexor muscles acquire an extensor action. He spoke of the tendinous intersection in the semitendinosus as being the representative of a similar intersection in the Cryptobranch found at the junction of the caudal with the femoral muscle; and believed it to be present in man for the purpose of dividing the fibres of this muscle, which are of unusual length in consequence of the range of action required of them by the insertion of the tendon at a greater distance from the centre of motion at the knee than the other hamstrings. The ulnar origin of the pronator teres is a representative of the pronator intermedius of reptiles. It is found in the chimpanzee as well as in man, and serves to carry on pronation of the forearm in the flexed position of the elbow in which the remainder of the muscle is relaxed. In like manner the accessorius in the foot serves to maintain the flexion of the toes, while the flexor digitorum is relaxed by the bending of the ankle. This muscle has no representative in the upper limb of man, or in the fore limb of mammals, but it is well represented in the fore limb of Saurians. The great difference in the construction of the two limbs is caused by the pronation and supination in the fore arm and hand, and by the projection of the heel; and the representatives of the muscles, which in the upper limb affect the movements of pronation and of flexion of the fingers, are in the lower limbs, to a considerable extent, concentrated upon the knee. In instituting the comparison between a hand and foot, the Professor observed that it is necessary to eliminate from the consideration those features which ordinarily distinguish the one limb from the other; and this has not been done with sufficient care by some of those who have recently discussed the subject. We should first determine the points in which the hand of man differs from the terminal part of the ordinary fore limb of a mammal; and we may apply the term "hand" to the terminal part of either limb which presents a corresponding modification. This may take place in the hind limb, although the heel may project, and though a peroneus longus muscle, and a short flexor, and a short extensor of the digits may be present. These have been insisted on as the anatomical features of a foot as distinguished from a hand. But in reality they are rather the features of a hind limb as distinguished from a fore limb; and their presence is quite compatible with a modification of the hind limb corresponding with that modification of the fore limb which constitutes the hand of man. Judged in this way the terminal part of the hind limb of a gorilla or a chimpanzee has as much claim to be called a hand as has the terminal part of the fore limb in the same animals. Hence the term "quadrumanous" is, on anatomical ground, fairly applicable to these animals and their allies, as there is no sufficient reason to deny to man the prerogative of special subdivision of labour in the parts of his frame, and, consequently, of high organisation which is significantly implied by the term "bimanous." These lectures are in course of publication in full in the *British Medical Journal*.

THE concluding excursions of the Geologists' Association for the present season will be as follows:—Monday, July 8, excursion to Walton-on-the-Naze; director, Prof. Morris. Saturday, July 13, visit to the International Exhibition; director, Prof.

Tennant. Monday, July 22, and five following days, excursion to Ludlow and the Longmynds; directors, Prof. Morris, Mr. Robert Lightbody, and Rev. J. D La Touche. This excursion is intended to afford members an opportunity of studying Silurian and Cambrian Strata, and will be of great value to students of the Palæozoic Rocks. On Monday, after arrival at Ludlow, the party will examine the exposures of the Ludlow group of rocks in the immediate vicinity of the town. On Tuesday the objects of attraction will be the outcrop of the Upper Ludlow on the road to Wigmore, the Aymestry Limestone, and the Lower Ludlow. After passing Elton Hall, examine fine and Fossiliferous Section of the Lower Ludlow, at Evenhays (Vinal Ridge). Proceed then to Burrington (Wenlock Shale), Bank of the Teme (good section of Wenlock Shale), and Church Hill Quarry, Leintwardine, famed for Echinoderm remains. On Wednesday, leave Ludlow for Hayton's Bent, where the Old Red sandstone is exposed, and may be examined with advantage. On Thursday, proceed by Bishop's Castle Railway for the south end of the Longmynds, where the Wenlock Shales will be examined, and the physiography of the district formed by the Llandeilo Rocks observed. Walk back through the Valley of the Onney, observing on the way sections of Silurian Rocks from the Lower Caradoc to the Wenlock. On Friday, proceed by rail to Lydham Heath, thence to Shelve (Lower Llandeilo). Walk over the Stiperstone and Longmynd Ranges to Church Stretton, and examine junction of Cambrian and Silurians. On Saturday, from Craven Arms walk to Norton Camp, observing by the way Wenlock and Ludlow Sections, and at Norton inspect a good exposure of the "Bone Bed." Should time permit, proceed to Onibury, and inspect junction of Silurians and Devonians, and a fossiliferous section of Lower Ludlow Shale.

A NEW work on Electrostatics by Sir William Thomson will be published by Messrs. Macmillan and Co. early in August. It will consist chiefly of articles which originally appeared at different times during the last thirty years, in the *Cambridge Mathematical Journal*, the *Cambridge and Dublin Mathematical Journal*, Liouville's *Journal de Mathématiques*, the *Philosophical Magazine*, Nichol's Cyclopædia, the Reports of the British Association, the Proceedings of the Royal Societies of London and Edinburgh, of the Royal Institution of Great Britain, and of the Philosophical Societies of Manchester and Glasgow; and which will now be first collected and published together. The rest, constituting about a quarter of the whole, will be printed from manuscript, which, except a small part about twenty years old, entitled "Electromagnets," has been written for the present publication, to fill up roughly gaps in the collection.

THE two modes of photographic printing known as the Albert and Woodbury processes have both been employed by Prof. Agassiz in the illustration of his forthcoming "Revision of the Echini." Prof. Agassiz has kindly permitted us to see specimens of both modes of illustration. They are simply exquisite, portraying every marking on the surface of the shells with the accuracy of nature printing, and with the beauty of a lithograph. The practical working of these new modes of photographic printing must speedily work a complete revolution in the illustration of works on Natural History.

A VALUABLE report, prepared by Mr. R. D. Cutts, of the United States Coast Survey, upon commerce in the products of the sea, has just been published by the Senate, and is considered a valuable contribution to the statistics of the fisheries of America and of the rest of the world. In this the different marketable products are described in detail, and the relative rank which they occupy in commerce indicated. In addition to this, there is given the area, population, most important ports, and commercial tonnage of the principal nations of the world; the imports and exports of the products of the sea; showing the capacity of the markets and the countries supplied; and also the catch, consumption, and balance of trade, from official statistics. This Report was prepared in 1869, by request of the Secretary of State, and transmitted in February of that year, but the order to print was not made till recently.

THE Report of the Government Cinchona Plantations at Ootacamund in India for 1870-71 states that the growth of the plants has been very satisfactory. The older shrubs have grown into trees 22ft. to 23ft. high, and 18in. to 21in. in girth. Of the *Cinchona succirubra* the finest samples reach a height of 30ft., with a girth of 3ft. Among the new species of plants lately introduced is the Pitayo bark, which appears hardy and well suited to the climate. During the year 51,353lbs. of fresh bark were supplied to Mr. Broughton, the Government quinologist, for the manufacture of amorphous quinine. From 1,000 eight-year-old plants of the *Cinchona succirubra*, as much as 2,560lbs. may be expected to be extracted this year. This average of more than 2½lbs. to each tree will yield at the present rate of 2*s*. 8*d*. to 3*s*. per lb., a clear profit of at least 2*s*. per lb.

PROF. LEIDY, at a meeting of the Philadelphia Academy of Seciences, on February 6, exhibited specimens of corundum from Macon County, North Carolina, which, he said, were especially interesting, as they consisted of fragments of large crystals of gray corundum, containing in the interior dark blue sapphire, and coated on the exterior with bright red ruby. One pyramid of a large crystal from the same locality recently brought to that city weighs 300 pounds.

AT the meeting of the American Fish-Culturists' Association, held at Albany, it was voted to present to Congress a memorial for aid from Government in stocking the rivers of the United States with useful food fishes. Mr. George Page Shepherd, of New Jersey, was appointed to present the memorial, and had an opportunity not long since of expressing his views before the House Committee on Appropriations.

ACCORDING to Dr. Uhler, of Baltimore, the European cabbage butterfly (*Pontia Brassicæ*), the pest of the agriculturist, has reached Baltimore in its invasion of the United States. It has been known for some years more to the eastward, and has been slowly but surely creeping along, until it bids fair to involve the whole country in its ravages.

WE reprint the following interesting note from *Harper's Weekly*:—"As is well known, grouse, pheasants, ptarmigans, and some other gallinacea have a red patch or wattle above the eye, this being so conspicuous in some species as to resemble a piece of red flannel. This has been lately subjected to a careful analysis by Dr. Wurm, who ascertains that it contains a new organic colouring material, which he calls *Tetronerythrin*, or grouse red. It seems to lie in the deeper strata of the epidermis, like the colouring matter of the human skin, and to be partly dissolved in the deep layers of the cells, and partly to common with the colouring matter of the blood. The fact has been well known to hunters that if a white cloth be rubbed over this red process the colour will come off."

THE *New York Journal of Commerce* gives circulation to a story of an extraordinary fall of fish-bones (?) in Louisiana, covering the ground over a considerable area.

LATE Chilian papers announce the discovery of important mines of coal in that country, especially along the Gulf of Aranco, near the mouth of the Carampangue River. According to an official report, one of these veins is five feet thick, and is estimated to contain four million tons of coal.

THE SCIENTIFIC RELATIONS OF GERMANY, FRANCE, AND ENGLAND

THE following extracts from M. Berthelot's recent excellent article in the *Temps*, on the Relations which should exist between Germany and France, are taken from a recent number of the *Pharmaceutical Journal*:—

We know that modern civilisation depends upon three nations, which should at all times and at any cost remain united—namely, France, Germany, and England, each with its peculiar genius and its share in the historic development of the human race. From the seventeenth century each of these nations has taken an active and prominent part in the progress of science.

To speak first of physical and mathematical sciences. Though the initiative was due principally to a few men of other countries—Galileo, an Italian, and Copernicus, a Pole, being the founders of modern astronomy and mechanics—yet the development of these sciences was concentrated chiefly in France, Germany, and England. In France, Descartes discovered the methods of geometric analysis, which have proved more durable than his philosophical and cosmogonical theories. In Germany, Kepler invented the laws of planetary movement; and Leibnitz, who by education and the clearness of his conceptions was perhaps more French than German, laid down the rules of the differential calculus under a form in which they still exist amongst us. At the same time, England produced Newton, greater, perhaps, in the science of nature than either Descartes, Kepler, or Leibnitz; for Newton discovered both new methods of calculation and the laws of astronomy, and since his time we have scarcely done more than develop his ideas and doctrines in studying the movement of the stars.

This same concourse of the three great nations of modern times is seen also in the foundation of chemical science, which in the present day plays so important a part, whether it be in the theories relative to atoms and the constitution of matter, to the formation of stars and of the successive layers of the terrestrial globe, to the origin of life itself; or, on the other hand, in the applications of human industry, dealing with metals, colouring matters, remedies, agriculture, and manufactures.

Towards the end of the eighteenth, and at the commencement of the nineteenth centuries, chemistry was established upon a durable basis, after having floated during nearly two thousand years amongst mystical, obscure, and incoherent notions. It was a Frenchman, Lavoisier, who fixed these indecisive ideas, by the definite principle of the stability of matter, invariable in the nature and weight of its simple bodies. Perhaps, as has been asserted, Lavoisier did not discover any particular fact; but, according to Aristotle, principles and causes are things which are of more scientific importance, for by them we arrive at other knowledge. Now Lavoisier discovered the fundamental principle of chemistry; the science dates from him.

Is this saying that Lavoisier divined all, perceived all, traced for all time the plan of chemical science? Not at all; no more than that Newton alone founded astronomy. For this the inevitable concourse of the great nations was required. Whilst Lavoisier published his immortal researches, the English Priestley and Cavendish discovered the principal gases and the nature of water—inventions that were seized immediately by Lavoisier to support his theory. The Swedish Scheele brought also his precious contingent to the common work. Some years afterwards, an Englishman of genius, Humphry Davy, completed the edifice by the discovery of the alkaline metals, which he obtained by the application to chemical decompositions of the pile recently discovered by a great Italian, Volta.

Germany equally marked its place in the foundation of the new science. It was in the law of numbers that its work was principally characterised: Richter, Wenzel, and the great Berzelius (a Swede) established the law of chemical equivalents, that is to say, a law as general and as absolute in chemistry as the law of Newton in astronomy. It is remarkable that the part of the Germans in this discovery has been principally experimental and practical, contrary to the opinion generally received of German genius. On the contrary, the atomic theory, properly so-called, of a character more abstract and more litigious, is due to an Englishman, Dalton; whilst its demonstration by the physical study of the gases has been accomplished by a Frenchman, Gay-Lussac. This shows that the geniuses of the European races are not so different as has been asserted. Give them a common and equally high culture, and from each will proceed inventions equally original.

This conjunction of Germany, France, and England is to be seen in every great epoch in the history of modern science. The demonstration could be carried down to the present time, proving that neither of these three nations has degenerated from its past: the doctrine of substitutions, the theory of the ethers, that of the polyatomic alcohols, dissociation, the idea of organic ferments, the methods of synthesis of organic principles, have been principally established by French discoveries; the theory of the radicals and that of the polyatomic elements are rather to be attributed to German discoveries; whilst the electro-chemical theory and the method of double decompositions have been invented in England. Finally, the great doctrine of the equivalence of the natural forces, more particularly designated under the name of the mechanical theory of heat, was first discerned by a German, Mayer, and an Englishman, Joule. Developed afterwards by a German mathematician, it has been established in chemistry principally by the experiments of French, English, and Danish scientific men. But it would not be wise to dilate upon the science of the present day; we are too near to it, and are too much engaged in it, for any estimate to escape suspicion of partiality.

In looking back over this short sketch of the progress of the science with which I am best acquainted, I would not ignore the part of Italy, which in the past was so great (may it resume its importance in the future!), nor that of the United States, nor of Russia. But, I repeat, the initiative of the ideas and discoveries has rested for more two centuries in the bosom of three nations—English, French, and German. Their union and their reciprocal sympathy is indispensable, under the penalty of a general loss to civilisation.

INSTRUCTIONS FOR PREPARING BIRDS' EGGS*

I WISH to say a few words for the benefit of those engaged in collecting oological specimens.

Twenty years ago all eggs were blown with two holes—one at each end, and until within ten years most eggs have been emptied with two holes as above, or at the side. Very many of the eggs which I now receive in my exchanges are similarly prepared. At the present time no experienced collector ever makes but one hole to remove the contents of the egg, using a blowpipe in some form to accomplish this object. The following rules should invariably be followed:—

1. Prepare your eggs neat and clean. There is no excuse for having a dirty set of eggs where water, soap, and a tooth-brush can be found. Some eggs will not bear washing, as the shell is so calcareous that the characteristic markings will wash away. There are, however, but few of this class, and I believe this peculiarity is confined to the water-birds. You can see it in any of the species (Smithsonian Catalogue) from 615 to 628 inclusive, and also in the eggs of the Grebes and Flamingo, and some others. Having once seen it you will never mistake it for anything else.

2. Make but one hole, and that a small one in the middle of the egg; cover this hole, when the contents are removed, and the specimen is dry, with gold-beater skin or the paper number indicating the bird. Use an egg drill or a pointed wire of four or six sides to make the opening.

3. If the blowpipe does not readily remove the contents of the egg, inject water and shake the specimen thoroughly, then blow again, and repeat the operation until every particle of the egg is removed.

4. If the embryo is too far advanced to remove through a moderate sized hole, blow out what you can of the liquid part and fill the egg with water, wipe it dry and put it away in a covered box in some warm place, and every twenty-four or forty-eight hours shake it well and remove what you can, and then refill with water. Repeat this operation several times, and after a few days the contents will become sufficiently decomposed to take away.

5. After removing the contents of any egg, cleanse the shell thoroughly. Fill it with clean water and shake vigorously, blow out the contents and repeat the operation until the specimen is perfectly clean. This is particularly desirable in white eggs, as black spots will show through the shell after a time if the least particle of the egg or blood stains remains inside.

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* By William Wood, M.D. Reprinted from the *American Naturalist*.

6. Save all your eggs in sets—that is, keep all the eggs each bird lays by themselves. This is the only way to form a correct knowledge of the eggs of any species, as a single egg, particularly of the blotched ones, frequently gives a very erroneous idea of the general markings—a very unsatisfactory representative of a set. For instance, in my collection are four eggs of the *Buteo lineatus*, found in the same nest, two of which are pure white and two blotched. It is not very uncommon to find great variations in markings in the same species and in the same nest.

7. Keep a memorandum of the place and date of collecting each set of eggs.

8. Use some kind of blowpipe in preparing your eggs for the cabinet. The common blowpipe, with the addition of a fine pointed tip, will answer; yet it is a severe tax on the lungs and brain if you have many eggs to blow. I have many a time been dizzy and almost blind from overtaxing my lungs in this operation. Within a few years Mr. E. W. Ellsworth, of East Windsor Hill, Conn., has invented a blowpipe which is operated by the thumb and finger, which works very perfectly and expeditiously. I would not be without it on any account. After using it for a time, and then letting it remain unused until the leather packing becomes dry, the instrument does not work satisfactorily to those unaccustomed to it. The remedy is simple. Take off the blowpipe and work the instrument submerged in a bowl of warm soap suds, when the leather packing becomes pliable and works as well as new. I have used the same instrument for years, and it works to-day as well as when new, by following the above directions. The printed directions which accompany each instrument are intended to be a sufficient guide in case repairs are needed, and the maker can be referred to for any further information required.

SCIENTIFIC SERIALS

THE *Geological Magazine* for May (No. 95) opens with an important article (illustrated with a plate) on some coniferous remains from the Lithographic Stone of Solenhofen by Mr. Dyer, in continuation of a former paper on the same subject. This paper includes the description of a new species of *Pinites* (*P. solenhofenensis*), a revision of the genus *Athrotaxites*, with descriptions of two new species (*A. longirameus* and *A. ? laxus*), and a notice of a new genus, *Condylites*, probably belonging to the Cupressine group, and including a single new species, *C. squamatus*.—MM. H. B. Woodward and J. H. Blake communicate a valuable paper on the relations of the Rhætic beds to the Lower Lias and Keuper formations in Somersetshire, in which they cite additional evidence and arguments in support of the view that the Rhætic beds constitute true passage-beds between the Keuper and Liassic series.—Principal Dawson gives us the results of a new examination of the geological structure of Prince Edward's Island in the Gulf of St. Lawrence; and Mr. Alfred Bell a paper on the succession of the Crags, the latter containing a criticism of Mr. Prestwich's recently-published memoirs on the same subject.—Mr. James Geikie communicates a sixth paper on changes of climate during the glacial epoch, and the number concludes with an interesting lecture on meteorites by Mr. David Forbes.

THE *Monthly Microscopical Journal*, No. 42, for June, completing the seventh volume, contains communications on "An Improved Reflex Illuminator for the Highest Powers of the Microscope," by W. H. Wenham; on "A Silvered Prism for the Successive Polarisation of Light," by J. W. Stephenson; "Structure of Battledore Scales," by J. Anthony, M.D., detailing fresh investigations by a new method of of illumination, the results of which confirmed those of a previous communication; "Beale's Nerve Researches: the Reply of Dr. Beale to Dr. Klein;" "On Bog-Mosses," by R. Braithwaite, M.D., part iv., devoted to *Sphagnum tenellum* Ehrh., and its varieties. This is the *S. molluscum* of Wilson's "Bryologia;" "Crystallisation of Metals by Electricity," by Philip Braham; "On the Means of Distinguishing the Fibres of New Zealand Flax from those of Manilla or Sizal, by the Microscope," by Captain Hutton. The average length and diameter of the ultimate fibres are held to be distinctive, as well as some other less important points, in the discrimination of these fibres. The residue of this number is occupied, as usual, with brief notices of new books, notes on microscopical subjects, and the proceedings of microscopical societies.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 19.—Prof. Ramsay, V.P., in the chair.—The following communications were read:—1. "On *Trochocyathus anglicus*, a new species of Madreporaria from the Red Crag," by Mr. P. Martin Duncan, M.B., F.R.S. The author described a coral, of which a single specimen had been found in the Red Crag, in the grounds of Great Bealings Rectory, Norfolk. He stated that it belonged to the genus *Trochocyathus*, and was distinguished from the other species of that genus by its dense epitheca, its small and prominent columella, and its inverted calicular margin. He proposed to name it *Trochocyathus anglicus*, and stated that its nearest alliance is with the Australian Upper Tertiary form described by him under the name of *T. meridionalis*. Mr. Prestwich inquired whether the fossil bore any resemblance to any of the French Eocene forms, and whether there was any possibility of its being derivative. Prof. Duncan replied that the specimen was but little worn, and was therefore probably not *remanié*, though this point was not absolutely certain. 2. "On the Discovery of Palæolithic Implements in association with *Elephas primigenius* in the High-terrace Gravels at Acton and Ealing," by Colonel A. Lane Fox, F.G.S. The gravels in the neighbourhood of Acton have been divided by Mr. Prestwich into two principal groups—viz., the high-level gravels on the hills above the valley, and the valley-gravels on the sides and bottom of the valley itself. The valley-gravels have been again divided by Mr. Whitaker into three terraces—viz., a high terrace, between 50ft. and 100ft. above the Ordnance datum, a mid terrace, between 20ft. and 40ft. high, and a low terrace, at an average height of 10ft., occupying the low ground in the bends of the river. On both sides of the river the high terrace is separated from the mid terrace by a strip of the London Clay, which is laid bare at an average level of 50ft. The London Clay is also laid bare on the sides of the tributary streams running into the valley on both sides of the river, thus dividing the high-terrace gravel into patches. The mid terrace is continuous, and follows the sinuosities of the valley on both sides up to the strip of London Clay. The author accounts for this distribution of the gravels by supposing that a large body of water must at one time have stood at the 50-feet level, and the denudation of the high terrace have been caused by the waves beating on the sides of the valley, and by drainage into this body of water. The mid terrace he conceives may have been caused in part by accumulations beneath this body of water. The position of the high-terrace gravel at Acton corresponded so closely to that of the implement-bearing gravels of the Somme and the Ouse that the author was led to examine carefully the excavations made in it for the construction of houses. He discovered a number of implements of the drift-type, together with flakes and cores, and a few roughly-formed scrapers; all these were found in close contact with the London Clay, and beneath the gravel. Fragments of fern (*Osmunda regalis*) and of wood (*Pinus sylvestris*) were also found with the implements at the same level. Two implements were found at Ealing Dean, two miles westward, on nearly the same level as those at Acton—viz., 90ft.; and these also came from the bottom of the gravel. Another implement was found south of the river at Battersea Rise, in the same position above the strip of London Clay as at Acton, and at about 60ft. above the Ordnance datum. The implements are of the pointed and oval types. The only animal remains discovered in the high terrace consisted of a tooth of *Elephas primigenius* in the Acton gravel. The position of this the author believes to be reliable, although he did not discover it himself *in situ*. In the mid-terrace gravel a number of pits were examined between Shepherd's Bush and Hammersmith, and in the neighbourhood of Turnham Green, which resulted in the discovery, at the latter place, of a large quantity of animal remains (noticed by Mr. Busk in the following paper), all of which, like the implements of the high terrace, were at the bottom of the gravel; but no evidence of human workmanship was found in the mid terrace. All these were found together, in the same seam of gravel, 12ft. beneath the surface, and all appeared to have been deposited at the same time. The surface was here 25ft. above the Ordnance datum, and consequently about 50ft. lower than the implements of the high terrace, 1½ mile to the north. The section across the valley, taken through the two places, here shows the strip of the London Clay intervening between the two terraces. The chief points of interest which the author submitted to the judgment of geologists consisted in the presence of drift implements in the high terrace, their absence in the mid terrace, and reappearance in the existing bed of the Thames; the great rarity or absence of animal remains in the high terrace, and their abundance in the mid terrace, and the occurrence of both implements and animal remains at the bottom of the gravel in both terraces. The writer concluded by adducing proofs of the great antiquity of the present river-bed, which it was shown must have run in its present meandering course in the bottom of the valley for at least 2,000 years. 3. "On the Animal Remains found by Col. Lane Fox in the High and Low-level Gravels at Acton and Turnham Green," by Mr. George Busk, F.R.S. The author described the mammalian bones referred to in the preceding paper. The remains from the high-level gravels at Acton belong to the genera *Bos*, *Ovis*, *Equus*, and *Elephas*? The greater part belong to the first-named genus, and are probably modern, as are also those of *Ovis*. The remains of *Equus* may be of greater antiquity. The other bones found may belong either to Elephant, Rhinoceros, or Hippopotamus; they include a large portion of an Elephant's molar, and are much rolled. The remains from the mid-level gravel at Turnham Green generally present the characters of great antiquity. They include bones of *Rhinoceros hemitœchus*, *Equus caballus*, *Hippopotamus major* (one of them the left frontal of a very young animal almost unworn), *Bos* (probably *B. primigenius*, and some perhaps *Bison priscus*), *Cervus* (*C. clactonensis*, Falc. = *C. Browni* Dawk., *C. elaphus*, and *C. tarandus*), *Ursus ferox priscus*, and *Elephas primigenius*. Mr. Prestwich complimented the author on the exactness and completeness of his description of the classical district which he had investigated, in which mammalian bones had been found and described by Mr. Trimmer so early as 1815. In that case Hippopotamus remains, very fresh and unworn, had also been discovered. Prof. Morris had also described a deposit near Brentford in which numerous remains of Reindeer were present, showing how variable was the distribution of mammalian remains even in a limited area, and how unsafe it was to base theories upon merely negative evidence. It was to be hoped that other investigators would extend similar discoveries to other parts of the valley of the Thames. Mr. Godwin-Austen did not think that the presence of the young Hippopotamus was absolutely conclusive of its having been born in this country. With regard to the presence of remains of Reindeer and Hippopotamus in the same beds, not only might there have been an overlapping of fauna such as has been pointed out by Sir Charles Lyell, but there also might be an intermingling of the included remains from two beds of different ages. He was not altogether satisfied with the evidence as to the coexistence of man with *Elephas primigenius*, nor as to the artificial character of some of the presumed implements. He did not attach any great importance to the merely fragmentary bones. Mr. Evans maintained that the implements exhibited were of necessity artificial, and commented on the nature of the evidence as to the coexistence of man with the Pleistocene fauna. Under any circumstances the gravels containing the implements could only have been deposited at a time when the Thames valley had not been excavated to anything like its present depth; and they were therefore of great antiquity. There was, moreover, a notable absence in them of a number of the animals usually found associated with Neolithic implements; and if man had not subsisted on the animals the remains of which were found associated with his handiworks in the gravels, it was a question on what food he had had to depend. The absence of implements in the low-level gravels seemed to him significant of a diminution in the number of the human beings who frequented the banks of the river. Mr. Carruthers said that as the rhizome, whether it was that of *Aspidium* or *Osmunda*, was an aerial, and not a subterranean rhizome, it must have been carried to its present position; and it consequently indicated, as Col. Lane Fox had pointed out, the direction of the stream. Mr. Flower regarded Col. Lane Fox's memoir as of great interest, as affording an additional instance of that perfect similarity of these deposits, whether in France or England, which in places so wide apart might reasonably be taken to indicate a common origin. It was indeed generally assumed that these deposits were brought down by rivers; but this, according to his view, was by no means certain. Col. Lane Fox had described the valley as 4½ miles wide; but there was at Croydon, 12 miles distant, a deposit of gravel capped with loess, containing elephant remains, and exactly resembling the Thames valley-gravels, and communicating with them. This evidently formed part of the Thames valley system, whatever that system might be taken to be; and if so, he thought it incredible that the loess should have been distributed by river-action over an area 12 or 15 miles in width. In conclusion, he was quite content to adhere to the opinion held by the French geologists, and formerly by several of our own most able writers, that the distribution of these superficial drifts was in the first instance diluvial rather than fluvial. Col. A. Lane Fox, in reply, pointed out the artificial character of the implements, and the manner in which the mammalian remains occurred. He thought that the lower terrace of gravel might have been formed at the bottom of a lake. Mr. Busk, in proof of the animal remains not having been brought from a distance, showed that the remains of the same animal were found in close proximity to each other. Prof. Ramsay made some remarks on the undoubtedly artificial character of the implements, and on their position at the base of the gravels. The origin of the Thames valley he had already maintained to be of Postmiocene age; and though there was at present no evidence of man's existence at that time, it was still possible. Of the extreme antiquity of the human race there could, however, be no doubt. 4. "On the Evidence for the Ice-sheet in North Lancashire and adjoining parts of Yorkshire and Westmoreland," by R. H. Tiddeman. The country of which the earlier glacial phenomena were described in this paper lies between the Lake-district on the north and the plains of South Lancashire and Cheshire on the south, and extends from the great watershed of England to the Irish Sea. On the west is a seaside plain rising to levels of less than 200 feet. On the north-east is a portion of the Pennine Chain, comprising Ingleborough, Pennigent, and other Fells, rising to heights of from 2,000 to 2,400 feet. Between these, from north to south, we pass over (1) a range of moorlands from 1,000 to 1,500 feet high, called the Rossendale Anticlinal, which forms the watershed between the basins of the Mersey and the Ribble; (2), the valley of the Burnley and Blackburn Coal-field, which drains north through gorges in (3) the Pendle chain of hills into (4) the broad valley of the Ribble; (5), a group of Fells rising to a general level of 1,800 feet, between the valleys of the Ribble and the Lune, called, for the purpose of this paper, "The Central Fells;" (6), north of this the valley of the Lune and the estuary of the Kent. The main direction of all these features between the seaside plain and the Pennine Chain, is from north-east to south-west. The paper was illustrated by a map of the district on the scale of 1 inch to a mile, coloured to represent elevations, the level contours having been reduced from the 6-inch scale. Upon this all the ice-scratches found on the solid rocks were inserted. A diagram illustrating the proportional number of scratches in different directions showed that 20 per cent. of them were due south, although the general direction of the valleys was to the south-west. An instance was mentioned of a ridge of 1,400 feet in height, which had scratches at the top running directly across it to the south, although no land of equal height occurred north of it within a distance of seven miles. A similar instance was shown to exist on the ridge north-east of Pendle Hill. A *roche moutonnée* in the gorge of the Calder at Whalley was shown to have been formed by ice working from the north, although the river drains from the south. Other systems of scratches were mentioned in detail. All these tended to show that, though the general slope and drainage of the district is to the south-west, the movement of the ice at the period of maximum cold was to the S. or S.S.E., or nearly parallel to the watershed. The author goes on to describe certain disturbances at the surface of the rocks, which are dipping at high angles to the south, they having been overturned by some force coming from the north. Such surface-disturbances are not found on rocks dipping to the north; and this fact may be explained by an illustration: in one case the brushing was with the nap, in the other against it. In was shown that these phenomena could not be attributed to any other agent but a great ice-sheet pushing on from its northern gathering grounds, recruited by the greater elevations on its course, but overriding the lesser, grinding down and smoothing by its friction rocks presenting but a gentle incline, tearing up and turning over the basset edges confronting its approach. The author next described the arrangement of the Till as to colour and material, and endeavoured to show that all the facts which he has observed are in favour of the existence of an ice-sheet travelling south in this district. Mr. Cumming's observations in the Isle of Man were considered to confirm these views. He describes the general glaciation of the island as being from the E.N.E. or Lake-country, and describes many large blocks of granite which had been carried from their parent rock up the high hill of South Barruh and down on the other side. This was referred by Mr. Cumming at the time to a great "wave of translation;" but the facts are quite easily explained by an ice-sheet. Other observations of Mr. Cumming upon the drifts of the Isle of Man were taken by the author as confirmatory of his views. Mr. Morton's observations on the glaciation of the Mersey basin were touched upon; and it was suggested that the glaciation of that district was produced by an ice-sheet, not coming from the south-east, as Mr. Morton holds, but working to the south-east from the Lake-country, and across a part of what is now the Irish Sea. Prof. Ramsay's observations on the glaciation of Anglesey being to the S.S.W. instead of from the Snowdon group, as might be expected, were considered by the author to be confirmatory of his views of a great ice-sheet having filled what is now the Irish Sea, and emptied itself by St. George's Channel on the one hand, and by the Cheshire plain on the other, as well as by some of the passes in the Pennine Chain.—5. "On the Mammalia of the Drift of Paris and its Outskirts." By Prof. Albert Gaudry, F.C.G.S. In this paper the author briefly indicated those mammals the remains of which have been discovered in the Pleistocene or Quaternary deposits of Paris and its vicinity. His list includes flint implements as evidences of the existence of man, and bones of the following species:—*Canis lupus*, *Hyæna crocuta* (*spelæa*), *Felis leo* (*spelæa*), *Castor trogontherium* and *fiber*, *Elephas primigenius* and *antiquus*, *Hippopotamus amphibius*, *Rhinoceros tichorhinus* (a Rhinoceros of doubtful species), *Sus scrofa*, *Equus asinus* and *caballus*, *Bos primigenius*, *taurus* (?), and *indicus* (?), *Bison priscus* and *europæus*, and *Cervus tarandus*, *Belgrandi*, *megaceros*, *canadensis* (?), *elaphus*, and a small species.

Zoological Society, June 18.—Mr. John Gould, F.R.S., vice-president, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of May, 1872. The most noticeable of these were two Argus Pheasants (*Argus giganteus*), presented to the Society by Mr. J. G. Fanshawe.—Mr. P. L. Sclater exhibited a pair of Ceylonese birds sent to him for determination by Mr. W. Vincent Legge. These birds Mr. Sclater considered to belong to a new species of the genus *Prionochilus*, which he proposed to call *P. vincens*, after one of the names of its discoverer.—Mr. Edwin Ward exhibited the horns of a Barasinga Deer (*Cervus duvaucellii*) with twenty points, and a very handsome and peculiarly grown specimen of the Gaur or Indian Bison, from Central India.—Mr. St. George Mivart read a paper "On the Axial Skeleton of the Ostrich" (*Struthio camelus*).—Dr. J. Murie read a paper "On the Cranial Appendages and Wattles of the Horned Tragopan" (*Ceriornis satyra*). After alluding to the phenomena of display during courtship, he went on to show that rudimentary horns are found in the female. In the male the pseudo-horns are composed of firm fibro-elastic substance, and are not due to vascular erection. The wattle, on the contrary, is a true erectile vascular organ.—A communication was read from Prof. H. H. Giglioli, containing an account of the Cetacea observed during the voyage round the world of the *Magenta* in the years 1865–68. In this were contained descriptions of several new or little known species, and of a new genus and species of Fin-backed Whale, proposed to be called *Amphiptera pacifica*.—Dr. J. Murie continued the series of his observations on the Macaques, commenced at the last meeting. The species selected for special notice were *M. arctoides* of Is. Geoff., which he showed to be identical with *M. brunneus* of Anderson:—The Formosan or Round-faced Monkey (*M. cyclopis*), and the Japanese Monkey (*M. speciosus*). Points in the anatomy and skeleton of each of these species were described.—A communication was read from Dr. J. E. Gray, containing the description of the younger skull of Steller's Sea Bear (*Eumetopias stelleri*).—A communication was read from the Rev. O. P. Cambridge, giving descriptions of twenty-four new species of Spiders of the genus *Erigone*.—A second communication from Dr. J. E. Gray, F.R.S., contained additional notes on new corals from the Southern and Antarctic Seas.—A further communication from Dr. Gray contained additional notes on *Arctocephalus cinereus*, and on *Gypsophoca*, from the coast of New Zealand.—Mr. A. H. Garrod read a description of the tongue of *Nestor hypopolius*, which showed that *Nestor* does not belong to the Trichoglossine group of Psittacidæ.

Mathematical Society, June 13.—Mr. W. Spottiswoode, Treas. R.S., president, in the chair. Prof. Cayley, vice-president, gave an account of his paper, "On the surfaces divisible into squares by their curves of curvature." Sir W. Thomson, Mr. Merrifield, and Prof. Clifford, asked for information on one or two points in the communication.—Mr. S. Roberts, vice-president, gave some details of his paper, "Prof. Cremona's transformation between two planes and tables relating thereto."—Reference was made to a paper by Prof. Cayley. "On the rational transformation between two spaces" (Proceedings Lond. Math Society, vol. iii. pp. 127, &c.).—Dr. Hirst briefly sketched out a few results arrived at in his communication, entitled "A manifold correspondence of two planes." Sir W. Thomson explained the object of his short paper "On the simultaneous reduction of two prodynamical quadratics to sums of squares." He referred to papers by Prof. Cayley (Camb. and Dub. Math. Jour., Feb. 1869, and Quarterly Math. Jour. 1858), and to Conaby's problem "Sur l'équation à l'aide de laquelle on détermine les inégalités séculaires des planètes," treated of in vol. iv. of that writer's "Exercises." The Hon. J. W. Strutt referred the author to a memoir bearing on the subject of his paper.

Anthropological Institute, June 17.—Sir John Lubbock, Bart., president, in the chair.—Mr. A. W. Franks exhibited and described photographs of the tattooed man from Birmah—The following papers were read: "On the Hill Tribes of North Aracan," by Mr. St. Andrew St. John; "The Ainos of Yeso," by Commander H. C. St. John, R.N. (communicated by the Admiralty); "Indian Picture Writing in British Guiana," by Mr. Chas. B. Brown; "Report on Australian Languages and Traditions," by the Rev. W. Ridley, M.A. (communicated by the Colonial Office); "Report of the Anthropological Section of the Arctic Exploration Committee."

Linnean Society, June 20.—Mr. G. Bentham, president, in the chair.—Mr. A. W. Bennett communicated a short note on the mode of fertilisation in *Impatiens parviflora*. On the structural peculiarities of the Bell-bird (*Chasmorhyncus*), by Dr. Murie.

Geologists' Association, June 7.—Rev. J. Wiltshire, president, in the chair.—"On the Classification of the Cambrian and Silurian Rocks," by Henry Hicks. The author, after mentioning the groups now known to comprise the Cambrian and Silurian rocks, as exhibited in the British Isles, and the usual mode, hitherto, of dividing and subdividing these formations, stated that it was impossible in a science so progressive as geology, where new discoveries were continually being made, to accept at present any of these arrangements, which for the most part had been made some twenty or thirty years ago, unless with considerable modifications. The classification approved by the author has already been, to a great extent, adopted by Sir Charles Lyell in his "Students' Manual," and by the late Mr. Salter, and the author in papers to the British Association; and is based on the most recent palæontological and stratigraphical evidence. In a table exhibited for the purpose of illustrating these facts, the classification of Prof. Sedgwick, and of Sir R. Murchison, were placed side by side along with the one proposed. The columns in the table showed (1) the lithological characters of the beds comprising each group; (2) the thickness of the strata; (3) the organic remains contained in each group; (4) the number of genera and species which are known to reach from one group into another; (5) the order of the appearance of animal life upon the globe, and (6) the localities where the several groups are best seen in England. By means of the evidence set forth in these columns, the author was enabled to show the most natural divisions and subdivisions, so far as recent researches are capable of explaining them. The following are the chief divisions accepted as being the most satisfactory at present:—The *Lower Cambrian* to include the Longmynd (Harlech grits and Llanberris slates, and the rocks at Bay Head, &c.) and the Menevian groups, which were shown to be intimately connected palæontologically, and to be entirely distinct in their faunas from the overlying rocks. The *Upper Cambrian* to include the Lingula flags (lower, middle, and upper, called also Maenturog, Festiniog, and Dolgelly or Malvern) and the Tremadoc groups. These were also shown to be connected closely by some of the genera, especially by Olenus, Conocoryphe, and Dikelocephalus. The *Lower Silurian* to comprise the Arenig (Lower and Upper, the former a series only recently known through the researches of the author, and forming a connecting link between the Tremadocs and the true Arenig rocks), the Llandeilo (Upper and Lower, the former being black shales or slates, and the latter calcareous), and the Bala or Caradoc groups. The *Upper Silurian* to consist of the Llandovery (Upper and Lower), the Wenlock and the Ludlow groups. The whole of the Llandovery group was placed in the Upper Silurian in accordance with the evidence cited by Prof. Ramsay in his memoir on North Wales, along with the facts explained by the table, and which went to prove that when it was to be separated entirely from the other groups, as a Middle Silurian division, this was the most natural and proper position.—"On the Silurian Rocks of the English Lake District," by Prof. Alleyne Nicholson. In this paper the author classified the Silurian rocks of the English Lake District as follows, commencing with the lowest:—(1) The Skiddaw Slates, (2) the Borrowdale Series, or Green Slates and Porphyries, (3) the Coniston Limestone and associated shales, (4) the Graptolitic Mudstones, (5) the Coniston Flags, (6) the Coniston Grits, (7) the Ludlow Rocks. Each of these members of the series was described lithologically and palæontologically, and its geological position discussed, not only with reference to the other beds of the district, but also to the Silurians of Wales and North America.

Society of Biblical Archæology, June 4.—Dr. Birch, F.R.S., president, in the chair. "On a Religious and Political Revolution which took place in Egypt prior to the reign of Rameses III., having a probable connection with the rise of the Jewish religion. From the text of the Harris Papyrus," by Dr. August Eisenlohr. This magnificent Papyrus contains an account of the reign of Rameses III. [and the events preceding his accession to the throne. Among these he finds a most marvellous account of a politico-theological revolution made by a Syrian hero, who, after a period of general disorder, made himself chief of the whole country, and abolished the existing religion and the sacrifices then in use. The father of Rameses III., King Seti-nekht, suppressed this revolution and restored the country to its former religious institutions. The striking resemblance of this story with the narrative of the return of the Hykshos, which was extracted by Josephus from Manetho's work, and was held by nearly all authorities to be connected with the establishment of the Jewish religion is very remarkable. Dr. Eisenlohr considered these passages in the Harris Papyrus as representing the Egyptian view of this and other great events which were the immediate cause of the Exodus, in which case the Papyrus would constitute the first Old Egyptian document hitherto discovered to bear upon the subjects treated of in the Book of Exodus. And it is therefore an additional gratification to learn that the Trustees of the British Museum have recommended to the Treasury the purchase of this invaluable document.—"Observations on the Dimensions of the Great Pyramid and the Royal Coffer," by Mr. Solomon M. Drach.—"The XXXVII. Aamu in the Tomb of Chnum-Hotep, at Beni-Hassan, identified with the family of Israel," by the Rev. Daniel H. Haigh. The learned author maintained that the said group, representing 37 Aamu or Mestemmu from the land of Shu, depicted no other than the patriarch Jacob and the thirty-six legitimate members of his family (the offspring of his wives Leah and Rachel), who entered Egypt at Joseph's invitation; the concubines and their children holding a decidedly inferior rank, and regarded as slaves and slave-born, not being counted, reducing the legitimate family of Jacob to that number. Shu he supposed to be the "East;" Mestemmu he compared with Beto-Mestham (Judith iv. 6), in the territory of Dothan, conquered by Jacob (Gen. xlviii, 22). In a supplement to this paper, Mr. Haigh described a Babylonian cylinder brought from Hidah, engraved by Mr. Layard in his "Travels," and translated the cuneiform inscription as representing Terah and his children, Abraham, &c. The names Iscah and Milcah (Queen) he considered might be one name in duplicate, the result of a marginal gloss (one sign in the cuneiform writing representing the sounds *is* and *mil*).

GLASGOW

Geological Society, May 2.—Mr. James Thomson read some notes "On an Undescribed *Platycrinus* from the Mountain Limestone of Fifeshire," which he had found in a quarry to the west of Kirkcaldy. It differs in several respects from M'Coy's *Platycrinus punctatus*. The plates of the test of that author's form are punctate, while those of the form exhibited are smooth and destitute of surface ornamentation. It also differs in the form of the plates.—Mr. Thomson also laid before the Society a curious shell, which he discovered on the same occasion. It was of somewhat large dimensions, being 10 in. long, by 7½ in. in width at the broadest part. At first sight it seemed to be a variety of Nautilus, but he could not find any trace of the septa which characterise that important group of Cephalopoda. He was, however, disposed to view it as belonging to that group, but one which, so far as he was aware, had not been described in any work on Palæontology.—Mr. David Robertson read the following papers:—1. "On the Clay Beds at Kilchattan in Bute." Mr. Robertson gave an enumeration of 86 species which had been obtained from the locality, including 40 species of Mollusca, 16 of Ostracoda, 18 of Forminifera, and 12 of other orders. The prevailing shells of the deposit are *Tellina calcarea*, *Axinus flexuosus*, *Scobricularia prismatica*, *Cyprina islandica*, *Mya truncata*, and *Utriculus obtusus*. 2. "On a Fossiliferous Clay Deposit near Campbeltown." The chief interest of this section is that, contrary to the usual position of the boulder clay in the West of Scotland, here it overlies shell-bearing clay. The latter is dark grey in colour, and contrasts strongly with the overlying boulder clay, which is of a full reddish brown. The shell-bearing clay, as exposed in [the bed of a little burn or streamlet in Tangy Glen, about six miles from Campbeltown, is seen standing up in the boulder clay like a little knoll, and has doubtless been brought to that form by abrasion. It can be traced for a distance of 60 or 70 yards; its exact depth could not be ascertained, but as the rock is seen at a short distance on either hand, it is probably not more than a few feet deeper than what is exposed. The boulder clay overlies it to a height of 50 or 60 feet. The latter consists of 50 per cent. of fine mud and 50 per cent. of sand and gravel, while the shell-bearing clay gives 80 per cent. of fine mud and only 20 of sand and gravel. The fossils are but thinly met with in this deposit—molluscs in particular are comparatively rare, the few found being chiefly *Leda pygmæa*, with an occasional *Leda pernula* and a few fragments of other species. Ostracoda and Foraminifera are better represented, 18 species of the former and 26 of the latter having been obtained. A remarkable feature of the Ostracoda in this deposit is that they have much in common with those found in the clays on the east coast of Scotland, which have been held to represent more strongly Arctic types than those generally found in the West. Amongst these are *Cytheropteron Montrosiense*, *Cytheropteron vespertilio*, and *Cytheropteron Sorbyana*. None of these have hitherto been met with in the clays of the West of Scotland, with the exception of one specimen of *C. Montrosiense*, which had been found in the excavations for Messrs. Randolph and Elder's new dock near Govan. This specimen was found at a depth of 18 feet in a lower bed of clay dipping away from the river. An upper bed, which dipped to the river, contained only more recent forms common to our raised beaches and present seas. He might add that *C. vespertilio* and *C. Sorbyana* are common species in the clays of Norway. The chairman observed that this additional discovery of Arctic marine shells below the lower boulder clay of the West of Scotland was a further confirmation of the interesting fact that an Arctic or northern fauna had spread over certain tracts of the existing sea bottom before the lower Till of the country was deposited.

PHILADELPHIA

Academy of Natural Sciences, Oct. 17, 1871.—The president, Dr. Ruschenberger, in the chair. "Remarks on Fossils from Oregon."—Prof. Leidy directed attention to some fossils, part of a collection from Oregon, submitted to his examination by the Rev. Thomas Condon, and indicated in the "Proceedings" of Oct. 18, 1870. One of the fossils, a brain cast, or rather a cast of the interior of the cranium of a large mammal, has about the same form and size as that of the horse. The cerebral hemispheres are nearly as much convoluted as in the latter, and measure about 4½ inches in length and breadth. It may pertain to a large tapiroid animal, though I suspect it belonged to an oreodont. A large atlas, perhaps belonging to the same animal as the former specimen, measures 5 inches in breadth between the outer prominent borders of the articular concavities for the occipital condyles, and it is about 4½ inches from the neural tubercle to the hypapophysis. It differs in several important points from the atlas of the rhinoceros, horse, ox, &c., and the want of sufficient means of comparison prevents a determination of its near relationship. Another fossil, labelled "Alkali Flats," consists of the greater part of the crown, apparently of a last upper premolar, or perhaps of a transverse pair of lobes of a true molar, of an animal as large as that to which the preceding specimens belonged. The tooth approaches in character the corresponding portion in the oreodonts, but differs in the proportionately less degree of development of the inner lobe of the crown as compared with the outer one, and in the greater degree of development of the inner basal ridge. The crown measures 1⅓ inches in transverse diameter. These fossils appear to indicate an unknown pachyderm, which may be designated by the name of *Hadrohyus supremus*. Among the Oregon fossils there are a number of imperfect remains, of which it was formerly remarked they indicated at least two species of rhinoceros. One of these was thought to be the same as the *R. occidentalis;* the other was suspected to be the same as the Californian species, *R. hesperius.* Some additional specimens indicate the second species to have been intermediate in size to that last named and the *R. crassus* of the Niobrara river. One of the specimens from Bridge Creek, consisting of a mutilated upper jaw fragment, with portions of the fangs of the true molars, shows these to have occupied the space of about 5 inches. An isolated tooth from Alkali Flat, apparently a last upper premolar, probably belongs to the same animal. From the outer part of its crown three folds project into the bottom of the median valley. The tooth measures 1 inch and 10 lines wide. The species may be named *Rhinoceros pacificus.* Another fossil specimen, labelled "Crooked River," consists of an isolated vertebral plate of a large turtle, apparently the eighth bone of the series. It has the same shape as in *Stylemys niobrarensis,* but is proportionately much shorter in relation with its breadth. It measures 2 inches wide, 1 inch 7 lines long, and 7 lines thick. The specimen probably indicates an undescribed species, which may be named *Stylemys oregonensis.* Two additional fossils are brain casts, probably of *Oreodon superbus.* The cerebral hemispheres are 10 inches 8 lines long, and together about 2½ inches broad.

PARIS

Academy of Sciences, June 17.—A note by M. E. Combescure on a point in the theory of surfaces was presented by M. Chasles.—M. E. Roger read a third memoir on the theory of capillary phenomena.—Father Secchi forwarded a reply to the observations recently presented by M. Respighi in opposition to his remarks on some peculiarities in the constitution of the sun. —M. A. Genocchi read a paper on the intensity of the heat of the sun in polar regions, in which he discussed the calculation of the annual mean heat of the sun within the polar circles.—M. C. Martins communicated a note on the stormy nature and unequal distribution of the rains on the surface of the department of Hérault.—M. A. Bérigny forwarded a note describing the effects produced by the striking of a house by lightning at Versailles on the night of June 6.—M. Lartigue presented an explanation of the mistral.—M. H. Sainte-Claire Deville presented a note by M. Laurence on a compound of oxide of tin with anhydrous acetic acid, produced by heating the two substances together in a sealed tube to 302° F. Upon this paper M. Elie de Beaumont made some remarks, in which he put forward the notion that in the early ages of the world nature may have employed a chemistry different from that which we now see in action in volcanoes and in atmospheric phenomena.—M. Sainte-Claire Deville also presented a note by M. G. Saillard on a new phosphoplatinic derivative of toluidine obtained by heating an alcoholic solution of Schützenberger's ether Ph $(C^2 H^5O)^3 Pt Cl^2$, with an excess of crystallised toluidine.—M. Blanchard communicated a note by M. C. Dareste on the natural affinities of the fishes of the family Balistidæ, which he regards as most nearly allied to the Teuthyidæ.—M. Garrigou presented further observations on the constitution of the Pyrenees in reply to a note by M. Leymerie, and in support of his previous remarks on the same subject; and M. A. Brongniart communicated a note by M. G. de Saporta on a revision of the fossil flora of the gypseous deposits of Aix.

June 24.—M. O. Bonnet communicated a note by M. A. Ribaucour on the theory of lines of curvature.—M. Delaunay presented a note by M. Bresse on the determination of the trajectory of a point for which a certain integral is the minimum. —M. J. Morin read a notice of a new voltaic battery with continuous action, acting by sulphate of copper. This consists of a hollow cylinder of copper, in the centre of which is the cylinder of zinc, he two being separated by a cylinder of filtering paper, and the space between the copper and paper being filled with grit, and that between the paper and the zinc with flowers of sulphur. Batteries of this construction have been in operation for twenty months without being replenished.—M. Piarron de Mondésir forwarded a note on the theoretical value of the relation between the two specific heats of permanent gases.—M. de Saint-Venant presented a note by M. J. Boussinesq on the calculation of the velocity of light in bodies in motion.—M. Delaunay communicated a paper by M. F. Tisserand on movements relative to the surface of the earth.—A note was read by M. T. Schlœsing on the solution of carbonate of lime by carbonic acid, containing the results of series of experiments made by the author on this important subject.—A note by MM. Girard and De Laire on the manufacture of aniline colours, was read.—M. P. Champion presented a note on some compounds of paraffin, in which he described an acid, paraffinic acid, with the formula $C^{20} H^{30} NO^{10}$ derived from paraffin by the action of nitrosulphuric acid, and noticed the action of chlorine and bromine upon paraffin.—M. Milne-Edwards presented a note by M. Fischer on the geographical distribution of the podophthalmous crustacea of the Bay of Biscay, in which the author compared this part of the fauna of the Bay with that of the British coast on the one hand, and that of the Mediterranean on the other.—M. C. Bernard presented a further note by M. Oré on M. Liebreich's opinion that strychnine is to be regarded as an antidote to chloral; and a paper by M. Brémond containing an account of some experiments on cutaneous absorption.—M. Duchartre communicated a note by M. Prillieux on the disease of the peach tree known in France under the name of *cloque,* which the author ascribes to the action of a parasitic fungus, described by Tulasne under the name of *Taphrina deformans.*—M. Daubrée presented an examination of the rock-masses of native iron, discovered by Prof. Nordenskjold in Greenland, of one of which he gives an analysis. —M. Daubrée presented a report on a recent memoir by M. Delesse on the deformations which the strata of France have undergone.—M. de Quatrefages communicated an interesting paper by M. J. de Baye, "On the Prehistoric Caverns of the Marne, belonging to the Neolithic period;" and a second note by M. E. Rivière on the fossil man of the Mentone caves.

BOOKS RECEIVED

ENGLISH.—Concrete Arithmetic: T. H. Orme (Groombridge and Sons).

AMERICAN.—Description of the Balænoptera musculus: T. Dwight, jun., M.D. (Boston Soc. Nat. Hist.).

FOREIGN.—(Through Williams and Norgate)—Theoretische Maschinenlehre: D. F. Grashof. 1er Band, 1te Lieferung.—Des Préparations microscopiques tirées du régne végetal: Gronland, Cornu, and Rivet.—Lehrbuch der anorganischen Chemie, 3te Abtheilung: Dr. Büchner (Schluss).

DIARY

FRIDAY, JULY 5.

GEOLOGISTS' ASSOCIATION, at 8.—On Corbicula fluminalis, its associates and distribution in Britain: Alfred Bell.—On the Dip of the Chalk of Norfolk, and the Remains of Old Land Surfaces called the "Stone-bed:" Rev. John Gunn, M.A.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, JULY 11, 1872

ECONOMIC ENTOMOLOGY

We have more than once had occasion to refer to the zeal with which the investigation of the insect pests so destructive to our crops of roots and fruits is carried on on the other side of the Atlantic. Three publications now lying before us—two from the United States, and one from Canada—furnish a text for a few further remarks on this subject. They are: "Fourth Annual Report of the Noxious, Beneficial, and other Insects of the State of Missouri," by Charles V. Riley, State Entomologist; "Second Annual Report on the Injurious and Beneficial Insects of Massachusetts." by A. S. Packard, jun., M.D.; and "Report of the Entomological Society of the Province of Ontario for the year 1871." These are all official publications; the two first being Reports made to the State Boards of Agriculture; the last printed by order of the Legislative Assembly. They represent the result of investigations made at the expense of the citizens of the respective States or Province, who are willing to tax themselves in order that the highest scientific experience at their command may be brought to bear on researches whose object is the material improvement of the resources of their country. The only similar efforts to which we can point in this country are the result of private enterprise. The Royal Agricultural Society has long engaged the services of a chemist to analyse manures and feeding-stuffs, and we recorded not long since the addition to its staff of a consulting botanist, and its intention to appoint also a consulting entomologist; but their investigations are carried on exclusively for the benefit of the members of their Society. The Royal Horticultural Society has also arranged for a course of lectures on Economic Entomology, and last year invited competition for prizes for collections of insects to illustrate this subject.

We have so often expressed our view on the relative advantage of having these investigations carried on by private enterprise, or under the direction of the State, that it is needless to repeat it here. Dr. Packard thus forcibly advocates the latter alternative, referring to M. Pasteur's labours in endeavouring to mitigate the scourge of the *pébrine* in the South of France:—"It should be remembered that this remarkable result is due primarily to the most abstruse researches upon microscopic plants by specialists, for the pure love of science. Their cloister studies, put to practical account, save the destruction of one of the largest agricultural industries in Southern Europe. In like manner, had the general Government or individual States encouraged the botanist and entomologist in their studies, and caused them to be turned to practical accounts, we should not have had to give up the cultivation of wheat in the northernmost States; our cotton crop would perhaps have been doubled; and our garden and field crops have regularly yielded a steady return to the producer." It must be recollected that the Federal Government at Washington is now spending large sums in the compilation and printing of enormous numbers of the Monthly Agricultural Reports, in addition to the money devoted by the separate States.

Mr. Riley's Report is one of a series, of which we have already noticed its immediate predecessor, and is a pamphlet of 150 pages, containing, independently of its special object, no small amount of valuable information of general interest. The higher the summer temperature, the greater appears to be the variety of the foes against which the agriculturist has to contend; and their number in the south-western of the United States is legion. After discussing the various insect-enemies to the potato, the cabbage, the apple, the walnut, the grape-vine, and other crops, the causes of their appearance, and the best antidotes artificial or natural, the greater portion of Mr. Riley's Report is devoted to the Silkworm, on the cultivation of which he has bestowed great attention. He describes the various kinds of silkworm, their mode of cultivation, their food in the larva, and imago conditions, and the various enemies, parasitic and others, to which they are subject; and this portion of the Report is illustrated by a number of very well-executed wood engravings. The practical results are thus summed up:—"There can be no good reason why silk-culture may not become one of the industries of this country, or of our State, especially if fostered at the start. I would, however, advise no one to enter into it on a large scale as a business. The raising of silk is seldom lucrative even in the most favourable countries; for in this, as in most other industries, the principal profits accrue to the middle men, reelers, and manufacturers; but on a small scale, and prosecuted in connection with other branches of agriculture and horticulture, it will give the most desirable returns for the time employed. The erection of a few reeling establishments is absolutely necessary to establish this industry."

Dr. Packard's Report treats of similar matters in a similar scientific spirit. It is interesting to note the reciprocity of indebtedness between this country and America in the matter of insect pests; we have received from them the *Phylloxera vastatrix*, so destructive to our vines; and have given them as a set-off our white cabbage butterfly, the *Pieris rapæ*, first introduced into the United States fifteen years since, and now spreading every year with terrible rapidity. It is curious that the chief check on the increase of the larva is a native, not an imported parasite, the *Pteromalus puparum*.

The Canadian Report is interesting, as being the result of the first appropriation of money for these purposes, as far as we are aware, by any of our American colonies. The Entomological Society of Ontario is subsidised by the Legislative Assembly, in order to encourage the devotion of its labours to inquiries of a practical character for the benefit of the colony. The present Report, of nearly 100 closely printed pages, well illustrated with woodcuts, gives promise of much useful work. The Report is divided into sections relating to insects injurious to the apple, grape, plum, currant and gooseberry, wheat crops, potato, cabbage, and cucumber tribe, by different competent members of the Society.

We have referred to these Reports in order to draw from them a lesson as to the direction of the labours of our botanists and entomologists at home. We have among us at least as high scientific talent in these branches as in the United States; but, with a few rare exceptions, this talent is not devoted to researches which have a definite practical bearing on the welfare of the country. If, as we believe to be the case, the reason of this is that such researches can rarely pay the investigator directly, is not the reasoning sound which would advocate the devotion of public money to purposes which must inevitably yield such large returns to the community at large?

ORIGIN AND DESTINY OF MAN

Man in the Past, Present, and Future: a Popular Account of the Results of Recent Scientific Research as regards the Origin, Position, and Prospects of the Human Race. From the German of Dr. L. Büchner. By W. S. Dallas, F.L.S. (London: Asher and Co.)

The Martyrdom of Man. By Winwood Reade. (London: Trübner and Co.)

THE Science of Man has become matter of public interest. Fluent writers now popularise anthropological evidence, and even use it as a lever to force public opinion in some desired direction. The two clever books before us are in this way remarkable as signs of the times. They treat largely of Anthropology, but not so much for itself as in order to promote materialistic views of philosophy, and advanced schemes of social re-organisation. In briefly noticing Dr. Büchner's "Man," we shall not discuss his general doctrines on their merits. The development theory, if not by acceptance, at least by influence, has so far become part and parcel of modern thought, that it need not be re-investigated *à propos* of each new popular book which advocates it, thereby gaining a share of its prestige. The question for the critic, in judging the work of the self-appointed public instructor, is whether he deals fairly with his public, carefully weighing and testing for them the data which they cannot weigh and test for themselves. We regret to say of a writer so influential in Germany and England as Dr. Buchner, that on the present occasion at least he has not made out his claim to be a public expositor of a very serious subject. In fact, the seriousness of the subject, as demanding extensive study and careful judgment, is just what he fails to appreciate.

Dr. Büchner has the gift of easy exposition. Whether he is descanting on the drift implements and the antiquity of man, or tracing the analogies to successive orders of animals in the successive phases of the human embryo, or giving an account of the approaches made by other animals to the human mind and character, or commenting on theological dogma as opposed to scientific investigation, or pointing out how conscience can adapt itself to contradictory moral standards, or suggesting a re-adjustment of the laws of marriage in the interests of progressive society, his opinions run on with the same shapely smoothness, whether they happen to be solid or hollow. His generalities are put with praiseworthy neatness; his well-selected examples clinch them hard, and just in the right place. He is a capital expounder, when his facts and inferences happen to be sound. Unfortunately, however, his strength of rhetoric is not matched by strength of criticism, as we shall briefly show, choosing the department of anthropology as the trial-ground. In order to make the text of his work readable by the general public, Dr. Büchner has collected the copious materials which might alarm or tire the majority of readers in an Appendix, to which we are to look for actual details or more exact proof of what is enunciated in the body of the work. Looking into this Appendix accordingly, we extract some significant examples.

For a test in Prehistoric Archæology, a passage may be taken (p. 251), which shows our author not to be aware of the distinction in rude stone structures between the *dolmen* formed of several slabs and the single upright standing stone or *menhir*. His words are, "Still older than the so-called 'giants' graves' are the *Dolmens* or stone tables (also called *Cromlechs* or *Menhirs*), very ancient stone edifices, which have been found especially well-represented in Brittany. They consist of upright stones covered with slabs laid transversely upon them," &c. At p. 335 we find Dr. Buchner, on the question of low stages of human language, supporting his views by taking seriously such worthless remarks as the following: "The speech of the Fans of West Africa is, Du Chaillu says, a collection of guttural tones which no one can understand." We beg leave to refer Dr. Büchner to Captain Burton's observations in his "Wit and Wisdom from West Africa," on the grammar and vocabulary of the Fan language, which is in fact but slightly different from allied languages of the coast. In studying the relation between the lower animals and man, of course the problem of savage religion is important, and we looked curiously at Dr. Buchner's evidence. It begins with the following citation (p. 328): "Three large sections of the earth's surface, says G. Pouchet, which are still inhabited by savages, appear to have remained till now exempt from religious notions; they are the interior of Africa, Australia, and the Polar regions." And this in the face of twenty published accounts of the religious beliefs of the Australian and Esquimaux races! There are, it is true, ambiguities in the use of such terms as religion, behind which inaccurate statements may find a refuge. But as to such a subject as the arts of producing fire and cooking food, there can be no ambiguity. We take as they stand (p. 332) a series of statements on which Dr. Büchner relies. "There are still peoples, such as the Dokos, the Andamans, &c., who know not the use of fire, and devour all their food raw. Moreover, that the use of fire cannot be an attribute of humanity as such, is shown by the circumstance that so many peoples have been fire-worshippers, and in part are so still, that, therefore, they considered fire something extra and supernatural. In like manner, when Magellan set fire to the huts of the Marian Islanders, to whom fire was unknown, they looked upon it as a kind of living monster which devoured wood. Also in the Ladrone Islands the Spaniards found the natives unacquainted with the use of fire." We will take these sentences *seriatim.* As to the first, mention may be made of Dr. Mouat's account of the Andaman Islanders' practice of keeping fire constantly burning in hollow trees, covering it up with ashes, and cooking native pigs and fish in these natural ovens. As to the second, the inquiry suggests itself whether the fact of numerous tribes being water-worshippers proves the use of water not to be an attribute of humanity as such? As to the third and fourth sentences, it has to be pointed out that the contemporary account of Magellan's voyage knows nothing of the

Marian Islanders being unacquainted with fire, and looking on it as a living monster; this story was first told by an untrustworthy writer a century or two later.* Finally, after reading the last two sentences referred to several times, we cannot resist the inference from them that Dr. Büchner, putting down as separate data two fragments of one story, did so through not being aware that the Marian Islands and the Ladrone Islands are the same.

Mr. Winwood Reade's "Martyrdom of Man" is more careful as to evidence, though less shapely in arrangement, than Dr. Buchner's work. It begins with ancient Oriental history, then passes to primæval religion as leading up to Christianity and Mohammedanism, next proceeds to discuss negro slavery and its abolition, and lastly plunges into a dissertation on things in general, especially the origin of life, the evolution of orders of plants and animals, the development of the intellectual and moral faculties, and the rise and decline of religion. Mr. Reade's first principle affords him a theory which at any rate places these various topics along lines of continuity. "Mind is a property of matter. Matter is inhabited by mind. There can be no mind without matter; there can be no matter without mind. When the matter is simple in its composition, its mental tendencies are also simple; the atoms merely tend to approach one another and to cohere; and as matter under the influence of varied forces (evolved by the cooling of the world) becomes more varied in its composition, its mental tendencies become more and more numerous, more and more complex, more and more elevated, till at last they are developed into the desires and propensities of the animal, into the aspirations and emotions of the man. But the various tendencies which inhabit the human mind, and which devote it to ambition, to religion, or to love, are not in reality more wonderful than the tendency which impels two ships to approach each other in a calm. For what can be more wonderful than that which can never be explained? The difference between the mind of the ship and the mind of man is the difference between the acorn and the oak." Following this all-comprehensive doctrine, Mr. Reade works out in well-turned paragraphs the evolution of animal and human faculties, the development of science and art, the rise and change of social and moral laws. He especially dwells upon the imperfections and mistakes of early ages, which, while afflicting the world with temporary evil, yet prepare it to attain in later times to better things. This is what he means by the title of his book, of which the historical and scientific parts seem intended to give weight to a polemical point, that of urging all enlightened men to take part in the great work of demolishing one of those institutions which, once the highest attainable, has now become injurious. "Christianity must be destroyed." "I give to universal history," the author writes in his conclusion, "a strange but true title—'The Martyrdom of Man.' In each generation the human race has been tortured that their children might profit by their woes. Our own prosperity is founded on the agonies of the past. Is it therefore unjust that we also should suffer for the benefit of those who are to come? Famine, pestilence, and war, are no longer essential for the advancement of the human race. But a season of mental anguish is at hand, and through this we must pass in order that our posterity may rise. The soul must be sacrificed; the hope in immortality must die. A sweet and charming illusion must be taken from the human race, as youth and beauty vanish never to return."

Mr. Reade's plan of not making particular acknowledgment to the writers whose facts and ideas he incorporates, is likely to mislead his readers, some among whom may fancy him a great original thinker, while others will certainly set him down as a mere compiler. It is not our business to discuss either his fanatical anti-Christianity, or his promised "Religion of Reason and of Love;" but so far as concerns Anthropology, his book shows the results of considerable reading and observation, conveyed with a certain power of word-painting and epigram. Now that he has delivered himself on the great question of modern thought, and has probably ascertained that no individual martyrdom awaits him apart from the rest of his species, we should recommend him to turn his experience and ability as an ethnologist to doing more solid work in some special department of his science.

OUR BOOK SHELF

The Scientific and Profitable Culture of Fruit Trees. From the French of M. Du Breuil; adapted for English cultivators by W. Wardle. Second edition, carefully revised by George Glenny. (London: Lockwood and Co., 1872.)

THREE practical men have been concerned in the writing and editing of this little book, which is on all grounds well worthy of a place on the bookshelves of every gardener and grower of fruit-trees. It professes to direct the gardener in all the operations necessary, from the insertion of the graft to the completion of the tree, and the proper management through all its stages; and the instructions in all the various kinds of grafting are full and comprehensive. The first portion treats of the different modes of grafting, pruning, and training, and instructs in the mysteries of grafting by approach or inarching, branch grafting, cleft grafting, crown grafting, side branch grafting, and budding, or shield grafting. The main part of the book is then devoted to directions as to the mode of cultivation of the pear, apple, peach, plum, cherry, and apricot, and their best varieties. The portions of the work which were specially adapted for French cultivators have been modified or enlarged by the editor, and the language is throughout plain and simple. It is embellished also by nearly 200 illustrative woodcuts.

Reports of the Mining Surveyors and Registrars (Victoria) for the quarters ending June 30, September 30, and December 31, 1871. (Melbourne, printed by authority.)

THESE reports show that the mining operations in Victoria continue to be assiduously prosecuted, the total yield of gold during the three quarters being estimated at 1,055,808 oz. 4 dwt. 4 gr., of which 524,990 oz. 7 dwt. 21 gr. were got from alluvia, and 530,817 oz. 8 dwt. 7 gr. from quartz reefs. The quantity of gold, the produce of the colony, exported during same quarters, amounted to 916,114 oz. 6 dwt. During the quarter ending June 30, the greatest yield of gold occurred, and the largest quantity was exported. But the yield and export of each of the three quarters approach pretty nearly to the same figures, indicating that the working of the mines has got into a settled state, and that the produce for some time to come may be expected to be proportionate to the capital employed. A number of tables give some interesting details relative to the number of miners, the machinery in use, and its value, from which may be gathered some idea of the extent and importance of the several gold fields in the colony. Of these fields, that of Ballarat appears at the head of the list, showing a total number of miners employed of 13,892, the approximate value of mining plant being 516,825*l.*; 134½ square miles of auriferous ground are actually worked upon, and 189 auriferous reefs have been proved. In this district also occur the deepest shafts, two of which reach the depths of 866 ft. and 900 ft. respectively. The price of Ballarat gold varies from 3*l.* 13*s.* 6*d.* to 4*l.* 2*s.* 6*d.* per ounce, the latter appearing to be the highest price obtained for any gold in the colony. In this mining district 78,502 tons 10 cwt. of quartz were crushed during the last quarter of the year, yielding 5 dwt. 19·42 gr. of gold per ton; the average yield in the different gold fields being very variable. But in considering the relative importance of each district, we are reminded by Mr. Brough Smyth, the Secretary for Mines, that the table relating to machinery should be examined and compared, from which it appears that it is not always the mines that show the greatest yield of gold which give the largest return to proprietors. An interesting feature in these reports is the description by Dr. F. von Mueller (Director of the Botanic Garden of Melbourne) of certain new vegetable fossils which are met with from time to time in the deep auriferous drifts of older Pliocene age. These consist of the fruits of plants which, according to Dr. Mueller rejoiced in a milder climate, and displayed forms of tropical grandeur now foreign to the spot. Five genera are described and illustrated with beautiful lithograms. Of these, among fossil genera *Phymatocaryon* comes nearest to the extinct *Cupanoides*, *Tricarpellites*, and *Wetherellia* of the London Clay. Another genus, *Trematocaryon*, bears no very close alliance with any genus among living or fossil plants. The remaining genera are *Rhytidotheca*, with some affinities to *Chloroxylon* and *Flindersia*, *Plesiocapparis*, the real affinity of which is doubtful, and *Celyphina*, which appears to belong to the order of Proteaceæ. We are glad to learn that many other fossil remains have been secured, and are now under examination by Dr. Mueller. For the collection of these fossils palæontologists are indebted to the enlightened zeal of Mr. John Lynch, mining surveyor and registrar. When one looks over the long list of surveyors and registrars employed by the Colonial Government in gathering statistics, we can only hope that some, if not all, of these gentlemen will follow Mr. Lynch's example, and thus be the means of increasing our knowledge of the "old world" of Australia.

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* See Tylor, "Early History of Mankind," p. 234.

J. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Rigidity of the Earth and the Liquidity of Lavas

I WAS glad to see the powerful argument in favour of the solidity of the earth which Sir William Thomson has deduced from its great rigidity, republished in Nature of January 18, since, though never attacked, it has been ignored by those who hold a contrary view. In a lecture before the Royal Institution in May 1867, I appealed to it in support of the notion which I have long maintained of a globe solidified from the centre; and I remember that for that and for other heresies in my lecture I was sharply attacked in the *Geological Magazine*, and in turn defended myself as best I might in the same magazine for February 1868. I there said that "the conclusions of Hopkins from the phenomena of precession and nutation, the investigations of Archdeacon Pratt on the crushing effect of immense mountain masses like the Himalaya, and the deductions of Sir William Thomson from the phenomena of the tides, showing the great rigidity of the earth, are so many concurrent evidences that our planet, if not actually solid to the centre, has a crust far thicker than can be accounted for by the theory of a liquid globe, covered only with a crust resulting from superficial cooling." This discussion of the subject at that time seems to have had the effect of bringing to the front the defenders of the latter theory, which, in the popular mind at least, has a mythological rather than a scientific foundation. It was, therefore, well that Sir William Thomson should repeat his arguments.

Mr. Fisher has in this connection, in Nature for January 25, referred to the distribution of fluid ignited matter within the earth, and to the relation of volcanoes to great lines of elevation, which would seem to show that the local distribution of such matter may be connected with these lines. He says, "I have suggested that this fluidity may arise from a diminished pressure beneath mountain ranges, owing to their mass being partly supported by the lateral thrust which has upraised them," and adds, "If any of your correspondents can propose another explanation of this remarkable coincidence compatible with the supposition of a rigid globe it would be interesting to know it."

My present object in writing is to call attention to the explanation proposed by me in the *Geological Magazine* for February 1870, conceiving, with Archdeacon Pratt, that the condition of things beneath a great mountain mass is one of increased rather than of diminished pressure. I there said, "Pressure, which in the first case, that of simple fusion of anhydrous materials, prevents liquefaction by preventing expansion, in the second case (that of igneo-aqueous fusion, or liquefaction at high temperatures, by the aid of a small portion of water, as maintained by Scrope, Scheerer, and Elie de Beaumont) on the contrary, favours liquefaction by promoting the solution of the water-impregnated mass." As Sorby has shown, a conversion of mechanical into chemical force appears in the increase of solubility under pressure. In other words, pressure prevents fusion, when, as in most instances, it is a process of expansion; but favours solution, which is, with few exceptions, a process of contraction.

"Now since I place the seat of volcanic action in a region where solution, rather than simple fusion, is the cause of liquidity, I am led to consider pressure as one of the efficient causes of the liquefaction of rocks, and to regard its diminution as leading to solidification." (See also Amer. Jour. Science, ii., l. 27).

Montreal, June 21 T. Sterry Hunt

Fouling of the Nile

There is an account—of which I have unfortunately lost the reference—of the Nile becoming crimson and putrid, and its fish being killed, during the historic period, seemingly by microscopic algæ. If any learned contributor could tell me where I can find this fact recorded, and could give me any details of similar phenomena, he would do me an especial kindness.

Chester, July 8 C. Kingsley

Volcanoes of Central France

After carefully reading through the two extracts from Sidonius and Avitus contained in your number of May 30, my impression was that notwithstanding some manifest improbabilities, the conclusion was inevitable that earthquakes and other volcanic phenomena had actually been witnessed at Vienne. However, after examining all the facts of the case, a theory has occurred to me by which both accounts may be consistently explained without any such phenomena having occurred there at all. It would only remain to be shown that my supposed facts are not contradicted by evidence actually on record.

First, as to the persons:—The whole matter centres in the great Gallic family of Avitus, belonging to Auvergne. The most distinguished member of this family was Marcus Mæcilius Avitus, a person of so much importance that in the year 455 he became Emperor, but so little fortunate that in October 456 he was conquered and driven from the throne. He was allowed to become Bishop of Piacenza, but fearing for his life, returned to Auvergne and took refuge in the Church of St. Julian.* We find shortly afterwards two members of this family in possession of the See of Vienne, father and son, in succession, and the latter was the author of our Homily.

It seems, therefore, highly probable that the bishopric was in the gift of the family, or in some way dependent on it. In the year 463 we first find St. Mamertus in possession of the

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* According to Gibbon he died on the way; the one certain fact is that he was buried there.

See, and his brother C. *Ecdicius* Mamertus seems to have been closely connected with it; indeed, he seems to have composed music for the choir. We are warranted in conjecturing that this family must have had intimate relations with that of Avitus. The name of the author of the Homily is A. *Ecdicius* Avitus; Sidonius Apollinaris was son-in-law of the Emperor Avitus, and his intimacy and correspondence with "Lord Patriarch Mamertus" confirm the supposition.

We have seen that M. M. Avitus went to Rome as Emperor, A.D. 455. The first time we hear of Mamertus as Bishop of Vienne is A.D. 463. What, then, is the probability of the following facts?

When Avitus went to Rome, he took with him many of his intimate friends and connections, and among them St. Mamertus. He obtained for Mamertus some Italian See, of which the principal town was situate at no great distance from Vesuvius. It was walled, had a large forum, and was partly inhabited by the nobility of the country. Upon the expulsion of his patron, Mamertus exchanged to the See of Vienne, or obtained that See by some other means. During the short time he was in Italy he witnessed those earthquakes and showers of ashes described in the letter and the Homily.

Assuming these facts, how will they agree with the statements in these documents, and with the known history of Vesuvius?

It is clear, from the letter of Sidonius, that the reason why the people of Auvergne observed the Rogations was not the fear of earthquakes, but of the incursions of the Goths, against whose advances they formed the chief barrier; they lived in the midst of the "surrounding terrors," yet Sidonius himself had to make inquiries and get information (*Sciscitatio*) in order to become acquainted with the volcanic phenomena: he nowhere says that "the city divinely committed to Mamertus" was Vienne, and all the circumstances are connected with Mamertus personally, and with a town only through him.

St. Avitus succeeded his father A.D. 490, and died A.D. 525. At what precise date he wrote the Homily I suppose is unknown; it may, perhaps, have been written before he was Bishop. The date of his birth seems to be unknown, but he may very well have been old enough to have been with his father in Italy, and as a youth have witnessed the phenomena that were seen by Mamertus; and in the congregation that listened to the Homily, there may still have been "many" who had been there also (with the army?), and had also seen them.

The words "at that time" mean that the earthquakes, &c., occurred at the same time with the attacks of the Goths on the inhabitants of Auvergne and Vienne.

One astonishing circumstance is that the Rogations, though instituted (in Gaul at least) by Mamertus, say between 456 and 463, had already obtained in "nearly the whole world;" evidently no such rapid and wide extension could be due to the fear of earthquakes; but it is easily understood, if they were considered to be a protection against the attacks of barbarians; the time of their celebration shortly before Whitsuntide would, I suppose, be about the time of preparation for the usual summer campaign.

But how account for a Bishop of Vienne instituting at Vienne a ceremony which is alleged to have been occasioned by events that happened in Italy? Thus:—Mamertus, who had no previous knowledge of volcanic action, would be greatly struck by what he saw; and even to the Italians these manifestations were probably a novelty. Vesuvius had been comparatively quiet for 200 years, and these (by hypothesis) were among the first intimations of that renewed fury which reached its climax in 472, and is mentioned by Procopius (I have no access to his work).[*]

Struck with terror, the idea of the Rogations occurred to him while in Italy, possibly he may have instituted them there; but it agrees better with the claim of Vienne to suppose that before he had the opportunity of carrying out his idea, he found himself again in Vienne, and first instituted them there, not against earthquakes, but against the barbarians.

The two things together, the earthquakes and the wars, suggested to him the possible approach of the awful time indicated by Christ (Matt. xxiv. 15)—"Cum ergo videritis abominationem desolationis," &c.; hence the "abominabilia" and the "doom of desolation" of the Homily. The "magna tribulatio" would well agree with the constant wars, and the "ineffable distress" occasioned by them.

Norwich, June 5 HENRY NORTON

The Wanderings of the Esquimaux

IT is very gratifying to find that Mr. Howorth, whilst holding the position he has taken up with augmented force, has accepted, in the same kindly spirit in which they were written, the observations on the migrations of the Esquimaux which I was led to make after reading his very excellent letter in NATURE of May 9.

Mr. Howorth now brings forward the language of the Esquimaux in support of his views. This may be a strong point, although, if I remember rightly, one or two distinguished ethnologists think differently. As for myself, having little or no knowledge of languages, I can express no opinion one way or other.

Open as I am and shall be to conviction, and ready to give up my opinion cheerfully if proved wrong, I find that so far, the able arguments brought forward by Mr. Howorth have not in the slightest degree unsettled my belief in the truth of the Esquimaux tradition, communicated through interpreters whose competence I proved in a very satisfactory manner, at the same time that I ascertained the reliability to be placed on information transmitted from one to another by the Esquimaux. This I did by comparing information given by the natives of Repulse Bay, through my interpreter, with portions of the narratives of Arctic explorers of distinction (with which it agreed very closely) written in the one case more than twenty, and in the other more than thirty years before; of course neither the Esquimaux nor the interpreter knew what was in these narratives.

Mr. Howorth quotes numerous authorities, and I have no doubt does it so correctly that there is no necessity to look them up. These I shall now endeavour to answer, and as nearly as possible in the order in which they appear in his letter.

I do not think that the fact of the "Arctic Highlanders" building "stone igloos instead of snow huts," or their "ignorance of boats, either kayaks or oomiaks," makes any "broad distinction" between these people and the "American Esquimaux."

In the far west, that is, from Behring Strait to the Mackenzie River, the Esquimaux live in wooden houses during winter, probably because driftwood is abundant. They also have both kayaks and oomiaks.

As you go eastward and get near the Coppermine River, snow-huts form the winter shelter, probably because little or no wood is to be found, and a stone house without fuel to make a fire, is, as I know by experience, much colder and more comfortless than a snow-house under the same circumstances. The food of these natives is principally reindeer, musk cattle, and fish, with some seals, but neither whales nor walrus, as far as I could learn, to give fat for fuel. Here, also, the oomiak or large luggage boat disappears, it may be because the Esquimaux are less numerous, more scattered, and live in smaller communities, and do not require it. Another reason may be that the sea is more ice-encumbered. The kayak is, however, still in use, and when it is requisite to transport a family or a heavy load across water, two or more of these are fastened together, poles or paddles laid across them, and thus a kind of platform is constructed, which will carry very considerable weights in perfect safety.

This state of things prevails as far eastward as the great Fish River, Boothia, Gulf Committee, Repulse and Hudson Bays, but as the Esquimaux travelled northward to latitudes 76° and 77° by the route I have supposed them to do, as described in a former letter, the difficulty of building kayaks would be increased in consequence of scarcity of wood to make the frames.[*] The necessity for them would also become less, as the sea became less free from ice, and if, as I suppose, they chiefly hunted and lived upon reindeer and musk cattle. The usual season for killing seals is before the ice breaks up, so no kayak is required for that purpose.

From the description given me of the numerous "moss-grown ruins of deserted huts" seen near Smith Sound and on the Parry Islands, I am led to believe that many of them were not dwelling places at all, but the ruins of large stone "caches," such as are found in many places on the shores of America, where the natives have collected a quantity of provisions, round which stones are built in a very solid manner as a protection against the attack of the fox, the wolf, their own dogs, and, worst of all, the wolverine.

Let me now say a few words about the Arctic Highlanders of

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[*] A few days after writing this I read the passage in Procopius, and found that he expressly attributes the institution of Rogations to the action of Vesuvius.

[*] In 1846 the Esquimaux of Repulse Bay had plenty of wood, but in 1854 many of them, who had been hoarding up seal skins for the purpose, could not build kayaks, because they had no wood to make the frames of.

North West Greenland. I suppose these build their igloos of stone because they have no wood, and prefer this kind of house to a snow hut, because the walrus which they kill in great numbers, and which, according to Drs. Kane and Hayes, forms their principal food, affords abundance of fat for fuel. A stone house with fire is warmer than a snow hut without it.

As the Esquimaux, of all people I have ever met with, most readily adapt themselves to circumstances at very short notice, I believe that these Arctic Highlanders could under every difficulty build kayaks for themselves, were they absolutely necessary, otherwise how can we account for the Esquimaux in the south of Greenland (whom Mr. Howorth believes to be the descendants of these Arctic Highlanders) having built both kayaks and oomiaks almost identical in form and construction with those in use among the "American Esquimaux" of Behring Strait and the Mackenzie, several thousand miles distant, with whom they could have no direct communication? This has always appeared to me a very curious circumstance difficult of solution, except by supposing that the "Skrelings" crossed Davis' Strait at its narrowest part from Cumberland Island to Greenland, a distance of 200 geographical miles—a theory which I do not think so probable as the one I have already advanced.

"The Arctic Highlanders have become alarmed at the rapid diminution of their numbers through famine and disease."

This feeling is not peculiar to the Arctic Highlanders, for both at Repulse Bay and at the Coppermine River a very similar story was told me.

Between 1847 and 1854, the dates of my two visits to Repulse Bay, forty or fifty of my old friends in that neighbourhood—men, women, and children—had died in one season, and nearly all from starvation, caused, I was told, by one of those erratic migrations of animals I have already mentioned.

Although there is every probability that the musk cattle, of which skulls are found scattered along the shores of Smith Sound, had been killed by the Esquimaux, the "absence of the lower jaws" is no proof that they were so killed. Wolves, foxes, or bears, would carry off these lower jaws and very likely "break them up," but the head itself would be rather an uncomfortable burden for the two first-named animals, and would not afford much nourishment to Bruin, and even his strong teeth would find an old musk bull's skull rather a hard nut to crack if he did attempt it.

"The American Esquimaux never go from their own hunting-range for any distance to the inhospitable north."

It is very difficult to define what "any distance" may mean, but I have known them go several hundred miles in one season to look out for fresh hunting-grounds or seas, either north or south, and if they find game they remain there. If the game moves away, the Esquimaux will follow it, whether north or south, if not stopped as trespassers by some of their own countrymen who have had previous occupation.

When I went to Repulse Bay in 1853, I was surprised and disappointed at finding no Esquimaux—for we wanted dogs from them—where a very considerable number had been in 1846-7. In the spring (1854) we found that none had wintered, as far as we could learn, within 200 miles of our winter quarters. The Chippewayan legend told by Sir John Franklin is well known to the Hudson's Bay Company's people.

The Indians resorted in old times to the deposits of native copper on the Coppermine River to obtain that useful metal, with which to make spear and arrow heads, &c.; and it was probably on one of these occasions that an Indian woman may have been carried off "across the sea" to Victoria on Wollaston Land," some points of which are within sight of and at no great distance from the Continent. Very likely, instead of being kept in slavery, some good fellow made her his wife, and treated her as such, much more kindly than she would have been treated among her own countrymen.

In fact, although the habits of the Esquimaux near the Coppermine have nothing of the Indian in them, the face and form of several that I have seen differ widely from the true Esquimaux type, thereby indicating a mixture of blood or races.

That admirable traveller and keen observer, Mackenzie, "certainly knew the country well," but he did not know much of the Esquimaux, for the simple reason that he had very little opportunity of becoming acquainted with them. As an authority on anything relating to the Indians, either east or west of the Rocky Mountains, no man could be more reliable.

Mackenzie says at p. 406 of his book, "They (the Esquimaux) never quit the coast." I think Sir Alexander Mackenzie meant by this that they never went inland; the only interpretation which would, I think, give his opinion any weight. If in saying "they never quit the coast," he meant that they never crossed the sea or ice to other lands or islands to the north, which he by his own observation could not possibly have known, it would be in perfect contradiction to the Chippewayan legend of the woman being carried across the sea, &c., and to our present knowledge.

It is not at all necessary for the American Esquimaux to cross Behring Strait to enable them to obtain articles of Russian manufacture from the Tchuktchi, nor for these to cross over to America for this purpose. A number of Russian trading-posts have for very many years been established in Russian America (now Alaska), and these traders have carried on a large and direct traffic in articles of Russian manufacture with the Tchuktchi and Esquimaux.

It is that very "fragment" of so-called Tchuktchi, of Tchuktchi Ness, found in the extreme north-eastern part of Asia, and a few of the Kamskatkans, whose language, custom, or physique resemble, to some extent, those of the Esquimaux, which I humbly think give strength to my belief in the original eastward migration of those curious people.

That there may have been a subsequent re-migration, so to speak, of Tchuktchi from America westward across Behring Strait to Asia, is, I think, very probable.

The Esquimaux and Tchuktchi of America, although they meet to trade for mutual advantage, are by no means friends, for they are (or were very recently) often at war with each other.

I can scarcely think that the American Esquimaux have been "sophisticated" by contact with the Indians. At the present time they differ from the Indians in every particular. In their dress, in their manners, in their mode of pitching their tents, of cooking and eating, fishing and hunting, in the form of their fish spears and hooks, in sewing, in the way of treating their wives, &c. Indeed, even at Churchill, where they come much in contact with the Indians, they seem to have acquired none of their habits or customs.

This letter has increased in length far beyond the limits I had contemplated, and I am almost ashamed to forward it to you with any hope of its finding a place in the columns of NATURE, but I felt almost bound to write something: first because an answer of some kind was required to the several arguments so well and ably used by Mr. Howorth, and secondly because I wished to comply with the hope he so pleasantly expressed that I would bring orward some more facts on my side. JOHN RAE

The Aurora of Feb. 4

IN the February number of NATURE just to hand I find an interesting account of this aurora. It may interest your readers to know that a very fine aurora was visible at Eden, 230 miles south of Sydney, at the same time. The notice sent me states that the aurora was visible from 1 A.M. to daylight of Feb. 5 (*i.e.* from 3 P.M. to 7 P.M. Feb. 4, Greenwich time); the auroral light extended from S.E. to S.W., and to an altitude of 60°. No other particulars were sent by the person who saw it; but it would appear that the auroral display must have commenced before it was observed in Europe. H. C. RUSSELL

Sydney Observatory, May 15

THE ZOOLOGICAL STATION AT NAPLES

LETTERS from Naples inform us that the construction of the building for the Zoological Station is now advancing rapidly. As the building is close to the sea, the foundations had to be laid with especial care, the more so as the heavy pressure of the aquarium tanks, the laboratory tanks, the library and the collections, would require even on ordinary ground some precaution.

We are glad to hear besides that Dr. Dohrn is most effectively assisted in the technical parts of the construction by Mr. W. A. Lloyd, of the Crystal Palace Aquarium, Sydenham. This gentleman, having been in friendly relations to Dr. Dohrn some years ago, when still in Hamburg, has obtained from the Board of the Crystal Palace Aquarium permission to render all possible help to the Naples station, as to an institution of a purely scientific character. Whoever knows the technical difficulties of such a construction will be exceedingly glad that so experienced a man as Mr. Lloyd lends his assistance in so disinterested a way to an establishment which we trust cannot fail to exert a powerful influence on the progress of scientific Biology.

We are further informed that Dr. Dohrn has already received considerable presents for the future library of the Zoological Station. The celebrated firm of Engelmann, Leipzig, has sent all works published by it on biological topics, and which had not yet formed part of Dr. Dohrn's private library. The value of these books exceeds 100*l.* Besides this Vieweg, of Brunswick, has sent all that he has published on Biology. Theodor Fischer, of Cassel, the well-known publisher of the magnificent "Palæontographica," has also made very liberal grants. Others are promised. We trust our English publishers will not hesitate to send their publications on biological topics as soon as application is made by the administration of the Station.

The entrance of these books, and of whatever goods may be sent to the Station, is freed from any duty by the Italian Government. Dr. Dohrn hopes to obtain in short time, from a great steamboat company, free transport for all the objects sent from England to the Station.

On all these and other points we hope to give more full and special information at the beginning of next month, when the Committee for the Foundation of Zoological Stations in different parts of the World, will publish its Report to the British Association.

ON ALPINE MAPS

MR. LESLIE STEPHEN says that the Alps are the playground of Europe, and he is not far wrong; but the majority of the boys who frequent this playground are acquainted with only a very few of its nooks and corners, and not a single one of them can pretend to know the whole of it. The Alps are not likely to be thoroughly explored for several generations yet to come; and I doubt not that it will be possible for men half a century hence to spend their ten or twelve seasons in the Alps, and still to find at the end of their time valleys which have not been described or even visited.

The Alps are very imperfectly known, and they are still more imperfectly mapped. A complete map of the entire chain of the Alps is yet wanting. I mean one embracing the whole of the land between the Mediterranean on the south and Munich on the north, and from the valley of the Rhone on the west to the frontiers of Styria and Hungary on the east. I believe it is correct to say that there is no map in existence on a scale of $\frac{1}{500000}$ of nature (or on nearly so large a scale) which contains the whole of the above indicated regions; and even the scale of $\frac{1}{500000}$ is much too small to permit justice to be done to the intricacies of the chain. So long as the Alps remain divided between several countries it is not likely that we shall possess a complete map of them upon an uniform scale; and as it is improbable that boundaries will be rearranged for the formation of an Alpine State, and more so that private individuals will be enterprising enough to meet the want, it is in the highest degree unlikely that a complete map of the Alps will be produced within many years. Nevertheless, materials for a complete map are rapidly accumulating; the greater part of the chain has been surveyed with considerable accuracy; and the attention of the reader is now invited to some of the more important of the maps which have been, or are being, produced after these surveys.

The Swiss Alps have been surveyed with a thoroughness which leaves little or nothing to be desired. The Government map of Switzerland, on a scale of $\frac{1}{100000}$ of nature, in 25 sheets, besides possessing in a most remarkable degree the essential qualities of accuracy and clearness, has, regarded as a whole, a unity and perfection which place it far in advance of any other maps of mountainous countries with which I am acquainted. I have carried it in hand over a large part of Switzerland; and whilst I have never been able to detect anything more than trifling inaccuracies, I have been continually filled with admiration for the consummate ability displayed even in its most minute details, and for its almost faultless expression of every variety of mountain form. It would be a poor compliment to it to say only that one can distinguish upon it slopes from precipices, jagged ridges from rugged ground, and the "ice-falls" of the glaciers from the gentle undulating snowy slopes of the upper regions. One can do much more than that. When its conventional methods of expression have been mastered (and it must always be remembered that line engraving is necessarily conventional), the peaks detach themselves from the valleys and soar aloft, and the mountaineer sees the Alps before him with all their marvellous diversity of architecture; nay, more, he can fix his line of assault, and will say, "Here, if anywhere, the summit may be gained."

The effect of the whole map is as satisfactory as its details. The junctions of the sheets are admirably effected and are scarcely perceptible,* the relief of the hills gradually augments from the lowlands upwards, and the *massifs* are projected—as it were embossed—with astonishing power; whilst the great features of the country (such, for example, as the upper valleys of the Rhone and of the Rhine) are depicted so clearly that a single glance is sufficient to fix them indelibly upon the memory. As a work of art (irrespective of its other merits) this map has extraordinary excellence, and, taken as a whole, I believe it to be perfectly unique.

There were special maps of several of the Swiss cantons before the great survey was undertaken, but the map in 25 sheets is the first general official map of Switzerland that has been produced. It is drawn with what is termed *oblique light*—that is to say, with the light proceeding from the left-hand top corner to the right-hand bottom corner; and this treatment (which the Swiss Staff maintain with reason is the natural and the only effective system possible for a mountainous country) is employed consistently through all the sheets. A great part of the artistic effect of the map when the sheets are joined together is due to this treatment. The system of *central light*, which is sometimes adopted for maps, does not answer for a mountainous country; for it is evident, if it is employed in a map with numerous sheets, that each sheet becomes isolated from the surrounding ones, and unity of effect is impossible.

The claims of the draughtsmen and engravers of the Swiss map are deserving of especial recognition. It could not have been produced in its wonderful perfection unless every person employed upon it had been devoted to the work, and possessed of rare ability. But I believe that the reason of the extraordinary uniformity in the excellence of its parts, and of the unity of the whole, is found in the fact that the entire drawings and plates (and a great part of the survey†) were executed under the direction of a single head, namely, that of General Guillaume Henri Dufour.‡ The map is popularly and properly termed the "Carte Dufour."

The Swiss are keenly alive to the value and uses of good maps, and since 1869 have commenced a great one, which, in its dimensions at least, dwarfs the Carte Dufour to insignificance. The map of which I now write, when complete, will be in 546 sheets. The portion comprising the plains and the Jura will be on the large scale of $\frac{1}{25000}$, but the Alpine portion will be upon a scale of $\frac{1}{50000}$. As yet only 26 sheets are published. The large scale of this map admits of the introduction of details which would have overcrowded the Carte Dufour, and there is, indeed, scarcely anything omitted from it which could be inserted. Even the erratic blocks of the Jura are laid down. The rivers and torrents are printed in blue ink, the lettering and outline of the map is in black, and there is a third printing, in red, for contour lines. Originally it was intended to have had a fourth printing, in green, for the sake of the forests; but the idea was abandoned from considerations of expense. The engraving of the lowland portion of this map is of the most exquisite character, and the Alpine portions (which are lithographic) are extraordinary examples of drawing on stone. The published sheets have not, however, the relief which is such a picturesque feature of the Carte Dufour, and which makes that map so popular and so valuable for educational purposes. Notwithstanding this, the larger map will be found to be incomparably the more useful of the two. The contour lines are laid down on the Alpine portion at a height of 30 metres (= 100 Swiss feet), and on the lowlands at a height of 10 metres above one another. One can therefore determine at a glance the height of any point upon mountains, glaciers, or snow-fields, within a few feet; whilst the largeness of the scale renders it invaluable for purposes for which the Carte Dufour could not be employed.

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* For an example of the admirable way in which these sheets will join together, the reader is referred to a map of the Canton Uri, issued by the Swiss Statsbureau in 1867, which is composed of portions of four of the sheets of the Dufour map.

† The triangulation was commenced at the end of the last century by the cantons, and in 1828 was taken into the hands of the Confederation. The triangles of the first order were brought to a conclusion in 1840 The first sheet appeared in 1845, and the last one in 1864. In connection with the survey the names of Bétemps, Coaz, Mohr, and Colonel Siegfried should be mentioned. The last-named gentleman is, since 1864, director of the Topographical Bureau.

‡ Born 1787; entered the Federal Staff 1817; Colonel 1827.

There is also at the present time a third map of Switzerland being produced at Berne, which is a reduction of the Carte Dufour. This is upon the scale of $\frac{1}{250000}$, and will be in four sheets. Two sheets are already published, a third is complete so far as Switzerland is concerned, but is awaiting details of the Italian Alps, and the fourth sheet is scarcely commenced. The engraving of this map is not less admirable than of those which have been already enumerated; and as it will be sold at 10 fr. for the four sheets, it will be one of the cheapest, if not the very cheapest, copperplate map ever produced.[*]

There are, therefore, existing or in progress three maps of the Swiss Alps, each of which may be adopted with confidence as a basis for other maps; but if we pass to the French, Austrian, or Italian Alps we shall find a great inferiority in the materials at our command. The French Alps have been, perhaps, better surveyed than the Austrian, and the Austrian than the Italian; but all these countries are destitute of maps possessing anything like the perfection of the Swiss ones.

The survey of the Etat Major, for the great map of France in 258 sheets (subsequently extended to 263 sheets, in consequence of the annexation of Savoy), was commenced very many years ago, but there are about 20 sheets still remaining unpublished, and almost all of these belong to the Alpine part of the country. Sheet 189, published in 1866, is indeed the only one yet issued which embraces any very lofty peaks; and this one contains the whole of the so-called Pelvoux *massif*, which includes numerous splendid mountains ranging in height from 3,700 to 4,100 metres. The principal triangulation may have been performed, for aught I can say to the contrary, with the utmost precision; but there is in the engraved sheets of the Alpine portions of the map a general want of the intelligent rendering that is found in the Carte Dufour, and such inexactness in the topographical details that one's confidence is shaken in the whole work. I cite by way of example the *massif* of the Meije (3,987 metres), the highest mountain in the Alps which remains unascended. The glaciers and its ridges, both on the north and on the south sides, are very inaccurately represented, and upon the spot are scarcely recognisable; and in other places, even where attention has manifestly been paid to details, the work is very devoid of character, and has been freely generalised. One can only conclude, in regard to these matters, that the engineers either considered that details of the upper regions were of no importance, or else that they were ignorant of the meaning of the things which they regarded. No sheets have been published to the south of No. 189, and thus the greater part of the department of Hautes Alpes, and the whole of the departments of the Basses Alpes and of the Var (to say nothing of Nice) remain unmapped. It is, I believe, chiefly on account of the want of good maps that so few persons travel for pleasure in this beautiful corner of France. The means of communication are good, and living is cheap, but it is rare indeed to meet with a tourist anywhere; and the solitary pedestrian is likely to be mistaken by the natives (as I have been several times) either for a pedlar selling images of the Holy Virgin, or for a dealer in looking-glasses.[*]

The range of Mont Blanc, which partially reverted to France in 1860, was until quite recently one of the worst mapped portions of the entire chain.[†] Upon the annexation of Savoy, Captain Mieulet, of the Etat Major, was set to work to connect this part of the new territory with the great map of France. But before Mieulet's work was reduced, and indeed almost before his survey was completed, the range was mapped by an amateur, and the honour of first presenting to the world an accurate plan of the most important part of the greatest chain of mountains in Europe was secured by an Englishman. Mr. A. Reilly (the English amateur to whom I refer) in 1863 determined trigonometrically no less than 200 points, and in the winter of 1863-4 reduced his work to shape, and presented his map to the English Alpine Club. Its publication was immediately resolved upon, and in 1864 Mr. Reilly went over the ground again to correct his work. In June 1865 his map was published at the cost of the Alpine Club, in chromo-lithography, upon a scale of $\frac{1}{80000}$ under the title of "The Chain of Mont Blanc." In the mean time it had been represented in Paris that it would be a great advantage for Captain Mieulet to extend his work beyond the frontiers, and he accordingly carried his survey as far as Courmayeur. A special sheet on the scale of $\frac{1}{40000}$ was promptly engraved from the materials he accumulated, and was published in 1865 by order of the late Minister of War, Marshal Randon.[‡] This map, however, included the central portion of the chain only, and Mr. Reilly's map remains, I believe, the only trustworthy complete map of the whole of the chain that is in existence.

In accuracy there is probably little difference between these two maps. The French one is superior to the other in giving numerous altitudes, but the English one has the merits of greater clearness and picturesqueness. Mieulet, on his survey, discovered that the highest peak of the Aiguille de Trelatête was only 3,932 metres, whereas an altitude 1,000 ft. greater had been previously assigned to it. Reilly, on his part, demolished the Pointe des Plines, a fictitious summit, which he showed was identical with the Aiguille d'Argentière, although it had in former maps been laid down as a mountain separate and distinct. Both maps have especial merits, but the Englishman's, from being the work of an amateur, is the more remarkable of the two, and notably illustrates how much a single unaided person may accomplish who makes his work a labour of love.

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[*] All of the Swiss maps are remarkably cheap The sheets of the great map in 546 sheets will be sold at an uniform price of one franc each. The price of the Carte Dufour (which measures 3 50 metres wide by 2 40 metres high) was reduced about three years ago, by order of the Federal Council, from 100 to 40 francs, in order that it might be available for persons of small means. The whole of these maps can be procured from the bookseller Dalp, of Berne, who is the agent appointed for their sale.

[*] Another proof of the rarity of travellers is found in the ignorance of the natives of all kinds of money except their own, and the traveller should well supply himself with napoleons and francs, to avoid loss by exchange

[†] I do not of course in this remark include the Swiss portion of the range, nor the basin of the Mer de Glace. That renowned glacier and its tributaries were well surveyed by the late Prof J. D. Forbes in the years 1842-50, and the resulting map, which was published in 1855, was, I believe, drawn on stone by Dr. Augustus Petermann himself.

[‡] Under the title of "Massif du Mont Blanc, extrait des minutes de la carte de France, lévé par M. Mieulet, Capitaine d'Etat Major."

The condition of the Eastern Alps is even less satisfactory than that of the western part of the chain. The great French map will, doubtless, be completed sooner or later, and when it is finished the Western Alps will be fairly, although only fairly, represented. There is no immediate prospect of an equally perfect map being produced of the Eastern Alps. The map of Lombardo-Venetia, in forty-two sheets, on the scale of $\frac{1}{86400}$, is the most important one which touches these districts; and it is as much inferior to the great French map as the latter is to the Carte Dufour. Those who are intimately acquainted with these regions point to its too great generalisation of details and to its want of character, and observe that it (like other maps published at Vienna), although meritorious for its day, is now behind the times. Sundry amateurs have done good work in recent years in correcting or laying down afresh several of the *massifs* of the Eastern Alps, and amongst these individuals none are more worthy of mention than Mr. Tuckett and Lieutenant Payer. In the summer of 1864, Mr. Tuckett, of Bristol, devoted some time to the exploration of the Orteler group, and subsequently published a paper in vol. i. of the *Alpine Journal*, entitled "Contributions to the Topography of the Orteler and Lombard Alps:" This paper was accompanied by numerous outline sketches, and by a map. The latter, although roughly executed, gave, it is believed, for the first time with some approach to accuracy the positions and forms of the glaciers on the south side of the chain throughout the length of Val di Zebru, with the basins of the Vitelli and Nagler glaciers, and the lower portions of those of Sulden and Forno. All these were more or less incorrectly laid down (when represented at all) or vaguely indicated on the Government Maps. Further topographical corrections in connection with the Southern Orteler Alps (the result of visits in subsequent years) were recorded by Mr. Tuckett in the 2nd vol. of the *Alpine Journal*. With the labours of Lieutenant Payer the readers of this journal are already acquainted.*

Of the Alps of Piedmont there is no map in existence upon which dependence can be placed. A survey is said to be in progress, which will be eventually worked out upon a mammoth scale, and this will afterwards be adopted as the basis for a reduction of more moderate size. Many years must elapse before either of these maps can be produced, and in the meanwhile the old Sardinian Government map will be almost the sole authority. The shortcomings of this map are notorious, and one wonders at the rare ability of the draughtsmen who were employed upon it in projecting mountains which do not exist. The case of Mont Tseran is one of the most flagrant instances, but others could be quoted scarcely less audacious. Mont Tseran is laid down on sheet thirty-seven of the Sardinian map upon the northern side of the valley of the Arc, not far from the source of the river, and is credited with an altitude of 4,045 metres. There is no important mountain upon the spot which it is supposed to occupy, and Mont Tseran may be considered to be absolutely mythical. The late Mr. Cowell demonstrated that the peaks in this neighbourhood do not anywhere approach the height of 4,045 metres,† and those who cross the Mont Cenis Pass by the old road can easily satisfy themselves that no great mountain occupies the ground whereupon Mont Tseran was located by the Sardinian surveyors.

Several amateurs have endeavoured to reduce the Piedmontese Alps to a little order. Mr. Reilly (whose name has already been mentioned in connection with the chain of Mont Blanc) carried on a survey of the southern branches of the central Pennines in the years 1865-6, and afterwards projected his work upon the scale of $\frac{1}{100000}$. His map was published at the expense of the Alpine Club, and it is, I believe, the only one which at all fairly represents the southern side of Monte Rosa, the valleys of Valpelline, Barthelemy, and Tournanche, and the ranges which divide those valleys. Mr. R. C. Nichols has devoted several seasons to clearing up the topography of the Graians, and has from time to time communicated papers to the "Alpine Journal," some of which are illustrated by maps. But the full extent of his labours will not be known until a map is published, about which I have now a few words to say.

Some six or seven years ago the want of a general map of the Alps was a topic of conversation amongst those who habitually frequent those mountains, and ultimately a committee of the Alpine Club was appointed to superintend the production of a new map which was intended to rival the Carte Dufour in accuracy, and to comprehend the entire chain. The preliminary investigations quickly discovered that the plan must be modified, on account of lack of data; and it was at length determined to limit the scheme to the Central Alps, to the exclusion of the most eastern and western ones. Mr. William Longman, the eminent publisher, accepted the financial responsibilities, and Mr. Nichols was appointed editor.

This map will be in four large sheets on the scale of $\frac{1}{200000}$, and is being engraved on steel. It is now in course of production at the geographical establishment of Mr. Stanford, and it promises to be one of the most minutely and beautifully engraved maps ever published in this or in any other country. Its sheets are somewhat larger than those of the Swiss map on the scale of $\frac{1}{250000}$ and it does not extend quite so far to the north as the Swiss map, but in the south it embraces the important groups of the Graian Alps to the south of the Valley of Aosta, which include the Valleys of Locana, Cogne, Savaranche, Rhèmes, Grisanche, and the Tsère, with their peaks Tour de St. Pierre, Grivola, Grand Paradis, Grand Apparei, Aig. de la Sassière, Mont Pourri, and the Ruitor. All of these valleys and mountains are not included in the Swiss map. In the west the boundaries of the two maps are identical, but in the east the English one extends 10 kilometres beyond Landeck, thus taking in the Orteler group, the Zufallspitze, the Adamello and Presanella (all of which mountains will be just without the range of the Swiss map), but stopping short of the Oetzthal group, and thus excluding a great part of the Tyrol, the Bavarian Alps, and everything beyond. The map is expected to be completed in about two years, but it is not at the present time sufficiently far advanced to permit of an elaborate criticism. I believe, however, that it will be found to be little or not at all inferior to the Swiss maps in finish of engraving, in its relief, and in its accuracy, but will hardly be so clear as they are, in consequence of the introduction of details which would have been sufficient for a map of four times its scale. Still its appearance will be welcome to those who travel amongst or who are interested in the Alps, and it is to be hoped that the scheme as at first propounded will one day be carried out to its fullest extent.

In the foregoing rapid survey of maps of the Alps it has only been possible just to glance at some of the principal ones; but this glance has, I trust, enabled the reader to understand that an unlimited amount of work remains to be accomplished before the Alps can be said to be thoroughly explored, and that a splendid field still remains open for the employment of superfluous energy of men who desire to distinguish themselves. The efforts of individuals are scarcely perceptible upon so vast an extent of country; but a body of zealous observers, spread over its various districts, might break the neck of the work in a few years, and render it possible to produce for the first time a map of the entire Alps upon a uniform scale.

EDWARD WHYMPER

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* Papers have appeared from time to time upon Lieut. Payer's work in the pages of Petermann's "Mittheilungen."

† See "Vacation Tourists and Notes of Travel." London, 1861.

EVANS'S STONE IMPLEMENTS OF GREAT BRITAIN[*]

WHEN Shakespeare represented his philosophical Duke, as finding "sermons in stones," and "books in the running brooks," he was but unconsciously exhibiting the prophetic faculty which has been attributed to all true poets. He could hardly have foreseen that his pretty yet fanciful conceit would one day be found to be sober earnest. But so it is; we have here a goodly volume of more than six hundred pages, illustrated by nearly as many excellent woodcuts, discoursing learnedly of nothing save stones and streams, and finding in them sermons of great and, to many readers, novel interest.

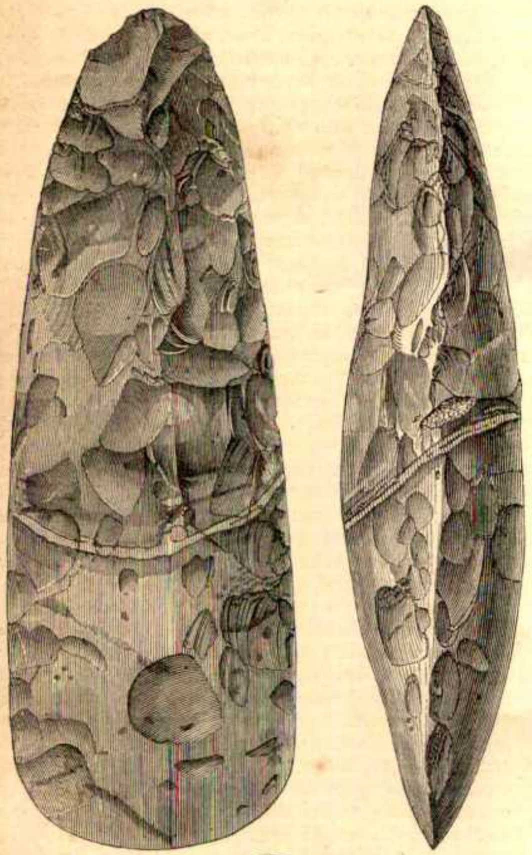

FIG. 1.—POLISHED CELTS, SANTON DOWNHAM, SUFFOLK

It might have been supposed, when Mr. Evans had published his well-known work on "The Coins of the Ancient Britons," that he had gone back as far as possible in the history of our land and nation; but in archæological as in other sciences, there is in the lowest known depth one lower still remaining to be fathomed; every chamber opened to the light discloses others lying beyond it. From a people who had no literature, or none of which they have left any trace beyond the rude characters inscribed on their rude coins, we are now carried back to tribes and races which possessed neither coins nor letters; people who have left us neither their sepulchres nor their ashes, nor indeed any trace of their existence, save the rude triangular or subtriangular fragments of worked stone which served them for tools or weapons; and even these are usually found buried beneath the wreck and ruin, it may be, of continents or islands which have long since been worn and wasted away.

The publication of this work is remarkable as an evidence of the quickened pace which characterises scientific research in our days. Palæontology and Geology, vigorous and flourishing as they are, are still hardly "out of their 'teens;" but Prehistoric Archæology has made comparatively more rapid progress than either. Not more than fourteen years have passed since the discoveries made by Boucher de Perthes of flint implements in the gravel beds of Abbeville and Amiens, although at that time discredited and disparaged by the geologists of his own country, were confirmed and supplemented by Mr. Prestwich and Mr. Evans. Previously to that time these objects had attracted but little notice; the things were "neither rich nor rare;" men looked at them and wondered, and then forgot them, just as before William Smith's time they gazed with a profitless curiosity on fossil shells and bones, and thought with Dr. Martin Lister, that they might be "the efforts of some plastic power, in the earth, being the regular workings of Nature, whereby she sometimes seems to sport and play, and make little flourishes and imitations of things, to set off and embellish her more useful structures."

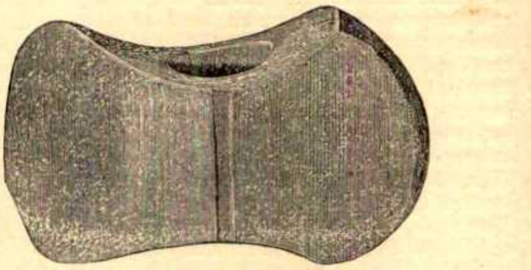

FIG. 2.—AXE-HAMMER, THAMES, LONDON

But since the discoveries in the Somme Valley were recognised, a flood of light has been shed upon the subject. These dry bones live, and these rude stones are found to be useful, indeed indispensable, materials for building up the earliest history of the human race. The *savans* of every country in Europe have hastened to take part in an inquiry so novel and so interesting; many volumes of memoirs have been written; our French neighbours, with their usual vivacity, have established a journal devoted to Prehistoric Archæology, as well as an annual *Congrès;* and these researches having been for several years conducted by so many able and eager observers, we need not wonder that Mr. Evans, having studied the whole bibliography of the subject both ancient and modern, and explored every considerable museum or collection, is now enabled to produce this Encyclopædia of the new-born science, which for want of a better word may, perhaps, be called Petrology or Petro-tomology. He has introduced us into the workshops and armouries of our most remote predecessors, it may be of our ancestors, as they existed not at any particular epoch, but in all probability through a long succession of ages; and he has shown us so clearly what were their weapons and tools, of which any vestiges remain, and how they were made and used; and has correlated them so accurately, as far as might be, with similar objects found in all quarters of the globe, as well as with those described by classical writers, or in use by modern savages, that in reading his work we know not which most to admire, the industry shown in the collection and examination of such a vast amount of material, or the skill with which the information thus obtained has been methodised and arranged. The book completely exhausts the subject, and will long continue to serve as a perfect manual for the collector, as well as furnishing most useful materials for archæologists and anthropologists.

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[*] "The Ancient Stone Implements, Weapons, and Ornaments of Great Britain." By John Evans, F.R.S., F.S.A. (London: Longmans and Co., 1872.)

Those who are not already somewhat versed in this science will be astonished to learn the infinite variety of uses to which the apparently stubborn and unmanageable rock called flint has been converted. We may, perhaps, doubt if in the very earliest ages it was used for purposes of warfare, and we prefer to give our progenitors the benefit of that doubt, and to believe that those were "golden ages"—times of primitive piety and peace; and that it was only for purposes of husbandry, and the chase, and domestic use that they worked up the materials found in their plains and valleys. Thus, we find descriptions of celts, or axes for felling trees, or hewing canoes, hoes, threshing machines—as now used in the East—or perhaps harrows, scrapers for preparing skins, arrows for birds or other "small deer," knives, gouges, saws, mullers or pounding stones, chisels, hammer axes or picks, and polishing or grinding stones, of which there must have been great need; nor were the women of the period left destitute of their share of the stony spoil; for we find in these pages descriptions and figures of rings, armlets, amulets, spindle whorls, pestles, and, in the cave deposits, needles of bone of admirable workmanship, which might have been, and probably were, drilled by flint flakes.

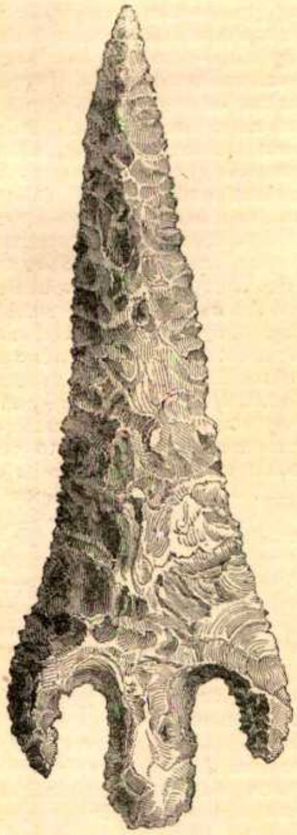

FIG. 3.—ARROW-HEAD, ISLE OF SKYE

As these primitive people have left us no record of their progress in arts and manufactures, and the material evidences bearing on the subject are found in a very confused and dislocated condition, it is a work of no small labour to classify and arrange them in order of date, or rather of sequence, and thus none but a rough and wide scheme of classification is possible. The Danish and French authors, as well as many of our own, usually divide the stone implement period into two principal stages only, the Palæolithic and Neolithic—unpolished and polished; placing them both before what has been called the Bronze age. This arrangement, however, although found convenient for popular use, and in that sense adopted by Mr. Evans, can hardly be regarded as scientifically accurate; as he has himself observed, there are blanks in the chronology of stone implements, which it is hard to fill up. The classification may be, and indeed is, too wide in one respect, and too limited in another. Whilst, on the one hand, the drift and the cave implement periods, which are usually bracketed together as Palæolithic, are characterised by very various conditions, both palæontological and geological, and, indeed, technological also—conditions which may indicate their separation by a vast interval of time; so, on the other hand, as Mr. Evans has shown at the close of the fourth chapter, some of the unpolished stones, chipped or rough hewn celts, were probably of a date not earlier than some that were ground and polished; and, in Great Britain, at least, there are not wanting indications that the use of bronze was coeval with the polished stone period, if not, indeed, with one or two exceptions (which were probably imports) anterior to it.

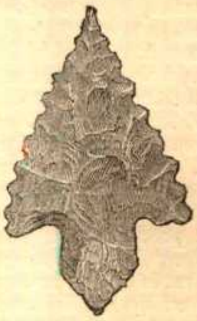

FIG. 4.—URQUHART

One of the most perplexing questions suggested by the discovery of the drift implements relates to the means by which they came into their present position. They are often met with at a depth of twenty or even thirty feet, usually at or near the base of thick beds of coarse flint gravel, which in its turn is overlain by masses, more or less thick, of brick-earth or loess. Occasionally, and indeed not rarely, they occur entirely beneath the gravels, and on the surface of the subjacent rock, whatever it may chance to be. Mr. Evans deals with them merely as constituent portions of the beds of sand, gravel, and clay, in which they occur, and so indeed they now are, but they are something more. Although of the drift, drifty, each has its own separate history; for each has been held and fashioned by hands guided by an intelligent will, and thus

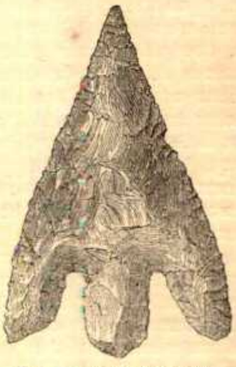

FIG. 5.—ABERDEENSHIRE

we are led irresistibly to inquire when, and why, and how did they come where we now see them, and why are they never found on the surface, nor under any other conditions?

To a certain extent this inquiry is involved in the far larger question of the forces by means of which the superficial gravels, of which the implements are as it were but the accidents, became dispersed—a subject which does not necessarily come within the scope of a work designed to be technological rather than geological. Mr. Evans has, however, very judiciously devoted one of his chapters to it; and as it is one of great interest, and is still involved in much obscurity, we may gladly welcome any attempt to deal with it, especially by one who has given so much attention to its investigation.

It was the opinion of the late Dr. Buckland, an opinion which was concurred in by Greenough, Conybeare, and other able writers of their time, that the general dispersion of gravel, sand, and loam, over hills and elevated plains, as well as valleys, was the result of a universal deluge, which is described as transient, simultaneous, and of a date not very remote; that the existing system of valleys was mainly due to the same cause, and that thus both valleys and gravels preceded our present river systems. Cuvier, and the French geologists generally, have held the same opinion, but of late years it seems to have been altogether discredited by English authors, with perhaps the exception of the late Sir Roderick Murchison. We may well entertain doubts as to the occurrence of a deluge that should be both universal and simultaneous; and it is probable that it is chiefly on that account that Dr. Buckland's theory has met with so little favour. Still, although we may be unable to adopt his views in their entirety, his statements as to the diluvial characters of the English drifts seem entitled to some further consideration before they are set aside altogether, and on this account it is fortunate that the recent discoveries of flint implements have excited so much interest in the gravels in question as to induce Mr. Evans to devote no inconsiderable portion of his work to the history and antiquity of the River drift.

In the last chapter he has adduced an elaborate argument in favour of the belief in fluviatile transport as opposed to diluvial, by showing first hypothetically the possibility that "deposits now occupying the summits of hills have originally been formed in and about river beds," and then, by reference to the actual phenomena, the probability that the implement-bearing beds were thus formed. No one can doubt, upon the hypothesis here stated, that rivers may have possessed at one time a far greater power of excavating and deepening their channels than now; but then the author is obliged to assume the prevalence of several conditions, and notably a far more rigorous climate, and a greater amount of rainfall; conditions as to which we have but little evidence, and some of that is of a doubtful tendency. If, as is now supposed, the hippopotamus and elephant and rhinoceros remained here all the winter, they would have fared but badly, had the climate been as severe as is supposed.

But passing by these topics as not bearing very immediately upon the question of transport, it cannot be doubted that submergence, by means of diluvial action, is quite possible, since we have many instances of it within the historical period, and some indeed within the last few years; and both modes of transport being alike possible, the probabilities of the case have alone to be considered; and, notwithstanding the various reasons so ably stated by Mr. Evans, it does not seem that there are sufficient grounds for rejecting Dr. Buckland's theory, and there are besides some inferences to be drawn from the position of the implements which, so far as they are concerned, are at variance with the theory of fluviatile transport. For instance, when met with in valleys, it appears that the implements are not found along the whole course of those valleys, as well where flint gravels are wanting, as where they abound, as would have been the case had they been carried down promiscuously by the streams from time to time; but, only in certain limited areas, and then usually in large numbers, and at about the same levels; and further, that in several of these deposits the implements are distinguished from those of neighbouring deposits by some slight difference in form. From these indications it may be inferred that they were made and left at or near the spots on which they are found, and afterwards covered up, and occasionally displaced, by the masses of drifted material which now overlie them; and this seems the more probable, when it is seen that some of them were formed from stones of the same kind as those composing the beds in which they rest, and that some of these appear to have lain exposed upon the surface for long periods before they were worked.

If, indeed, it had happened that these things had never been found elsewhere than in river valleys, the conclusions arrived at by Mr. Evans would have been irresistible, but so far from this being the case, it is certain that these implement-bearing gravels are occasionally found on the extreme margin of sea cliffs, or isolated hills on the verge of far-stretching plains—situations to which no river flowing in the same channels, and draining the same areas as now, could have carried them.

Mr. Evans has noticed several of these deposits as met with at Bournemouth, the Reculvers, and the Foreland cliffs in the Isle of Wight (to these probably should be added Southampton, and Brandon Down, and some others); and he has also alluded to the remarkable discovery in the Madras Presidency of implements of quartzite of true drift type, found on the cliffs at an elevation of three hundred feet above the sea, in a bed of ferruginous clay which forms the coast line for several hundred miles, and is intersected at right angles at various intervals, by the rivers of the country in making their way to the sea.

In all these cases all traces of the ancient rivers, if indeed they ever existed, have been entirely effaced; neither channels, nor outlets, nor adequate water-sheds, nor a single land or river shell, remaining to testify of them; and not only so, but we find many deposits of quaternary gravel (which Mr. Evans justly concludes to be of the same geological period as those of the implements, and to owe their existence and position to the same causes) on hills which could not have been reached by modern rivers. The whole country would have been a vast lake before such heights could have been submerged; and under such circumstances it may be fairly assumed that the same forces, whatever they were, that covered the hill-tops, may have partially filled up the valleys; the presence of gravel may suggest, but cannot prove, that the river brought it, however much it may have re-arranged and sorted it; both valley and gravel may have had an existence before the river began its course. We have many valleys and gravels without rivers, and rivers without gravels; they can very well exist apart, and, doubtless, have often done so.

(To be continued.)

THE HUNTERIAN MUSEUM [*]

ONE of the most interesting features in connection with the annual election of Fellows into the Council of the Royal College of Surgeons, is the exhibition of additions about to be made to the Museum, and which have accrued since the last meeting. Prof. Flower, the Conservator, states in his Report that in the pathological collection eighty-eight additions have been made, against sixty-two during the past year, and that the microscopical characters of all recent specimens sent to the College, and thought worthy of preservation, had been carefully described and delineated by Mr. Goodhart, the Pathological

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[*] From the *Medical Times and Gazette*.

Assistant, on whose great zeal a well-deserved eulogium is expressed.

Although but very little has been expended in the purchase of osteological specimens, the additions are of considerable importance. Amongst those specially mentioned is the skeleton of an extremely rare whale, the *Berardius Arnuxii*, purchased for the Museum by Prof. Wilson, F.R.S. The series illustrating the structural peculiarities of the human race has received two very valuable additions in skeletons of both sexes of the extinct aboriginals of Tasmania, presented by Mr. M. Allport, of Hobart Town. Both have been articulated. The male is in exceedingly good preservation; that of the female is unfortunately less complete, but still presents many interesting features.

The liberality with which the Smithsonian Institution of Washington is conducted has been shown by various important additions to the museum in former years, but especially during the present, in a series of skulls and skeletons of North American mammals, several of which were new to the collection. The College has reciprocated this liberality by presenting a set of casts of the interior of crania of various races of men and animals, and some duplicates.

Another important addition is the skeleton of an adult male Porbeagle shark, about eight feet long. This is valuable, from the great difficulties which have usually attended the attempts made to preserve the osseous system of cartilaginous fish. These difficulties have been overcome by the great skill of Mr. James Flower, the experienced articulator to the College. This will be found a very instructive specimen to students.

In the dermatological collection Mr. Erasmus Wilson still shows the deep interest he takes in it by presenting twenty-five beautifully executed models; and M. Voillemier, Surgeon to the Hôtel-Dieu, Paris, has presented two, representing elephantiasis of the scrotum, with its cure resulting from operation.

Amongst the contributors are Profs. Gervais, of Paris, and Peters, of Berlin; Sir William Fergusson, who gives a great number of preparations; Sir James Paget, Messrs. Hilton, Holden, Busk, Clark, Gay, Curling, Wilson, Jackson, the Zoological Society, and the Smithsonian Institution. Mr. Kiernan, a late member of the Council, has presented the whole of his collection.

During the day of election the theatre, where these additions are displayed, was visited by a large number of the Fellows. It remains open for the inspection of the members and their friends for a few days.

NOTES

We reprint in another column a document which will be read with the greatest indignation by scientific men in every part of the world, and with shame by all Englishmen. We refer to the remonstrance presented to the Treasury by eleven of our most distinguished scientific men against the treatment which the eminent Superintendent of the Royal Gardens at Kew has received at the hands of his official superior, the Chief Commissioner of the Board of Works. Though we might take exception to some of the arguments brought forward by the memorialists, yet there can be but one opinion that the systematic discourtesy and want of consideration with which Dr. Hooker has been treated is entirely incompatible with the efficient conduct of the department of the public service with which he has been entrusted, and deserves the most searching inquiry on the part of the Government.

Prof. Agassiz's Second Report to the United States Coast Survey of the scientific results of the *Hassler* expedition, a portion of which we reprint this week, is one of the most important contributions to scientific knowledge which has appeared for some time. His remarks on the glaciation of South America, and the origin of the so-called "raised beaches" of Patagonia, are especially valuable.

The short communication from Dr. Anton Dohrn on the present position of the Zoological Station at Naples will be read with great interest. The mode in which the Italian Government is assisting this important undertaking forms an instructive contrast to the action, or rather want of action, of our own Government in the matter of the Tides Committee to which we recently alluded. When will Englishmen be able to compare with any other feeling than that of shame the policy of their own Government with that of any other civilised nation of the world in all matters relating to Science?

According to the constitution of the Institution of Civil Engineers, it consists of three classes, viz., Members, Associates, and Honorary Members, with a class of Students attached. Of these several grades there were on the books on the 1st inst. 756, 1,127, 16, and 243, respectively, making in the aggregate 2,142. The increase in the last three months in the different classes has been 11, 36, 2, and 9, representing an effective addition of 58.

We understand that Prof. Palmieri's account of the late eruption of Vesuvius is being printed at Berlin by Link and Reinke, and will be published in several languages. It will be illustrated by five lithographs taken from photographs.

M. Delaunay, director of the National Observatory at Paris, has taken possession of the Observatory at Montsouris.

The Society of Sciences of Haarlem has named the following subjects for competition for essays, to be sent in before January 1, 1874:—1. A study in detail and exhaustively of the influence which chemical and physical modifications of the solvent exercise on the form of carbonate of lime when deposited from aqueous solutions. 2. A critical examination of the various hypotheses on the origin of polar auroræ, and the relations between the polar auroræ and other natural phenomena. 3 On the changes which stone-fruits undergo during their development. 4. On a satisfactory means of determining the temperature, the degree of humidity, and the density of the atmospheric air at a considerable height above the surface of the earth. Also before Jan. 1, 1875:—An exhaustive study of some Linnean species, chosen from those which present more or less divergent forms. The prize for each of these Essays consists, at the choice of the author, of the ordinary gold medal of the Society, or a sum of 150 florins, with an additional 150 florins if thought worthy. The memoirs may be written in Dutch, French, Latin, English, Italian, or German.

Mr. L. H. Courtney, M.A., Fellow of St. John's College, Cambridge, has been appointed Professor of Political Economy at University College, London, in the room of Prof. Cairns. Two Slade Scholarships in Fine Arts, tenable for three years, have been conferred at the same Institution on Miss E. M. Wild and Miss B. A. Spencer.

At a meeting of the Council held on July 6, the Associateship of the Royal School of Mines was conferred on the following gentlemen:—Mining and Metallurgical Divisions—W. Charlton, O. Pegler. Mining and Geological Division—G. M. Dawson. Mining Division—E. Dillon. Metallurgical Division—F. W. Harrold, J. H. Huxley, O. F. Mondy, A. G. Phillips. The following awards were made:—First year students—The two Royal Scholarships of 15*l*. each to Mr. S. A. Hill and Mr. J. Taylor. Second year students—H.R.H. "The Duke of Cornwall's" Scholarship, 30*l*. for two years, to Mr. Edgar Jackson, and the Royal Scholarship of 25*l*. to Mr. C. Law, and the Murchison Medal and Prize of Books to Mr. S. W. Davies. Third year students—The Edward Forbes Medal and Prize of Books in Natural History and Palæontology to Mr. G. M.

Dawson. The De la Beche Medal and Prize of Books in Mining to Mr. W. Charlton.

THE forty-fifth annual session of the German Society of Naturalists and Physicians will be held in Leipzig next month. The meetings will commence on Monday, August 12, and end on Sunday, August 18. The secretaries announce that visitors from foreign parts will be welcome.

THE Vicar of Folkestone has written to the *Times* stating that the memorial west window which it is proposed to place in the parish church to commemorate Harvey will consist of eight subjects from Our Lord's Miracles of Healing, and that underneath it will be recorded on a brass Harvey's great discovery. This project is, we believe, entirely apart from that set on foot by the committee of which Dr. Bence Jones is treasurer, whose object it is to erect a statue to Harvey at Folkestone.

THE death of Rev. M. A. Curtis, of Hillsborough, North Carolina, which took place in April last, must be considered as among the most serious losses that natural history in the United States has experienced for some time. The attention of Mr. Curtis was directed especially to the fungi, although he was more or less familiar with all the plants of the Southern States. In his special department, however, he stood at the head of American botanists, occupying the position in this country of Mr. Berkeley, his correspondent, in England. Mr. Curtis prepared the report upon the fungi collected by the United States exploring expeditions, and has contributed various articles to the literature of this science. Among other works nearly ready for the press was one upon the edible fungi of the United States, illustrated by figures and descriptions of some sixty species. The qualities of most of these he had personally tested by experiment, particularly during the late war, when the food question was for some time a matter of serious moment.

A LATE number of the *Chemical News* contains a suggestion for the formation of an "Association of Manufacturing Chemists," which might hold annual meetings after the fashion of the Iron and Steel Institute, at which topics would be discussed connected with the various important branches of chemical manufacture. It is obvious that meetings of this kind, and papers read to such a body, with the discussion which would result therefrom, might be in the highest degree beneficial to a large body of men engaged in a manufacture requiring a large amount of scientific knowledge and acquaintance with technical details.

A WRITER in the *Food Journal* points out the advantages, to the Central Authority and to the Government, of a National Registration of Sickness. As a basis for sanitary legislation, as an indication for interference on the part of the Central Authority in local sanitary affairs, as a test of the health conditions of the country, as well as of its separate parts, no mass of information could bear comparison with that which would result from a well ordered registration of diseases.

Harper's Weekly records the safe arrival of Mr. Henry W. Elliott at the island of St. Paul, Behring Sea, on April 28. He left Washington last March under an appointment from the Treasury Department, as an assistant of Captain Bryant in looking after the interest of the United States in connection with the fur seals on the Pribylow Islands. Mr. Elliott expects to spend some time at St. Paul, and to devote much attention to the study of the islands, and in collecting specimens of their natural history. Wielding a ready pencil, he also proposes to make sketches from life of the fur seal, the walrus, sea-lion, &c., so as to give us a better idea of their general appearance than we can gather from the stuffed caricatures in public museums.

IN addition to Prof. Agassiz's Report, given in another column, several communications have been received from gentlemen connected with the *Hassler* expedition, all bearing testimony to the zeal with which the operations of the party have been prosecuted. Under date of March 12 it was reported that, after leaving Montevideo on Feb. 20, the dredging work was carried on with due diligence and with very interesting results, numerous radiates of great beauty, many of them undoubtedly of new species, being brought to light. Among the objects of particular interest were the floating stems of the giant kelp of the South Pacific, *Macrocystis*, which sometimes attains the length of 500 to 1,000 feet, the stems being a foot in diameter, and resembling trees in magnitude. The vessel entered the Straits of Magellan on March 13, and passing through, arrived at Talcahuana, in Chili, on April 11, where it was expected to remain several days for repairs and provisions. Prof. and Mrs. Agassiz proposed to spend the interval in a visit to Santiago and Valparaiso, while Count Pourtales and his assistants were to make a line of deep-sea soundings and dredgings from Talcahuana to the island of Juan Fernandez and back to Valparaiso.

THE United States Coast Survey has received advices from Mr. William H. Dall as late as May 5. At that time he was at Illulluk, Oonalaska, the head-quarters of his explorations. He has been diligently engaged through the winter in carrying out the objects of his mission connected with the geography and hydrography of the Aleutian Islands, as well as the weather would permit, a constant succession of storms of extreme violence having interfered greatly with his work. The lowest temperature recorded by him during the past winter was 13°, the average from October to March being 33°. Mr. Dall has devoted such time as he could spare while detained in port, to making collections of natural history specimens, and has obtained quite a number of forms, some of which he considers new to science.

A PROPOSITION has been entertained to tunnel under the Strait of Canso, between Nova Scotia and Cape Breton, where the strait is only two and a half miles wide, for the purpose of connecting the island of Cape Breton with the mainland. The cost is estimated at 2,500,000 dols. This idea is connected with a proposition to run a line of steamers from Glasgow, or other British port, to Louisburg, the most easterly point of Cape Breton.

THE *Panama Star and Herald* records the first arrival on April 2, at Panama, on its annual eastern migration, of the beautiful sphinx moth (*Urania leilus*). The immense flights of this moth, and the extreme regularity of their recurrence year by year, have repeatedly been dwelt upon by the *Star*, and much interest has been excited as to its starting-place and ultimate destination.

THE Report of the Winchester and Hampshire Scientific and Literary Society for 1870-71, congratulates the members of the Society on the successful results of the second year of its existence. Though no important discoveries were due to the labour of the Society during the year, and the weather was very unfavourable for out-of-door work, steady progress was made in some departments, especially in the list of the flora of the neighbourhood, which now includes nearly 700 species. Abstracts of papers on various branches of science, read at the meetings of the Society, are printed in the Report.

THE Malvern, Bath, and Woolhope Field Clubs met for a joint excursion on May 17, an account of which is printed in a separate form, and to it is appended the Report of the Annual Meeting of the Malvern Field Club, with the Address of its President, Mr. Edwin Lees.

A NEW journal has been recently started in New York called "Handicraft: a popular Journal of Progress of the Industrial Arts, designed for workers and thinkers." The number on our table contains papers and short paragraphs, on inventions in the mechanical arts, illustrated with woodcuts, with notices of books, &c.

MR. AYRTON AND DR. HOOKER

THE comments which have been made by several London journals upon the circumstances referred to in the subjoined memorial, seem, in the interest of all persons concerned, to render its publication in its entirety desirable. We cannot doubt that any delay which has occurred, or may occur, in the delivery of a reply to the memorialists, is due to the desire of the Prime Minister to deal with the important question at issue in a manner which, while it is not inconsistent with the interests of the public service, is not incompatible with a due regard for the interests of Science, and for fair dealing towards a very eminent scientific man.

To the Right Hon. W. E. Gladstone,
First Lord of the Treasury, &c. &c.

We, the undersigned, deeply interested in the condition of English science, and viewing with special concern the treatment which the eminent Director of the botanical establishment at Kew has systematically received at the hands of Mr. Ayrton since his appointment to the office of First Commissioner of Works, do most respectfully beg your attention to the following statements and observations.

In the year 1840 the private Botanic Gardens of Kew, which had previously been in the possession of the Royal Family, were handed over by the Queen to the Government.

A commission then appointed to report on their condition recommended that they should be enlarged and maintained as a national scientific establishment, which should form a centre of reception for the useful products of the vegetable kingdom, a centre of reference and distribution for England, India, and the colonies, and a means of augmenting the rational pleasure, increasing the knowledge, and refining the taste of the English public.

The late Sir William Hooker was at that time Prof of Botany in the University of Glasgow. The founding of an establishment like that contemplated at Kew harmonised so completely with his scientific tastes and power of organisation, that, at a sacrifice of more than half his income, he offered to undertake the superintendence of Kew Gardens. His offer was accepted, and he was appointed Director of Kew, at a salary of 300*l.* a year.

Sir William Hooker was at that time the possessor of an excellent private herbarium and of a scientific library, both of which were wanting at Kew. To provide house-room for these an additional 200*l.* was granted by the Government. No allowance, however, was made for the maintenance or increase of either the herbarium or the library. The expense of both fell upon the director.

During his residence in Glasgow, the excellence of his collections had attracted to the house of Sir William Hooker various active investigators, the number of which increased materially after his arrival at Kew. Fourteen rooms of the house he occupied were devoted to his herbarium, which for twelve years was the resort of the scientific botanists of Europe. Unaided by the Government, save to the extent above mentioned, Sir William Hooker devoted his private means to the purchase of new books and specimens, and opened a correspondence with botanists of all lands. He thus made his house the most extensive botanical laboratory in this country, and the most important centre of reference regarding systematic, economic, and descriptive botany, as illustrated by his herbarium.

The gardens expanded equally under his vigorous and enlightened supervision; in ten years after his appointment they became the first in the world.

For twenty-five years he had been collecting textile fabrics, drugs, gums, dyes, and other products to illustrate the structure, uses, and physiognomy of plants. With these collections, made at his private cost, Sir William Hooker founded in Kew Gardens the first museum of the kind that had ever been established. Of such museums there are now three at Kew. They contain upwards of 50,000 named objects of scientific and economic interest, views of tropical vegetation, and maps illustrating the distribution of plants over the globe. These museums constitute concrete courses of instruction, unrivalled in concentration and completeness; and the public interest in them is proved by the number of persons who avail themselves of the stores of information thus provided.

The contributions of Sir William Hooker to these museums were his free gift to the country, for which he never received a farthing of remuneration.

In 1852 the director's salary, which had previously been raised to 600*l.* a year, was augmented to 800*l.*, together with a house which had become vacant at the time. The herbarium was then lodged in a separate building, and immediately afterwards donations and legacies (some to the director, some to the Government of the day) poured into it. The labour of naming the collections of expeditions, and of drawing up botanical reports, became at length so excessive that the public need of the herbarium was still further recognised by the Government. The director had previously borne the expense both of assistance and maintenance; of these he was now relieved, though he still continued to bear the cost of books for his library and of new specimens of plants.

Without this personal devotion on the part of the director, the development of Kew would have been a simple impossibility. For five-and-twenty years his purchases were made and his collections elaborated at his own expense and risk, though they were constantly employed in the work of the country. Before his death, knowing that his son could not afford to be as regardless of pecuniary considerations as he had been himself, he gave directions to have his herbarium valued by competent persons and offered to the Government at the lowest valuation. On these terms the collections which had previously been devoted to the nation's use became the property of the nation itself.

This is a brief but sufficient statement of the relationship of Sir William Hooker to Kew Gardens. It shows him to have been their virtual creator.

The antecedents and achievements of the present Director of Kew may be thus sketched. In 1839 Dr. Joseph Hooker was appointed Assistant-Surgeon and Naturalist to the Antarctic Expedition, the most perilous, perhaps, that ever sailed from these shores, and the scientific results of which exceeded in importance those of any other naval exploring expedition of this century. During this voyage Dr. Hooker received from the Government the pay of his rank as a medical officer. His outfit, his books, his instruments, were provided by his father. The expenses of travelling and collecting ashore during his four years' voyage of circumnavigation were defrayed from the same source, though this work was done with the express object of enriching a public establishment.

On his return, he waived his claim to promotion in the navy, and devoted four additional years to the classification and publication of the results of the voyage. He also aided his father as an unpaid volunteer in the development of the scientific branches of the Kew establishment.

In 1847 Dr. Hooker was sent to India, to explore, in the interests of Kew, an unknown region of the Himalaya; and he was directed to proceed subsequently to Borneo, to report on its vegetable resources. His outfit, both for India and Borneo, which embraced a large collection of expensive instruments, cost the Government nothing. To cover all expenses incidental to his three years' travelling and collecting, including the cost of assistants and specimens, a sum of 1,200*l.* was received, while the real disbursements of Dr. Hooker during this time amounted to 2,200*l.* The difference was contributed by Sir William Hooker and his son, in the interest of the establishment to which they had consecrated their best energies.

On his return from India, Dr. Hooker again devoted himself to the work of aiding his father in the scientific development of Kew. He was also employed by the Admiralty, during the nine years from 1851 to 1860, in publishing the botanical discoveries of various naval and other voyages, from Captain Cook's downwards, to parts of the world visited by Dr. Hooker himself. For this service he received three years' pay as a medical officer in the Navy, together with a sum of 500*l.*, which was accompanied by "the expression of their Lordships' approbation of the zeal, perseverance, and scientific ability displayed in bringing to a successful completion this great botanical work." For three years he was occupied with the arrangement and distribution of his Indian collections, and with the publication of his journals. To cover the expenses incidental to these labours, an allowance of 400*l.* a year was granted by the Government.

Besides the voyages and travels above adverted to, Dr. Hooker has made journeys to various parts of Europe, to Western Asia, and to North Africa. The expenses of these journeys, though they were made with the express object of adding to the interest and completeness of Kew, have been borne by himself, and the results given to the establishment of which he is director.

We place these data before you, not with a view of founding on them either censure or complaint. The labours of Dr. Hooker, and the heavy drain upon his father's purse which his unexampled education as a botanist involved, constituted the discipline which made him the man he now is. But we think it highly desirable that you and England should know as much of his career as will enable you to decide whether its arbitrary interruption by your First Commissioner be creditable to the Government of this country.

In 1855, Sir William Hooker being then seventy years of age, Dr. Hooker was appointed his Assistant-Director, at a salary of 400*l*. a year, without a house; and from this time his share in the duties of the Garden were added to his more purely scientific ones. In 1858 his salary was increased to 500*l*. a year, with a house, and in 1865, on the death of his father, he succeeded to the Directorship without an assistant.

The liberality of his father and his own self-denying life in the public service have, we think, been sufficiently illustrated. We will therefore ask permission to place before you only one additional specimen of his conduct. As regards the Floras of Asia, Africa, and America, the Herbarium at Kew had been long unrivalled. Europe, however, was but scantily represented. Three years ago, a collection embracing the very flora needed for the completion of Kew was offered for sale in Paris. At his own private cost, Dr. Hooker purchased this collection for 400*l*., and presented it to the Kew Herbarium.

His income at Kew is 800*l*. a year, and here is one-half of it voluntarily devoted to the establishment which it had been the continual object of his father and himself to raise to the highest possible perfection. Had these things been known to the Parliament and public of England, the First Commissioner of Works would, we imagine, have hardly ventured to inflict upon the Director of Kew the unnecessary toil, worry, indignity, and irredeemable loss of time against which this memorial is a remonstrance.

Under the auspices of his father and himself, Kew Gardens have expanded from 15 to 300 acres. They have long held the foremost rank in Europe. In no particular does England stand more conspicuously superior to all other countries than in the possession of Kew. The establishment is not only without a rival, but there is no approach to rivalry as regards the extent, importance, or scientific results of its operations. Upwards of 130 volumes on all branches of botany, including a most important series of Colonial Floras, but excluding many weighty contributions to scientific societies and Journals, have been issued from Kew. To these are to be added guide-books and official papers. This vast literature has been produced and published through the efforts of the directors of Kew, for the most part at no expense whatever to the nation.

To these labours is to be added the correspondence of the directors with all parts of the world, a mere selection from which, now bound together at Kew, embraces some 40,000 letters addressed to the directors, and for the most part answered with their own hands.

Of the popularity of the Gardens, which has been attained without prejudice to their scientific use and reputation, it need only be stated that from 9,000 visitors in 1841, the numbers have risen to an average of nearly 600,000 a year. What they have done towards the elevation and refinement of the tastes and conduct of the working-classes may be inferred from the fact that last Whit Monday 37,795 visitors entered and quitted the Gardens without a single case of drunkenness, riot, theft, or mischief of any kind being reported.

Since Dr. Hooker's accession the Gardens have been to a great extent remodelled, and the establishment wholly reorganised. A great saving in outlay has thus been effected, without any sacrifice of efficiency. During the ten years from 1863 to 1872 inclusive, the annual number of living plants sent from Kew to various parts of the world has been doubled, amounting on an average to eight or nine thousand annually. Of seeds ripened at Kew, or obtained by the director from various parts of the world, the annual average distributed amounts to about seven thousand.

Of the practical value of these labours, the introduction of the Cinchona plant into India, Ceylon, and Jamaica, the commercial success of which is established, constitutes one of many illustrations. The introduction of Ipecacuanha is another. This will be corroborated by Her Majesty's Secretaries of State for India and the Colonies. We would add, that there is scarcely a horticultural establishment at home or abroad which would not be willing to acknowledge its indebtedness to Kew.

In India upwards of thirty gardeners trained at Kew are now employed in forestry, cotton, tea, and cinchona plantations, Government gardens, &c., and a far greater number are usefully employed in other parts of the world.

By the joint efforts of the directors, a series of complete Floras of India and the Colonies was set on foot at Kew, of which those of the West Indies, all the Australian Colonies, New Zealand, Tropical Africa, the Cape Colonies, and British India are completed or in progress. These are standard works of inestimable value in the countries whose plants they describe, as well as to scientific travellers and institutions in Europe.

We have hitherto confined ourselves to a statement of Dr. Hooker's services in relation to Kew, and have said nothing of his labours in geology, meteorology, and other sciences, nor of his researches while Botanist of the Geological Survey. During his single year of office he contributed to the Records of the Survey two memoirs, which are to be regarded as landmarks in the history of fossil botany. In presenting the Royal medal to Dr. Hooker in 1854, the president of the Royal Society spoke of these memoirs as "one of the most important contributions ever made to fossil botany." We may add a reference to his adventurous explorations of the northern frontier of India, in regions never visited by a European before or since.

It is not likely that a man of these antecedents, accustomed to the respect which naturally follows merit of the most exalted kind, would in any way expose himself, and more especially in matters relating to the welfare of Kew, to the just censure of his official superiors. Until the advent of the present First Commissioner, he had never been the object of a censure, and was never interfered with in the practical discharge of his duties by the Board of Works. His proposals and suggestions were rightly scrutinised, and his estimates regulated by the opinions of the Board, but the current duties were left entirely to his conduct and supervision; the extension and improvement of the establishment being always the origination and work of the Director.

With this sketch of the early training of Dr. Hooker for his present post before you, you will be able to compare with it the early training of Mr. Ayrton for the position which, by your favour, he occupies as Dr. Hooker's master. You will be able to judge how far the First Commissioner is justified in treating the Director of Kew with personal contumely, and in rudely upsetting the arrangements which he had made with reference to the invaluable collections for which he is responsible, not to Mr. Ayrton alone, but to his conscience and his country.

Neither you, Sir, nor the English public have forgotten the speech of the First Commissioner on presenting himself for re-election at the Tower Hamlets, when he went out of his way to insult "architects, sculptors, and gardeners." That speech was a warning to every cultivated man who held office under the Board of Works, and it was, as you know, duly laid to heart by the Director of Kew. His desire to avoid all cause of offence was thus expressed in a letter addressed to yourself on August 31, 1871:—"Having regard to the tenor of the sentiments Mr. Ayrton is reported to have expressed in public on accepting office, I felt it incumbent on me to be especially circumspect in my conduct and demeanour under his rule."

Circumspection, under the circumstances, was of small avail, and one of Mr. Ayrton's first acts, after taking office, was to send a reprimand to Dr. Hooker. It was a new experience to the Director of Kew. During his thirty years of public service such a thing had never once occurred; indeed, the very reverse of it had always occurred, the respect due to intellectual eminence and moral worth having been always cheerfully accorded to Dr. Hooker by his official superiors. This first reprimand of his life was, moreover, not due to any fault of his, but arose entirely from the First Commissioner's own misconception.

The responsibility of the warming and ventilation of the plant-houses had, by special order, devolved upon the Director. After a searching inquiry, Dr. Hooker had been entrusted by a previous First Commissioner with the task of remodelling the heating apparatus throughout the establishment; and this led to the construction at Kew, in accordance with the Director's plans and estimates, of the most complete range of hot-houses for scientific purposes in existence. In 1871, however, he accidentally discovered that he had been superseded in the duty, without notice given or reason assigned. He wrote a respectful letter of inquiry to the First Commissioner, and received the short—we are persuaded you will agree with us in adding, insolent—intimation that he *had* been superseded, and would have "to govern himself accordingly."

He would, in our opinion, have been equally unfaithful to the science of which he is a leader, and the public which he had so long served, if he had bowed in silence to this rebuke. He wrote a second letter of remonstrance to the First Commissioner, in which he expressed himself as follows: "The matter, therefore, stands thus: Several months ago I was, unknown to myself, deposed from the discharge of a function of great importance. I was left to hear this accidentally and, I have now to add, through one of my own subordinates.

"I do not for a moment question the First Commissioner's power to exercise arbitrary authority over the Director of Kew, but I do submit that there has been hitherto no plea whatever for such action as regards myself, and that the repetition of such acts, and the leaving me to be informed of them, *on each occasion*, by my subordinate, constitute a grievous injury to my official position, and tend to the subversion of all discipline in this department."

At this point, Sir, Dr. Hooker turned in the fullest confidence to you. He had undoubting trust in your will and power to protect both Kew and him from the arbitrary and, we would add, ignorant acts of the First Commissioner. He respectfully claimed the privilege of bringing the matter under the cognizance of the Right Honourable the First Lord of the Treasury.

You doubtless remember the letter addressed to you by Dr. Hooker on August 19, 1871. You could not fail to remark the reluctance with which he appealed to you, and his previous anxiety to take all possible measures to avert the necessity of such an appeal. "I cannot express to you, Sir," he writes, "the anxiety that this step costs me, nor how earnestly I have endeavoured, by suppressing all personal feelings, to conduct my duties here under Mr. Ayrton to his and to my own satisfaction.

"After upwards of thirty-two years spent in the public service at home and abroad without a suspicion of mistrust on the part of my many previous superiors, I have had since Mr. Ayrton's accession to submit to various arbitrary measures, which, though compromising my position and authority, have been concealed from myself and become known to my subordinates, through whom alone I have first been made cognizant of them"

From you, Sir, the Director of Kew received no direct reply to this communication; but by the First Commissioner he was requested to "furnish the dates and particulars of the conspicuous proofs of disregard to his office, and the particular occasions and facts with dates, of his being left to be informed through his subordinates of acts of arbitrary authority of the First Commissioner, and the dates and particulars of those acts."

To this challenge the Director of Kew replied by adducing five distinct acts of arbitrary interference, with their "dates," "proofs." and "particulars."[1] Among them was included what we have a right to call clandestine tampering with the subordinates at Kew. For example, the Curator of the Gardens was tempted by Mr. Ayrton, personally, to leave Kew by the offer of a higher position, involving authority over works at Kew; and he was requested by Mr. Ayrton to keep the fact from the knowledge of Dr. Hooker. To the loyalty of this man to a master whom he trusted and loved, the Director of Kew owes the discovery of proceedings which under any previous First Commissioner would have been impossible.

Your attention, Sir, was drawn to this reply in a letter addressed to you by the Director of Kew on the 31st of August, 1871. It is in every respect so excellent, and so sure to be appreciated by all who know the real meaning of scientific work, and the baneful effect upon such work of this harassing conflict with your First Commissioner, that we do not hesitate to reproduce it here *in extenso*.

"Royal Kew Gardens, August 31, 1871

"Sir,—I beg most respectfully to submit copies of my further correspondence with the Right Honourable the First Commissioner of Her Majesty's Works, &c.

"The acts detailed in the accompanying letter are, I believe, correctly described. I trust that I do not exaggerate in characterising them as grievously injurious to my official position, and tending to the subversion of discipline in this establishment; and I have evidently no protection from a repetition of them, except through the intervention of a higher authority.

"Of these acts, those referred to under 1, 2, 3, formed the subject of a prolonged correspondence between the First Commissioner and myself; that under 4, I brought to the notice of Mr. Stansfeld, and the result was the abandonment of the proposal; that under 5 will, I venture to hope, be revoked by your authority. I refrain from commenting on these acts of the First Commissioner in reference to their seriously interfering with the execution of my peculiar and multifarious duties here.

"These include the labours of a Scientific Botanist, a Horticulturist, and the administration of Public Gardens, Museums, and Pleasure Grounds, frequented annually by upwards of 600,000 persons.

"Besides the living collections, I have the direction of the largest and by far the most frequented Herbarium (by botanists and amateurs) in existence, and a very extensive Library.

"I conduct, without a secretary, a responsible and onerous correspondence with Foreign and Colonial Gardens, as also with the Admiralty and Indian and Colonial Offices, on all subjects connected with Horticulture, Forestry, Botany, and the appointment of officers to duties in connection with these matters, and the introduction of useful plants everywhere.

"I have, further, the editorship or control of various botanical works now being published, by order of Government, at Kew; and I have to devote every moment that I can spare from my duties to maintaining, by researches and publications of my own, a position as a Scientific Botanist.

"Until the accession of Mr. Ayrton, I have been enabled to fulfil these duties with satisfaction to myself, having been treated with uniform confidence, consideration, and courtesy by my superiors. I was invariably consulted on all prospective changes affecting my own and my subordinates' positions and duties. On the revision of my estimates at the Board, before their transmission to the Treasury, I was referred to; and amongst my other current duties was the control of the construction and repairs of the hot-houses and heating apparatus throughout this establishment.

"Subsequently to Mr. Ayrton's accession, my position has been materially changed in all these respects. He had hardly entered on his duties when he hastily administered to me a wholly unmerited reprimand (the first I ever received), and his last act (known to me) has been to take from me the above-mentioned control, without pretext, warning, or subsequent intimation.

"I venture to hope that this may be restored to me, if the reasons I have adduced in the enclosed letter to the First Commissioner are satisfactory to you. To these I would add, that in all similar establishments with which I am acquainted, in England or abroad, the opinion of the cultivator is entitled to the first consideration in all matters relating to plant-houses and heating apparatus; that to trust him with the care and treatment of invaluable collections, and make him amenable to the opinions of another in respect of the apparatus he requires, is as obviously wrong in principle as to refuse a surgeon his choice of instruments and hospital appliances. Nor would it be candid in me to withhold from you my conviction, that I have by this arbitrary act of the First Commissioner been lowered in the eyes of those who know no more of the circumstances than that I am deposed from the full control of buildings and apparatus which I was entrusted to erect and have still to use.

"Let me assure you, Sir, that I am unconscious of any feelings of personal animosity against Mr. Ayrton.

"Having regard to the tenour of the sentiments he is reported to have expressed in public, on accepting office, in respect of professional duties such as mine, I felt it incumbent on me to be especially circumspect in my conduct and demeanour under his rule. And in evidence of this let me add, that when still smarting under his unprovoked reprimand, I, at his special request, devoted many nights to examining and reporting upon various books and pamphlets on the public parks of England, France, and America, for his guidance—a labour not very congenial and wholly beyond my province as Director of Kew, and which I further undertook in the hope that it might lead the First Commissioner to judge more generously of the acquirements and duties of some of the officers of the department he controls.

"I am, Sir,
"Your most obedient Servant,'
"JOS. D. HOOKER, *Director*

"To the Right Honourable
The First Lord of the Treasury"

To this letter Dr. Hooker was honoured by a reply from yourself, couched in kind and considerate terms. You had communicated with Mr. Ayrton, and had received his explanations, which you forwarded to the Director of Kew, "in the hope that they would convey to his mind the assurance that there has been no intention on Mr. Ayrton's part to disregard his feelings or withhold the consideration due both to his person and his office."

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[1] Dr. Hooker's letter containing these charges was never answered, or even acknowledged, by Mr. Ayrton.

Stress of public duty is quite sufficient to account for the fact of your overlooking the serious omissions and inaccuracies of the First Commissioner's "explanations." These, however, were immediately pointed out to you by Dr. Hooker. His object in addressing you was not simply to complain of personal discourtesy on the part of the First Commissioner, but of five official acts subversive of discipline in the Kew establishment, and fraught with mischief to the public service. Had these acts merely affected him personally, he would have been perfectly willing to accept the assurance of Mr. Ayrton's consideration, though he failed to discover any trace of it either in his explanations or in the treatment which official papers sent from Kew continued to receive from the office of Works. He regarded it, however, as his duty, as an officer in the public service, to the Government and the scientific public, to spare no effort to procure a reversion of the policy introduced into the management of each and all of the departments of Kew (the Gardens, Museum, and Herbarium) by the present First Commissioner.

The specific acts enumerated by Dr. Hooker in answer to the request of the First Commissioner are thus summarised in a letter to yourself:—

"1. A transaction with my subordinate of a nature so new to my long experience of official life, and so repugnant to my principles, that I refrain from characterising it.

"2. Removing the Curator from his duties under me, without any communication with me.

"3. Empowering the Curator to act independently of me in regard to the times he should consider himself under my orders, and instructing me to make my arrangements in deference to his, and in concert with him.

"4. Submitting to the Treasury plans and estimates for extensive alterations in the Museum at Kew, without even informing me of his intentions; which works would have most seriously embarrassed me, as Director of the Museums, and would have involved a large expenditure, for which I believe no estimate was submitted, and which would have been in every respect detrimental.

"5. Superseding me, without previous or subsequent communication, in duties for the execution of which I held the Board's authority, and which I am of opinion should unquestionably be performed by the Director, *i.e.*, the control of the heating apparatus of the hot-houses, &c."

In Mr. Ayrton's letter of "explanations," which you considered so satisfactory, the first three of these charges are skilfully ignored, and the other explanations contain statements which are demonstrably at variance with fact.

In the very considerate note above referred to, which was addressed by you from Balmoral to Dr. Hooker, you say: "There must be some mistake about Mr. Ayrton's failing to see you at Kew, as he assures me that he paid the visit there for the very purpose of personal and friendly communication."

We respectfully ask you to consider how this "purpose" was carried out. Dr. Hooker was at home when the First Commissioner paid this visit to Kew. He omitted to inquire for the Director at his house, or at the Gardens, or of his subordinate, to whom Mr. Ayrton's visit was really paid. He held a conversation with this subordinate seriously compromising the Director's position and authority, which conversation he subsequently desired should not be communicated to the Director. The result of this conversation, moreover, was a communication to the Treasury affecting Kew, which was also kept from the knowledge of the Director. You will learn from these facts what the First Commissioner understands by "personal and friendly communication."

From its effects upon himself, Dr. Hooker could infer how disturbing the continual intrusion of this subject upon your attention must be. He was anxious to reduce this disturbance to a minimum, and therefore ventured to suggest that he should be put in communication with one of your private secretaries, to whom he might explain his position. To this request you, in the kindest manner, assented, and placed Mr. West in communication with the Director of Kew.

Sorely against his inclination, but driven to it by the necessities of the case, Dr. Hooker, at an interview with Mr. West on the 30th of October, distinctly pointed out the grave errors and omissions contained in the "Explanations" given by the First Commissioner to the First Lord of the Treasury.

The end of the year approached without any answer being made to these communications and representations, and towards the close of December Dr. Hooker wrote again to Mr. West, who thereupon replied that a plan was under the consideration of the Government which would materially alter his position with reference to the First Commissioner of Works. He was subsequently informed semi-officially that the scheme was maturing, and the hope was expressed that he would take no step likely to embarrass the Government. This was far from his wish or intention. But after waiting till the 21st of February, the Director was semi-officially informed that the Government plan for his relief, and for the protection of Kew, had been abandoned.

In the hope of a satisfactory settlement, the matter was subsequently placed by you in the hands of the Marquis of Ripon, and on the 13th of March, 1872, before a Committee of the Cabinet, consisting of the Marquis, Lord Halifax, and Mr. Cardwell, Dr. Hooker, by the desire of the Committee, handed in a memorandum containing a statement of the points wherein his relations to the Government required definition and correction.

The upshot of these friendly efforts was this: On the 15th of April, 1872, Lord Ripon was asked to convey the following verbal message from yourself to Dr. Hooker, which, the noble Marquis added, was to be regarded by the Director of Kew as a final answer to his appeal: "Mr. Ayrton has been told that Dr. Hooker should in all respects be treated as the head of the local establishment at Kew; of course in subordination to the First Commissioner of Works."

At this time the controversy had, unhappily, reached a pitch far too serious to be stilled by such a message. In a letter to your private secretary, written immediately subsequent to the interview on the 30th of October, Dr. Hooker put his case thus: "I am at a loss what to say as to my future position under a Minister whom I accuse of evasion, misrepresentation, and misstatements in his communications to the First Minister of the Crown, whose conduct to myself I regard as ungracious and offensive, and whose acts I consider to be injurious to the public service, and tending to the subversion of discipline. Granting," he continues, "that the functions of a Director are restored to me, how am I to act when ordered to undertake works that involve wasteful expenditure, or are otherwise detrimental? I should be thankful for Mr. Gladstone's instructions on this head."

With great deference, we submit that the verbal intimation conveyed from you to Lord Ripon, and from Lord Ripon, through Mr. Helps, to Dr. Hooker, by no means met the issues here raised by the Director of Kew. He had suffered from the secret tampering of the First Commissioner with his subordinates; he had successfully resisted extravagant and foolish proposals made by the same Minister. His duties and responsibilities as regards the warming of the plant-houses had, to the imminent jeopardy of plants of the rarest value, been transferred, without notice or justification, to the Director of Works. Another class of duties had, in the same secret manner, been transferred to the Secretary of the Board of Works. Surely, Sir, your message through Lord Ripon, to all intents and purposes, empowered the First Commissioner to continue his course of studied indignity? Wrong upon wrong had been committed, which your answer left unredressed. No wonder that, notwithstanding his esteem and regard for you personally, and his respect for all authority rightly exercised, the Director of Kew should be driven to address to you, on the 22nd of April, a letter containing the following remonstrance:—

"The fact is, that the Directorship of Kew, which was formerly subordinated to the First Commissioner alone, has been by Mr. Ayrton officially subordinated to the Secretary of the Board and the Director of Works in London, and this surreptitiously, without fault found or notice of any kind given, the Director being left to discover his altered position as best he could: and the Director has further been subjected to a series of arbitrary and offensive measures on the First Commissioner's part, against which he could not defend himself. These measures being destructive of discipline, and injurious to this establishment, the Director felt it to be his duty to bring both their nature and consequences officially under your notice, and to seek from you that justice which (as he had been assured by the officers of the Treasury) could be obtained only through an official appeal to the Prime Minister.

"After eight months' interval, during which further arbitrary measures have been resorted to by the First Commissioner (and four of which were passed under the assurance that a measure for effectual relief was under consideration), the position of the

Director of Kew is not better, but worse than when it was first brought under your notice; for within that period his views with regard to the scientific and other appointments in the establishment have been absolutely set at naught.

"These circumstances are well known to the Director's subordinates. They know that he has been virtually deprived of authority and responsibility, and that his official appeals have been unanswered, and his complaints ignored. The basis of all order and discipline in the establishment is thus sapped, and the position of the Director rendered so anomalous, that his desire and determination to uphold the interests of Science at Kew, strengthened as they are by the moral and material support guaranteed to him, hardly suffice to render that position endurable.

"Your own practical wisdom will enable you to judge whether such a state of things is to be remedied by the curt and vague announcement (and such you must allow me to call it) which you have been good enough to make me through Lord Ripon.

"I have the honour to be, &c.

"(Signed) J. D. HOOKER"

Your verbal announcement through the Marquis of Ripon was subsequently defined by Mr. West as a private and friendly communication; and your secretary proposed that, as an official answer would be sent to Dr. Hooker's official application, the letter from which the foregoing extract is made should be considered as *non avenue*. Dr. Hooker, however, had shown his letter to friends whose counsel he had sought in this matter, and he therefore pleaded that you ought to see that which had been seen by others. With regard to the character of the verbal communication, Dr. Hooker had been given distinctly to understand that it was *official* and *final*. He, however, cheerfully accepted the assurance of your secretary, and awaited the official reply. It came, and we hereby respectfully submit it to your calm interpretation.

"Treasury Chambers, April 25, 1872

"SIR,—I am directed by the Lords Commissioners of Her Majesty's Treasury to acquaint you that their Lordships have been in communication with the First Commissioner of Works as to the matters contained in the letter which you have addressed to the First Lord of the Treasury.

"Their Lordships find that there is no difference of opinion upon the question of your position, which may be briefly defined as that of head of the local establishment at Kew, of course in subordination to the First Commissioner, and they anticipate no difficulty in the future regulation of the relations of that important establishment to the office of the Board of Works, in which the duties and powers of management are vested by statute.

"The present form of estimate for Kew Gardens laid by their Lordships before the House of Commons cannot now be altered, but it will be acted upon, and will in future be framed in accordance with this letter.

"I am, Sir,
"Your obedient Servant,
"(Signed) CHARLES W. STRONGE, *Principal Clerk*

"Director of Royal Botanic Gardens, Kew."

The concluding paragraph of this document, which is evidently the really important one, has been submitted to various persons accustomed to the language of official life, and we do not believe that a single one of them is sure of its meaning. Dr. Hooker, while willing to put the best construction upon it, thought it necessary to make a final inquiry, which was preceded by these remarks:

"I am most desirous of giving their Lordships no further trouble, and am, of course, prepared either cheerfully to submit to their decision, whenever it is clearly given, or to resign the office which I hold; but I am unable to find in your letter any judgment whatever upon the points contained in the accompanying Memorandum, which have all been submitted to the First Lord of the Treasury in my letters of August 19 and 31, or to the Committee of the Cabinet which I had the honour of attending on March 13 at Lord Ripon's residence.

"I trust that their Lordships will observe, that in seeking their decision on these several questions, I am raising no superfluous difficulties, but that it is impossible for me to understand my position until it receives their Lordships' authoritative definition in respect of the above matters.

"I am, Sir,
"Your obedient Servant,
"(Signed) JOS. D. HOOKER, *Director*

"Charles W. Stronge, Esq."

"MEMORANDUM

"1. Up to the date of the appointment of a Director of Works (under the Board of Works) in 1870, I was entrusted, by a special warrant of the Board, with the duty of preparing the estimates for the construction and repairs of the plant-houses, museums, and warming apparatus in this establishment. This warrant has been cancelled, without fault found, inquiry made, or intimation given, and the duty transferred to the Director of Works.

"Will you be good enough to inform me if it is their Lordships' decision that the powers conferred upon me by that warrant be restored to me?

"Previous to the accession of the present First Commissioner to office, I was consulted whenever changes were made in the estimates which it is my duty to submit to the Board, prior to their transmission to the Treasury.

"Am I to understand that hereafter the estimates will not be altered by the Board without giving me an opportunity of stating my views?

"3. I was entrusted with the custody and distribution to scientific bodies, &c. of the copies of the first volume of the 'Flora of Tropical Africa,' a work the publication of which I am officially instructed to superintend at Kew. On the publication of the second volume, the undistributed copies of the first were withdrawn, without inquiry, from my custody, and sent, together with those of the second volume, to the stationery office for sale.

"Would you be so good as to state whether I am in future to be entrusted with the custody and distribution of scientific works of which I (the unpaid editor) am entrusted with the publication by the Board?

"4. Previous to the accession of the present First Commissioner to office, I was consulted in all cases of prospective changes in the position and duties of my subordinates, and in all cases of proposed works that might affect my duties and responsibilities

"I shall be glad to know whether I am in future to be consulted in regard to such matters.

"5. The Department of Works having been brought under the rules of the Civil Service Commissioners, all candidates for employment at Kew are liable to be chosen by open competition, except in cases where the qualifications required 'are wholly or in part professional, or not ordinarily to be acquired in the Civil Service,' as set forth in Clause VII. of the Commissioners' Rules. The present First Commissioner of Works refuses to allow me to take advantage of Clause VII. in cases both of purely botanical and horticultural appointments.

"Am I hereafter to be allowed to avail myself of this clause, when it is of importance to the public service that I should do so?

"6. The Director of Works having been given power to interfere in matters for which I am still in part responsible, I am anxious to know—

"Whether I am to consider myself subordinate to the Director of Works in such matters, and to submit to his control in respect of them. "JOS. D. HOOKER

"Kew, May 1, 1872"

To this letter no answer has been received.

It but rarely falls in either with our duties or our desires to meddle in public questions; and not until we found Dr. Hooker maimed as regards his scientific usefelness—not until we saw the noble establishment of which he has hitherto been the living head in peril of losing services which it would be absolutely impossible to replace, not, indeed, until we had observed a hesitation upon your part which we believe could only arise from lack of information—did the thought of interference in this controversy occur to us. Knowing how difficult it must be for one engrossed in the duties of your high position to learn the real merits of a conflict like that originated by the First Commissioner of Works, we venture to hope that you will not look with disfavour on an attempt to place a clear and succinct statement of the case before you.

That statement invites you respectfully to decide whether Kew Gardens are or are not to lose the supervision of a man of whose scientific labours any nation might be proud; in whom natural capacity for the post he occupies has been developed by a culture unexampled in variety and extent; a man honoured for his integrity, beloved for his courtesy and kindliness of heart; and who has spent in the public service not only a stainless but an illustrious life. The resignation of Dr. Hooker under the circumstances here set forth would, we declare, be a calamity to English science and a scandal to the English Government. With the power to avert this in your hands, we appeal to your justice to do so. The difficulty of removing the directorship of Kew from the Department of Works cannot surely be insuperable; or if it be, it must be possible to give such a position to the Director, and such definition to his duties, as shall in future shield him from the exercise of authority which has been so wantonly abused.

CHARLES LYELL
CHARLES DARWIN
GEORGE BENTHAM, Pres. Linn. Society
HENRY HOLLAND, Pres. Royal Institution
GEORGE BURROWS, Pres. Roy. Coll. of Physicians
GEORGE BUSK, Pres. Roy. Coll. of Surgeons
H. C. RAWLINSON, Pres. Roy. Geogr. Society
JAMES PAGET
WILLIAM SPOTTISWOODE
T. H. HUXLEY
JOHN TYNDALL

PROFESSOR AGASSIZ'S SOUTH AMERICAN EXPEDITION

I.

PROF. AGASSIZ'S Second Report to Prof. B. Peirce, Superintendent of the United States Coast Survey, dated U.S. steamer *Hassler*, Concepcion Bay, June 1, is given in the *New York Tribune* of June 26. The Report is of so great value and interest, that we reprint it entire:—

Since I sent my first report concerning erratics in the Southern Temperate Zone, I have been much engrossed with this subject, and have turned my attention chiefly that way, leaving to Pourtales the superintendence of the dredging, and to Steindachner and Blake the care of the zoological specimens.

On the eastern coast of Patagonia I had but little opportunity of adding to the information I had already obtained at Monte Video. It was not till we put into San Mathias Bay for some repairs that I could gather a few new facts. This bay is particularly interesting, because one can there compare the position of the tertiary beds in the cliffs bordering the Atlantic with that of similar beds in the cliffs along the northern shore of the bay. The southern exposure of the latter runs for nearly a hundred miles at about right angles with the sea coast. In both cases the outcrops of the beds are so nearly parallel with the surface of the sea, that whatever may have been their changes of level with reference to the ocean, they still retain the horizontal position in which they were deposited. It is of the utmost importance to remember this point when considering the distribution of the erratics over this part of the country with reference to the agency that may have transported them to their present resting-place. Among these tertiary deposits are well-marked banks of colossal oysters of considerable extent, one of which coincides with the level of low water, while another stands at least twenty-five feet higher. The difference of level between these two great beds of oysters is so considerable as to suggest a subsistence of the sea bottom during the deposition of the tertiary beds. Higher up there are outer layers full of smaller fossils—some about ten, others about twenty feet above the second oyster-bed. The oyster-beds are perfectly parallel with one another, and separated by thin layers of clay and sand. And so, also, are the upper tertiary beds containing the smaller fossils. Among these, one bank consists almost entirely of large numbers of a species of *Scutella* with a single perforation in the posterior interambulacral area. This bank is particularly well marked. A bank of hard sand higher up is also conspicuous, and so is another of hard clay standing about 100 feet above the sea-line.

As we shall see hereafter, and as Darwin has already stated, these tertiary beds extend all over eastern Patagonia, including the Straits of Magellan as far as Sandy Point. In consequence of disintegration the harder beds form as many retreating shelves, like stairs, upon the slope of the shore bank. Wherever surface denudation has taken place these shelves give rise to terraces, stretching horizontally at various heights all over the plains of Patagonia. The scenery at Cliff End reminded me somewhat of Gay Head and its tertiary formation, except that the upper part of the Cliff consisted chiefly of sandy clays, alternating with which are two distinct horizontal beds of considerable thickness, formed entirely of pebbles, rather small, and uniform in size. These pebbles vary from the dimensions of a pea or a hazel nut to that of the fist, or more; but there are no boulders or large fragments of rock among them. It is noteworthy that, while these pebbles alternate in regular stratification with the sandy clays in the upper part of the cliff, they also occur upon the shelves below. In the latter case, however, they form only superficial deposits, and do not penetrate with the beds on which they rest into the interior of the strata. It has occurred to me that similar superficial accumulations of pebbles upon the shelves bordering the bed of the Santa Cruz River may have been mistaken by Darwin for indications of successive upheavals. It is certain that there are no beaches here, marking successive steps of the upheaval of the country. What Darwin has considered as evidence of a gradual rise of the shore are the denuded surfaces of the horizontal tertiary deposits which everywhere form parallel terraces. As for myself, I see here no evidence of upheaval except the level of the fossil beds of oysters and other fossils in the tertiary beds above the water, and the presence of fresh shells of living species upon and above the shore banks. These, however, only indicate that an upheaval has taken place since the deposition of the tertiaries, and while the shells now living already existed, without pointing to the rise by successive steps. Still less does it appear to me that the country has been submerged during the transposition of the erratics. Toward the west end of San Mathias Bay, at Port San Antonio, where extensive denudations have taken place in the very formations here described, these same pebbles occur again. But at Port San Antonio, instead of being well defined, continuous horizontal beds above the sea-level, they are shore pebbles, covering in a deep layer the whole extent of the beach, the inequality of which they follow. Their position here shows, beyond the possibility of doubt, that the whole set of beds above which they rest in regular stratification at Cliff End has been completely broken down and recently removed by the action of the sea, and the pebbles themselves thus brought to the sea level. Of course it follows that these pebbles have not been ground upon the modern beach, but upon an older foundation, corresponding at the time to the level at which the pebble beds now stand at Cliff End.

So far the facts. I am inclined to add, as an inference from subsequent observations made farther south, the relation of which to the facts above stated seem to me clear, that these pebbles have passed through the mill of a glacier's bottom before they were worked up by the floods into their present position in the beds of Cliff End and upon the beach of San Antonio; and I do not see why the floods which formed these denudations could not as well have been the result of the melting of ice at the close of the glacial period, as the result of a change of level between land and sea. As soon as geologists have learned to appreciate the extent to which our globe has been covered and fashioned by ice, they may be less inclined to advocate changes of level between land and sea, wherever they meet with the evidence of the action of the water, especially where no marine remains of any kind mark the presence of the sea. As I have already said, the small and remarkably uniform size of the pebbles in Port San Antonio is particularly noticeable, and also the fact that none but hard rocks, indeed, only the very hardest kind of rocks, are represented among them.

(*To be continued.*)

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THURSDAY, JULY 18, 1872

MEDICAL EDUCATION

A THOUGHTFUL address on "Medical Education in America," read by Dr. Bigelow to the Massachusetts Medical Society, has just been published in the form of a pamphlet. It discusses the important subject of the kind and degree of instruction in collateral sciences that should be given to the student of medicine during the short period of four years now at his disposal, and in the course of which he is supposed to have acquired the knowledge that will enable him to practise with advantage, or at least let us say with safety, to his patients, and with credit to himself. The question is of a very complex nature, and its difficulties can, perhaps, only be properly appreciated by those who have themselves been teachers, and who are not, therefore, likely to be led away by Utopian ideas of the amount of information that can be acquired by a man of ordinary abilities in this space of time even under favourable circumstances. It is very easy to say Educate to the highest point possible; let the student know something, at least, of Chemistry, of Botany, of Comparative Anatomy, of the origin, composition, and mode of manufacture of the drugs he uses; but the fact is overlooked that almost all he learns of these subjects is quickly cast aside when he begins to practise, because he finds that it is of no earthly use to him, and he regrets when too late the time he has spent in acquiring them, because it has led him to neglect the far more important matters of Pathological Anatomy, and the actual practice of Medicine and Surgery. "The Medical Student," Dr. Bigelow observes, "does not need to pick herbs from the field, or treat horses and dogs, or consider his parallelogram of forces before putting in a dislocated shoulder; but he does need to know how to recognise and exactly how to reduce a dislocated shoulder, how to recognise and treat human disease, and what are the medical properties of the drug which the farmer has grown, or the merchant imported for the apothecary. This is but a fair division of labour. He has enough to occupy him profitably and exclusively in his own immediate field of study, without wandering over the whole domain of knowledge—at least, at the mistaken behest of those who have a confused notion of a liberal education and large culture." "There is a fallacy in the idea of culture. Talent and power of application may, indeed, incidentally lead a man to eminence in several directions. But a cultivated, a literary, or even a scientific man is not necessarily the best physician." At the same time Dr. Bigelow concedes that there should be a certain latitude in the study of medical science on the ground that "no student or artisan is the worse for an outlook upon kindred arts and sciences which will help him to establish the true relations of his own, which will supply him with additional facilities and light for its pursuit, and with that training of his intellectual powers afforded by a systematic variation in their exercise." It must be remembered that all the sciences collateral to medicine have undergone extraordinary development during the past few years; and that to acquire a very moderate knowledge of chemistry, for example—such knowledge as would enable the student to analyse a single animal fluid, or even a fragment of a calculus—would be the undivided work of a year, and when accomplished he would scarcely be one step in advance of the man who had learnt a few rules of general application to the diagnosis of disease taught by an accomplished chemist.

Whilst agreeing with the general views expressed by Dr. Bigelow upon the education of the medical student as he now presents himself at the Hospital Schools at the age of eighteen or nineteen, it yet appears to us that the quality of the raw material, if we may call him so, might be immensely improved by the general adoption of a well-directed scheme of preliminary education. "One of the enormous follies of the enormously foolish education of England," said Sydney Smith, "is that all young men, dukes, fox-hunters, and merchants, are educated as if they were to keep a school or serve a curacy." Just so; and it is precisely in this respect that the education of the medical student of this country requires revision and improvement. The medical profession is not essentially a literary one. What is really required is a seeing eye and an understanding heart; the faculty of correct observation on the one hand, and on the other the ability to single out what is important amidst a multitude of unimportant particulars—in a word, judgment; and as this is capable of being immensely improved by exercise, it should surely be the point to which the education of the student should be directed. But, as a matter of fact, no line of education can be better adapted for the purpose in view than that of the medical student of the present day. From the beginning to the end it is or might be made a "questioning of nature." The grand defect of the system is that insufficient time is at the disposal of the student to master the details. He learns a little of many things; nothing well, unless it be his anatomy; but the advantage a knowledge of all would give him may be estimated in some remote degree by the value that teachers and students alike set upon this single acquisition. Seven years are not thought too long to make a master workman in any of the humblest trades; and yet the student is expected to acquire a fair knowledge of all the branches of a very wide, difficult, and profound intellectual pursuit in four short years, or if we read Dr. Bigelow aright, in three years in America. It is here we think, then, that some alteration is requisite. A boy who is going to enter the medical profession should be early set apart for that ministry. We are bold to say that a boy of fifteen knows or ought to know enough of Latin, Greek, and Arithmetic for any subsequent use he is likely to make of them. At this age he should be called upon to select what his future career shall be, and his education should be directed accordingly. From the inquiries of the Committee of Convocation of the University of London, *à propos* of the proposal for rendering the examination in Greek optional at Matriculation, it appears that there are several large schools of good repute in this country, as those of Cheltenham, Clifton, Haileybury, Marlborough, and Wellington, in which a "modern side" has been established, where attention is chiefly directed to the cultivation of mathematics and modern languages, Latin and Greek being considered as subsidiary branches of knowledge, or even completely omitted, as in the case of Greek at Cheltenham College.

We venture to suggest that in these, and, indeed, in every other large school in England, a third, or Natural Science department should be founded, in which Practical Chemistry, Field Botany, and Natural Philosophy, with the French and German languages, should form the subjects of study. We are confident that in the hands of competent teachers, a lad might obtain between the ages of fifteen and eighteen or nineteen, a very large amount of useful knowledge on these subjects, without any undue strain upon his intellectual powers; while we are convinced that such a scheme would prove successful in a pecuniary point of view; and that there would be ample funds, derived from the scholars in attendance, to pay the additional teachers that would be required. The instruction given need only be rudimentary; but it should be most precise and thoroughly acquired. Any chemist could select six elements, any botanist six natural orders, any zoologist six classes of animals, which, if thoroughly known, would constitute an invaluable training to the future physician. He would then enter the medical school with a well-cultivated mind accustomed to close observation, and prepared to profit to the utmost by the system of education now generally adopted.

ORNITHOLOGY OF NEW ZEALAND

Catalogue of the Birds of New Zealand, with Diagnosis of the Species. By Frederick Wollaston Hutton, F.G.S., Assistant Geologist. Published by Command. 8vo. (New Zealand, 1871.)

A History of the Birds of New Zealand. By Walter Lawry Buller, D.Sc., F.L.S., C.M.Z.S., &c. Part I. 8vo, coloured plates. (London, 1872.)

BIRDS, as most people know, or ought to know, form the most important part of the vertebrate Fauna of New Zealand, and their importance is maintained not only when they are compared with their compatriots of other classes; but, when regarded in reference to members of their own class in the world at large, the birds of New Zealand offer so many singular forms that as a whole they deserve every consideration. Some of the most remarkable of these have already been mentioned by a distinguished writer in this periodical,[*] but perhaps hardly sufficient prominence was then given to the fact in the ornithology of New Zealand which seems of all others to demand attention; for, recent birds being divided into two great and trenchantly marked groups, of very unequal extent, the smaller of these groups (the *Ratitæ*) is found to contain six most natural sections, comprising, to take the most exaggerated estimate, less than two score of species, while the larger group (the *Carinatæ*), though perhaps not containing more natural sections, comprehends some ten thousand species. Now, two out of the six sections of this small group are absolutely restricted to New Zealand; and these two sections contain considerably more than half of the species known to belong to it. Thus, setting aside the Carinate birds of our distant dependency (and some of them are sufficiently wonderful), its recent Ratite forms—some twenty species, let us say—alone may be regarded as the proportional equivalent of one-tenth of the birds of the globe, or numerically, we may say, of an avifauna of about one thousand species.

The birds of New Zealand, therefore, merit especial attention, and we are happy to say they receive it at the hands of the authors whose works are above cited. Taking the field in or about the year 1865, Mr. Buller, till then unknown to fame beyond the limits of his native colony, brought out an "Essay on the Ornithology of New Zealand," which at once attracted notice in this old world of ours. Some of his views were challenged by Dr. Finsch, then of Leyden, who had paid attention to this extraordinary avifauna, and a controversy ensued. This, to the credit of the controversialists, was carried on in a spirit very different from that in which many another war in natural-history circles has been waged, and the happy result is that on most points the combatants have arrived at the same conclusion, thereby giving assurance to the general public of its being the right one. The Essay we have mentioned may be regarded as the preliminary canter which a race-horse takes before he puts forth his full strength; and Mr. Buller's book, or that part of it which is as yet published, shows what he can do now that the colonial authorities have allowed him to come to England for the express purpose of completing his design.

Captain Hutton is known as an observer who, during several long voyages, had proved that some rational occupation could be found at sea even by a landsman; for, instead of devoting his energies to the ordinary time-killing amusements of shipboard, he watched the flight of the various oceanic birds which presented themselves, and speculated on the mode in which it was performed and the forces it brought into operation—to some purpose as the Duke of Argyll and Dr. Pettigrew have testified. The pamphlet whose title we give is in some respects a not less significant, if a less ambitious, work than Mr. Buller's; and though to the last must belong the crown of glory, we by no means wish to overlook the useful part which Captain Hutton's publication will play. If here we do not notice it further, it is because its value will be most appreciated in the colony itself, while Mr. Buller's beautiful book appeals to a larger public.

Of the baker's dozen of species included in this first part of the "History of the Birds of New Zealand," we propose to notice only those belonging to three genera, two of them quite peculiar to the country, while the third is, or was, found in the neighbouring islands of the same zoo-geographical province. The remaining eight species belong to types of far wider distribution; hawks, owls, and kingfishers present much the same features all the world over, and the New Zealand parrakeets do not much differ from their congeners which are found in other portions of the Australian region.

The Kakapo, or Owl-parrot (*Strigops habroptilus*), is without doubt one of the most remarkable of New Zealand birds. It has already been figured in these columns;[*] but perhaps a few more words about it may not come amiss. Its crepuscular habits seem to have kept it hidden from the earlier explorers, and it was not until 1845 that this singular form was made known to naturalists by the late Mr. Gray. Possessing ample wings, it disdains their use; and to such an extent has this desuetude reached that its osteology is thereby materially affected, and it stands alone among Carinate birds as having the keel of its breast-bone dwarfed into a mere ridge, such as is familiar to the anatomist as the attachment of an ordinary muscle, instead of the highly developed crest common to every other known Parrot. Prof. Huxley has even suggested a doubt whether its sternum is ossified as in other *Carinatæ;* but this seems an excess of caution on his part, though we must admit that, until an investigator such as Mr. Parker has had the opportunity of examining an embryo, the question cannot be decided.

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[*] NATURE, June 23, 1870, and Jan. 5, 1871.

[*] NATURE, Jan 5, 1871.

Of the Kakas (*Nestor*) Mr. Buller admits three species —*Nestor meridionalis*, *N occidentalis*, and *N. notabilis*— the two first of which, we think, are barely separable. This very remarkable genus of Parrots includes some two or three other species, one of which, the *N. productus* of Phillip Island, is believed to have gone the way of so many animals that only inhabit small islands, and the same fate in all likelihood awaits its congeners. Most animals suffer from not being able to accommodate themselves to change of circumstances, but the very adaptability of the Mountain-Kaka, or Kea, will tend to its early destruction; for, though belonging to the groups of Parrots distinguished by their brush-like tongue, and deriving a considerable portion of their subsistence in a manner worthy of the Golden Age from the nectar of flowers, this wretched Kea (*N. notabilis*), since the introduction of sheep to New Zealand, has incurred the imputation of a fondness for mutton-cutlets *à l'Abyssinie**, and the charge, whether true or false, is likely to bring about its doom, since the shepherd is apt to practise what in good old times was called "border justice," and the species will probably suffer extinction before its guilt is fully proved or extenuating circumstances admitted. The Common Kaka (*N. meridionalis*), on the other hand, is ably defended by Mr. Buller as one of the most useful birds in the country; yet this also is rapidly diminishing. "In some districts," he says, "where in former years they were excessively abundant, their cry is now seldom or never heard," and though he adds that "in the wooded parts of the interior they are as plentiful as ever," it requires no prophetic eye to see that, with the extension of settlements, the Kaka must succumb.

The last bird we can especially mention is the Huia (*Heteralocha acutirostris*), known to our readers by a woodcut in these columns.† Mr. Buller, we must remark, has unhappily referred it to the Hoopoes (*Upupidæ*) with which, so far as we can see, it has nothing in common. Mr. Garrod has lately informed the Zoological Society that it belongs to the Starlings (*Sturnidæ*), and there can be little doubt that he is right in doing so; but it seems also to have some affinity to the Crows; and it is worthy of remark that one of its chief peculiarities, the diversity of the bill according to sex, is shared to some extent with an aberrant corvine form (*Nucifraga caryocatactes*). A distinguished zoologist has said that "such a divergence in the beak of the two sexes is very uncommon, and scarcely to be paralleled in the class of birds. It is difficult to guess at the reason of this, or to explain it on Darwinian or any other principles." Now, to us the difficulty does not seem greater than that presented by any other sexual characteristic, and on Darwinian principles the explanation is easy enough, if once the utility of the difference is established. This last is well shown by Mr. Buller's remarks on a pair of caged Huias which he kept for more than a year, and his account justifies the belief that had previously been entertained about them:—

"But what interested me most of all was the manner in which the birds assisted each other in their search for food, because it appeared to explain the use, in the economy of nature, of the differently formed bills in the two sexes. To divert the birds, I introduced a log of decayed wood infected with the huhu grub. They at once attacked it, carefully probing the softer parts with their bills, and then vigorously assailing them, scooping out the decayed wood till the larva or pupa was visible, when it was carefully drawn from its cell, treated in the way described above, and then swallowed. The very different development of the mandibles in the two sexes enabled them to perform separate offices. The male always attacked the more decayed portions of the wood, chiselling out his prey after the manner of some Woodpeckers, while the female probed with her long pliant bill the other cells, where the hardness of the surrounding parts resisted the chisel of her mate. Sometimes I observed the male remove the decayed portion without being able to reach the grub, when the female would at once come to his aid, and accomplish with her slender bill what he had failed to do. I noticed, however, that the female always appropriated to her own use the morsels thus obtained."

Here we must pause. Mr. Buller's book is in every way worthy of its subject, and we trust that we have shown that the subject is worthy of close attention—whether we regard the various forms of New Zealand birds from the point of view of their intrinsic interest, or from that of so many being now on the verge of extinction. It is easy to be wise after the event, and ornithologists at home do not in these days look back affectionately towards their predecessors who have let so many species pass away without tracing the process of extermination. We have above hinted at some of the causes of extinction which seem to be at work; and most of them, it is to be feared, are insuperable: but there is another, and possibly more powerful cause which is entirely under control. This is the silly mania for "acclimatisation" which has been so warmly fostered by many well-meaning though ill-advised persons, both at home and in the colonies, and nowhere more than in New Zealand. The English Acclimatisation Society fortunately came to an end, and before it had time to do any harm here; but its example has been mischievous in our dependencies. In a reckless way animals of extremely doubtful advantage have been transported to the antipodes, and there it seems impossible to deny that they will in a few years be found not only ousting the kinds which are less specialised, and therefore less able to meet them on an equal footing; but, unaccompanied by any of those checks which keep the whole of a natural fauna balanced, the importations will inevitably become the greatest of nuisances. The memory of the patriotic Scot who could not live without his thistles is not exactly blessed by Australians, and among the pilgrim fathers of New Zealand who will ultimately obtain an apotheosis, the members of their various acclimatisation societies will, we suspect, scarcely be reckoned.

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* NATURE, Oct. 19, 1871, and Feb. 1, 1872.

† NATURE, June 23, 1870.

OUR BOOK SHELF

The Messenger of Mathematics. New series. Edited by Messrs. Whitworth, Taylor, Lewis, Pendlebury, and Glaisher. Vol. i. (Macmillan and Co., London, 1872.)

THE twelfth monthly number of the new series of the "Messenger of Mathematics" has just been published. This offers a convenient epoch for estimating the aims and achievements of this new mathematical periodical.

The principal aim of the editors was stated in their introductory note to be the fostering of a continuous and ample supply of original investigations into the more and more specialised branches of mathematics. This originality was to be welcomed from whatever quarter it came—whether from mathematicians of established reputation or from junior students of the science; whether from the Universities of this country or from more distant centres.

The intentions of the editors have been fully realised by the publication of the twelve numbers now before us. The list of the contributors to these numbers contains nearly twice as many names as there are numbers of the magazine. And while in that list we find the names of some of the foremost mathematicians of the age, such as Messrs. Cayley, Routh, Stokes, and Townsend, we find also a considerably greater number of the names of junior students, some of whom have only recently graduated. The localities of the contributors are also as various as was intended, one of them, for example, writing from Queensland.

As regards the articles themselves, there are about four times as many on pure mathematics as there are on applied mathematics. Among the former we find, for example, such a simple matter as a very elegant proof, by Mr. Taylor, of Euclid, ii. 8, in which he makes a further step towards the elimination of the diagonals from the diagrams of the Second Book of Euclid. This construction is recommended to the attention of the Association for the Improvement of Geometrical Teaching. We find also such interesting contributions to the study of the higher geometry as Prof. Cayley's articles on the "Theory of Envelopes," and on "Penultimate Quartics;" Mr. Merrifield's article on "Families of Surfaces;" and Mr. Townsend's on "Confocal Quadrics." The articles on other branches of pure mathematics are as varied and as instructive as those on geometry. There is, for example, a spirited controversy between Prof. Cayley and Mr. Wilkinson, about the quantitative limitations which have, in more recent times, been imposed on the generality of Taylor's theorem. Prof. Cayley's plea for greater liberty of interpretation, and against confining our symbols in mathematics so exclusively to quantity, is especially effective and well-timed; for the more liberal our interpretations the vaster will be the domains we can overrun and occupy by means of our symbols, and the greater will be the tendency to that specialisation of efforts, or division of labour, which is so characteristic of modern mathematical research, and which the publication before us aims to combine by co-operation. Then there are "Exercises on the Integral Calculus," by Sir John Cockle, and papers on "Definite Integrals," by Glaisher; besides articles treating of many other subjects, too numerous to mention.

Among the articles on applied mathematics we find one by Prof. Stokes, on the "Compound Pendulum;" two by Mr. Routh, on the "Oscillations of a Heavy String," and an improved solution of a problem in the Astronomer Royal's "Undulatory Theory of Optics;" one by M. Leclert, on "Naval Geometry;" two by Mr. Hopkinson, on "Electricity;" and so on. There is also a most elegant model of mathematical style, especially suitable for intending competitors in mathematical examinations, namely, the solutions, by Prof. Cayley, of the whole of a Smith's Prize Paper, which are of the stiffest ever set.

With all its variety of contents, this magazine is still eminently readable, principally on account of an utter absence of that tendency to riot in new terminology, or scientific slang, which disfigures the pages of some modern mathematical writers. The typography is also very good.

We shall be well satisfied if vol. ii. maintains the high standard set by vol. i.; but we should be pleased to find in it a little larger proportion of articles on applied mathematics. There is much interest felt at present in such subjects as molecular mathematics; the theory of electricity and magnetism; the determination of the centres of gravity of ships; and such like problems in advanced theory or in complicated practice. We should be glad to see the miscellaneous portion of the magazine, containing notices and reports of the meetings of mathematical societies, reviews of books, &c., somewhat extended, even if the present very moderate charge of a shilling a number was somewhat exceeded in consequence. There has been, for example, no notice of the large and influential meeting held last January by the Association for the Improvement of Geometrical Teaching. Now, in that association the want of a monthly organ is much deplored. Why should not the "Messenger" fill that void? Perhaps, also, if the size of the "Messenger" were increased, space might be found for queries and notes from correspondents; and the name might be advantageously altered to "The Mathematical Magazine." But, in any case, every student who wishes to keep abreast of the current of contemporary mathematical thought should subscribe to this excellent little periodical.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Spectrum of Lightning

ON the 11th inst. there was a considerable thunderstorm in Patterdale, and I was again able to observe the lightning spectrum. Among other lines I saw one repeatedly near D_1 and about the centre of the bright yellow band between the two masses of atmospheric absorption lines in that neighbourhood. I also saw the line near *b*. Both correspond in position with principal air-lines.

But besides this line spectrum I repeatedly saw a continuous spectrum with bright bands, which might have been the low temperature nitrogen spectrum, though I feel no certainty that such was the case. There seems, however, no doubt that lightning gives two different spectra, one of bright lines, and the other continuous; unless indeed the latter be identical with the former, but with the lines much expanded. I do not think this is the case.

HENRY R. PROCTER

P.S.—Since writing the above there has been another thunderstorm, during which I more distinctly saw the band spectrum. I find that it is not the ordinary nitrogen band spectrum, but might be a very much expanded line spectrum. It is however difficult to understand the cause of so great a difference, for the line spectrum was very sharp and well defined. I thought I was able to recognise that the latter corresponded to the shorter and sharper peals of thunder. The spectrum showed no connection with the brilliancy of the flash. I recollect that in a brilliant thunderstorm which I lately witnessed at night in Syria, some flashes lighted up the dull foliage of the prickly pear to a vivid green; while others, showing the form of the landscape with nearly equal distinctness, left it almost colourless. I had then, unfortunately, no spectroscope.

Patterdale, Penrith, July 12

Aurora of July 7

THE very fine aurora of Sunday night, July 7, was well seen near Leenane, on Killary Harbour (lat. 53° 36′, long. 9° 45′, nearly), in the west of Ireland. Probably this is the most westerly station in Europe from which the phenomenon has been observed with some approach to accuracy; and if the centre of the corona be, as it seems to be (*pace* some of your correspondents), an actual, substantive point, and not merely the effect of perspective, the following observations may help in determining its height above the earth. At 11.0 exactly, Greenwich time, the centre of the very well developed corona had an altitude of 68°, with an azimuth bearing of 21° E. of S., a little W. of magnetic S. (these measurements being taken as correctly as could be done with a good-sized compass furnished with a clinometer), and it was distant from α Lyræ (*Vega*) about 8° towards S.W.; this distance being afterwards diminished by the rotation of the earth. The aurora was observed independently by Messrs. Kinahan and Symes, of the Irish Geological Survey, in the same neighbourhood, and they also noted the proximity of the centre to the above-mentioned star. I do not trouble you with other details.

M. H. CLOSE

Newtown Park, Black Rock, Dublin, July 13

I WITNESSED last night what must have been a somewhat brilliant display of aurora. From 10.45 P.M. to 11.0 P.M slight breaks in the N.E. were brightly lit up with a rosy glare, and another break in the N. with the green rays. Coruscations of light flashed up from time to time, and were visible even through the clouds, which consisted of somewhat dense nimbus. The display in a clearer sky must have been decidedly fine.

Bridport, Dorset, July 8 J. EDMUND CLARKE

Registering Thermometer

MR. WHIPPLE and Mr. Bushell have shown great ingenuity in suggesting remedies for the copious condensation of moisture which takes place in the interior of the jackets of minimum thermometers on the grass.

Both their suggestions I have found fail to work a permanent cure; the chloride of calcium (only of use when the stopping is perfect) soon becomes super-saturated, and itself gives off moisture. The putty and sealing-wax hold good for about five or six months, and then the insidious moisture permeates again. Would it not be better to adopt my very simple plan of discarding the jacket altogether? It is absolutely useless, and no stopping is really effectual in resisting for long the daily extremes of heat and cold and damp and dryness these thermometers must undergo.

I have used a thermometer on the grass for the last three years entirely unmounted, and find its indications in no way effected by the loss of its outer coat.

CHARLES H. GRIFFITH

The Rectory, Stratfield Turgiss, Winchfield, Hants,
July 5

Luminous Matter in the Atmosphere

I WAS much interested by M. Waldner's short article on "Luminous Matter in the Atmosphere" in the number of NATURE for Feb. 15, 1872. Being unable to see the particles described, I applied to him for further directions, and he was kind enough to inform me that they may be seen *à vision directe* with a telescope.

I have since found that many of the little bodies may be seen with the naked eye by shutting out the powerful direct and lateral rays. This may be done, *e.g.*, by partly closing the *jalousies* or outside shutters used here, and then looking for the particles at about the distance of quarter of a degree, or of the sun's radius from the border of his disc, when the sun is either just below the upper edge of the shutter, or immediately above the ledge of the house if it looks east and west. Shutters are not always necessary. I have just seen great numbers by standing on the border of the shadow thrown by the adjoining house. Even by holding my hat over my head I can perceive some of them. The chief object to be aimed at is to prevent the eyes from being dazzled by the direct rays of the sun. The same principle explains the reason why stars are sometimes seen during the day from the bottom of a well or through a telescope, and why the red protuberances of the sun become visible during an ordinary or artificial eclipse.

On the 9th and 10th current, and again this morning, there was a haziness round the sun, which gradually diminished as the angular distance from his disc increased, until the sky became beautifully blue at an angle of 25° or 30°. I found that the haziness was produced by the reflection of the sunbeams from innumerable little particles. Many of them were distinctly visible to the naked eye, but many more were seen with an opera-glass or telescope. They passed too rapidly to be counted, but fifty at least were in the field of my glass at one time. I am sure that this is no optical delusion, for several of my friends have seen them too.

What were these little particles? M. Waldner supposes them to be crystals of ice, and they certainly look like miniature snowstorms. Perhaps some of your readers may be able to decide whether the higher particles are composed of ice-like cirri, the loftiest of the clouds. But some of the corpuscles of the lower strata of the air are undoubtedly seeds, little organic substances, insects, &c. I have caught several feathery little seeds of this kind. They are almost imperceptible when seen against some white substance, and are so light that the slightest currents of air waft them to great distances.

Another instance of numberless little vegetable substances being blown here may be cited, namely, the pollen of the pine trees growing on the Landes. An unusually large quantity of this fell here on April 17, 18, 19, and 20, and may probably be traced to three extensive fires of pine woods sixty or eighty miles N.W. of Pau (April 14-16). The pollen was doubtless drawn upwards by a strong current of heated air, and then wafted to Pau by the wind, which blew in the right direction (April 17-19). The wind changed at Bordeaux on the 20th, and the pollen then fell (April 21-22) near Perigueux, nearly as far to the N.E. of the fires as Pau is to the S.E. A correspondent of the *Times* (April 30), mistaking the pollen for sulphur, announced that a shower of this substance had fallen here, and supposed that it was connected with the eruption of Vesuvius, which, however, did not begin until the 24th.

I would suggest that some record should be kept of the direction of the wind which these particles indicate in different strata of the atmosphere in fine weather. J. F. ANDERSON

4, Place Duplace, Pau, June 12

Vibration of Glasses containing Effervescing Liquids

IT is known that a glass containing effervescing liquid will not give a clear note when struck, and that as the effervescence subsides the tone becomes more and more clear. When the liquid is perfectly tranquil the glass will ring as usual, but on re-exciting the effervescence the musical tone again disappears. Sir John Herschel (Encyc. Met., Art. "Sound"), who states that this experiment appears to have been originally made by Chladin, quotes it as an "example of the stifling and obstruction of the pulses propagated through a medium, from the effect of its non-homogeneity;" and, in explanation of the phenomena, he says:—"We must consider what passes in the communication of vibrations through the liquid from one side of the glass to the other. The glass and contained liquid, to give a musical tone, must vibrate regularly in unison as a system; and it is clear, that if any considerable part of a system be unsusceptible of regular vibration, the whole must be so."

The phenomenon, then, according to this explanation, is due to the fact that the liquid during effervescence, becomes non-homogeneous, and thus obstructs the passage of the sonorous vibrations from one side of the glass to the other.

It is with much diffidence that I venture to express dissent from so eminent an authority as Herschel; but it does not appear to me that the above explanation is entirely satisfactory, for the following reasons:—

1. It seems probable that the sonorous vibrations pass *round* the glass rather than *across* it. For, if they pass across the glass, that will occur whether it contains water or air. Yet the musical tone of a glass containing air is not destroyed by suspending within it, so as not to touch it, a ball or cylinder of wool or cotton, although the sonorous vibrations certainly cannot pass freely through that substance.

2. If the non-homogeneity of the contained liquid be the cause why the sonorous vibrations will not pass, whence comes it that treacle, clear honey, &c., which are homogeneous *fluids*, destroy the musical ring of a glass fully filled with any of them?

The phenomenon presents itself to my mind as being due to a certain amount of vibration communicated to the glass by the agitation arising from the effervescence. This vibration—which can be easily heard by placing the ear close to the glass—interferes with that caused by striking the glass, and destroys more or less the proper rhythmic movement necessary to the production of a musical note, according as the intensity of the agitation of effervescence is greater or less.

The dead sound of a cracked glass is probably owing to a similar cause. For in that case, as soon as the vibrations travelling round the glass arrive at the crack, the edges of which are wholly or partially in contact, they are transmitted from edge to edge, and as, owing to the friction of the edges one against the other, their vibrations do not synchronise, a reflex wave is impinged upon each, having a less velocity than the original wave. This reflex wave will correspond to the vibrations caused by effervescence. If the crack be cleanly cut out, so as to separate the edges by a well-defined interval, the glass will again emit a musical note. In the latter case, the sonorous vibrations, on arriving at the cut portion, return by the way they came, synchronising with those which they meet.

The dead sound of the glass, when filled with honey or treacle, is probably owing to the circumstance of these fluids being not sufficiently mobile to vibrate in unison with the glass; and thus they destroy its musical tone as effectually as if they generated an independent and non-synchronous vibration.

London, July 4 ALLEN BEAZELEY

The Names Cambrian and Silurian in Geology

WILL you allow me to express, as an humble worker among the rocks of North Wales, my sense of the high value of the contributions to your pages recently by Prof. Sterry Hunt on the "History of the names Cambrian and Silurian in Geology?" I have long felt—and have not hesitated to express my feeling—that a great wrong was done to Prof. Sedgwick when the North Wales groups of rocks from the Bala Beds to the Lingula Flags—the order of which he was the first to unravel in that difficult region—were unceremoniously engulphed in Siluria.

It has also appeared to me one of the greatest anomalies in English geological classification, that the magnificent and well-defined groups of North Wales should be typified by their attenuated and broken easterly outcrops in the Silurian district of South Shropshire. What Llandeilo section of Siluria is there that worthily represents the Arenig and lower Bala rocks immediately east or west of the Berwyn Mountains? What Caradoc section of Siluria is there at all worthy of the fine series of the Upper Bala rocks of Glyn Ceiriog? Of the unworthiness of the schists near the Stiper stones to represent the Lingula beds of North Wales, Prof. Hunt justly speaks in his papers.

I am glad that justice seems at last likely to be done to the veteran, Prof. Sedgwick, than whom a more philosophical geologist I am persuaded does not exist. Let but a sufficient number of scientific men resolve to use his older and truer, because more natural classification, and the justice will soon be complete.

D. C. DAVIES

ON THE VARIATION OF SPECIES AS RELATED TO THEIR GEOGRAPHICAL DISTRIBUTION, ILLUSTRATED BY THE ACHATINELLINÆ

IT has long been known that island species are usually different from, but allied to, those of the neighbouring continents. Darwin has also made us familiar with the fact that each of the Galapagos Islands has a fauna, and to some extent a flora, of its own. Other explorers have called attention to the somewhat limited distribution of species in the West Indies and on other islands. I have been informed by Mr. T. Bland, who has given special attention to the terrestrial molluscs of the West Indies, that if Cuba should be divided into two islands by the submerging of the central portion, about half of the species on either of these islands would be different from those on the other. Some of the most remarkable facts of this kind appear in the distribution of the Achatinellinæ on the Sandwich Islands. As they have never been fully recorded, I make the following brief statement of the leading facts, gathered from the results of personal exploration, and suggest a few inquiries.

Many types of the Sandwich Island Helicidæ have at different times been classed under the generic name of *Achatinella*. These widely differing forms have, in the structure of the shell, one point of correspondence that holds them together. The columella has a spiral twist which is more or less apparent in all. In most of the species this character is so strongly developed that the columella seems to be armed with a lamellated tooth revolving within the shell. This common characteristic, in connection with the fact that they are all confined within the limits of one small geographical area, affords sufficient reason for regarding even the most divergent of these types as belonging to one group. As the humming birds are peculiar to America, so the Achatinellinæ are peculiar to the Sandwich Islands.

Though the forms thus brought together evidently constitute a natural group, it has long been apparent that they should be classed under more than one generic name. Some of these genera are restricted to one or two islands.

Genera on Kauai

Several large turreted species of a peculiar type, found only on the island of Kauai, had been provided for at different times under the names of *Achatina*, *Achatinella*, and *Spiraxis*; but no resting-place was found for them till shelter was provided under the separate name of *Carelia*, given by H. and A. Adams. *Carelia turricula*, a species which is sometimes three inches in length and about an inch in diameter, may stand as the representative of this genus. Besides the six or eight species of *Carelia* which have been described, there are many other species of land shells peculiar to this island, the most northern and western of the group. Some of these are *Helices*; the others belong to *Amastra* (H. and A. Adams) and *Leptachatina* (Gould), two genera which are also represented on the other islands of the group. None of the species of this island present any of the brilliant colours that are so common in the shells of Oahu. The peculiar forms of some of the species, as of *Amastra Kauaiensis* and *Carelia cumingiana*, as well as the relations of these aberrant types to the types found on the other islands, render them objects of great interest.

Genera on Oahu

On the island of Oahu, which lies next to Kauai on the south-east, we find a remarkable development of the Helicidæ. The ground species belong to the two genera just mentioned, *Amastra* and *Leptachatina*. *A. ventulus* is an example of the former, and *L. vitrea* of the latter. Over twenty-five species of each have been found on this island. Two arboreal genera—the *Bulimella* (Pfeiffer) and *Helicterella* (Gulick)—are found only on this island. The ellipsoidal form, as in *B. rosea*, characterises the former; and the conical form, as in *H. apiculata*, the latter. Of *Bulimella* there are about thirty known species; of *Helicterella* thirty-five. The different species of *Bulimella* present a great variety of colours, ranging from bright green and rose, through yellow, brown, and ash, to simple black and white. The prevailing colours of the *Helicterella* are white, black, and brown, variously arranged in bands and stripes. The arboreal genus *Achatinella* (Swainson) may also be regarded as belonging especially to Oahu, as it is here represented by fifty-four species, and elsewhere by but three, which are found only on the island of Molokai, about fifteen miles to the east. *Achatinella producta*, about one inch in length, is one of the largest of the family. The *Auriculella* (Pfeiffer) is a genus of small arboreal species found on Oahu, and also on the islands to the east. Many of them are unnamed; but those on Oahu probably number more than ten. *Auriculella auricula* is given by Pfeiffer as the type. Two other arboreal genera—*Partulina* (Pfeiffer) and *Laminella* (Pfeiffer)—which find their chief development on the islands of Maui, Molokai, and Lanai, are represented on this island by three species each. The types, as given by Pfeiffer, are *Partulina virgulata*, found on Molokai, and *Laminella gravida*, on Oahu.

Number of Species and Varieties on Oahu, and the Regions they Inhabit

The number of species of Achatinellinæ on Oahu may be estimated at about 185, representing eight genera, but belonging chiefly to five. Besides these there are many small *Helices*, which would probably bring the whole number of the species of Helicidæ on Oahu up to about 200, the varieties numbering 800 or 900. None of these species—excepting, perhaps, one or two of the ground species, which are reported to have been found on the island of Maui—exist anywhere beyond the narrow circuit of this island, the extreme length of which is only 60 miles, with an average breadth of about 15 miles. Nor does any one species occupy a large proportion of even this area. Nearly all are confined to the forest regions skirting two ranges of mountains. The chief range, on the north-east side of the island, is about 40 miles in length. The forest region that covers it has an average breadth of five or six miles. The range on the opposite side of the island is about half as long, and has only about one-fourth as much forest land. The north-eastern side of the island, owing to its receiving the trade-winds when they first strike the island, enjoys a moister climate and possesses more luxuriant vegetation than the south-western side. We are therefore somewhat prepared to find that seven-eighths of the species, or about 175, are found in the former area, about 40 miles in length and five or six miles in width. Passing over innumerable minor variations, the varieties found in this area are no less than 700 or 800.

That so large a number of species and varieties of land molluscs should be found within so small an area is, I think, unparalleled in the records of conchology; but that this great number of forms should, with but two or three exceptions, be found nowhere beyond these narrow limits, not even on the other half of the same island, is still more astonishing. What shall we say when we discover that no one of these species is distributed over even half of this small mountain range; but that in most cases they are restricted to areas of from one to five miles in length? Have we found one of the "centres of creation"?

The principal facts in the geographical distribution of these forms are as follows:—

I.—*Facts relating to the Position and Natural Divisions of the Territory*

The Sandwich Islands are surrounded by a wider expanse of open ocean than any other islands of equal extent. The forms of Helicidæ on this group differ widely from those of other lands. Not only do we find distinct species and genera, but a separate group of genera.

The group of islands may be divided into four provinces, each of which has a separate set of species and possesses one genus or more that is peculiar to the province, besides other genera that are common to several provinces. (*a*) On Kauai alone is found the *Carelia;* (*b*) on Oahu, the *Bulimella* and *Helicterella;* (*c*) on Maui, Molokai and Lanai, the *Newcombia* (Pfeiffer); (*d*) on Hawaii, certain peculiar forms that have not yet been fully collected or classified. Kauai, which is separated from the other islands by the widest channel, has the forms that differ the most widely from those of the central part of the group.

Most of the species are confined to the forests of mountain regions; and where, as on Oahu and Maui, there are two regions of forest divided by several miles of grass country, the island is also divided into two sections, having but few, if any, species in common.

On the island of Oahu, the two sections which occupy separate mountain ranges are divided into many minor sections in the following manner. From each side of the main range project mountain ridges, which separate deep valleys a mile or two in width. Each of these valleys is a subordinate section, having its own varieties and in many instances its own species, which are found nowhere else.

II.—*Facts relating to the Variation and Affinity of Species*

Nearly all the species of one genus found on one mountain range are connected by varieties presenting very minute gradations of form and colour. Species of the same genus on different islands are not so completely connected by intermediate forms.

The degree of difference between several species of the same group is in proportion to their separation in space.

Nearly allied species, occupying neighbouring localities, pass into each other by all the intermediate gradations of form and colour, while those whose homes are separated by a distance of eight or ten miles, cannot be connected by minute gradations without bringing in some of the forms occupying the intermediate territory.

As the relations to each other of the valleys surrounding any mountain are determined by the shape of the mountain with its ridges, so are the relations of these species to each other, in the arrangement of their affinities and divergences, influenced by the same cause. As the geometrical relations to each other of valleys clustered around one central peak differ from those distributed on either side of a long range, so do the affinities and divergences (the structural relations to each other) of the species on one of the high solitary mountains of Maui differ from those on one of the mountain ranges of Oahu. On the eastern range of Oahu the species of *Achatinella* are distributed on both sides of the mountain in parallel lines, the extremes of divergence being in the forms at the ends of the range. But either on East Maui or West Maui, where the arrangement of the valleys is more concentric, the varieties of any one group of species converge so rapidly toward one central type, that it is difficult to distribute them into well-defined species.

III.—*Facts relating to the comparative Area occupied by Species of different Classes*

The average length of the area occupied by different species is perhaps five miles.

Field species have the widest range. Arboreal species have the narrowest range. Ground species found in forest regions have a medium range.

Many interesting questions are suggested by these facts. How can we account for the species being restricted in their distribution to such narrow limits? Why do not the species of North-eastern Oahu pass over their narrow bounds and become mingled throughout the whole extent of that short mountain range?

Questions Suggested

The minute gradations by which the species of each genus of the Achatinellinæ are connected with each other strongly favours the belief that many of them must have been derived the one from the other by successive variations. If created independently, why should there be such gradation? Why should the species of one group be so arranged that those intermediate in form are found in intermediate localities? It may be said that those of one group, which are gradated together by intermediate varieties, are not only from one stock, but are one species. If they are one species, how shall we account for the difference of size and form, the entire change of colour, and in many instances of habits, leading some of the so-called varieties to avoid plants that are chosen by other varieties living only a few miles distant, and to choose plants that are rejected by the others? Why should the *Achatinellæ* feed on Kukui trees (*Aleurites triloba*) in the eastern districts of the island, and in the north-western choose small shrubs, leaving the Kukui trees to the *Bulimellæ?* Whether we call the different forms species or varieties, the same questions are suggested, as to how they have arisen, and how they have been distributed in their several localities.

In attempting to answer these questions, we find it difficult to point to any of those active causes of accumulated variation, classed by Darwin as illustrations of

"Natural Selection." The conditions under which they live are so completely similar, that it does not appear what ground there can be for difference in the characters best fitting the possessors for survival in the different valleys in which they are found. The vegetation is much the same; the bird and insect enemies, so far as they have any, are the same. The north-east side of the mountain range is a little more rainy than the opposite side, but this does not account for the different forms found in the successive valleys on the same side of the range. In what respect can the conditions of survival to which *Achatinella Stewarti* is subjected in Manoa, differ from those under which *A. producta* lives in Makiki, only a mile distant, or from those in which *A. varia* is placed in Palolo, three miles away? There is no reason to doubt that some varieties less fitted to survive have disappeared; but it does not follow that the "Survival of the Fittest"—(those best fitted when compared with those dying prematurely, but equally fitted when compared with each other)—is the determining cause which has led to these three species being separated from each other in adjoining valleys. The "Survival of the Fittest" still leaves a problem concerning the distribution of those equally fitted. It cannot be shown that the "Survival of the Fittest" is at variance with the survival, under one set of external circumstances, of varieties differing more and more widely from each other in each successive generation. The case of the three species under consideration does not seem to be one in which difference of "Environment" has been the occasion of different forms preserved in the different localities. It is rather one in which varieties resulting from some other cause, though equally fitted to survive in each of the three localities, have been distributed according to their affinities in separate localities. There is no reason to think that *A. producta* is not as well fitted to live on the Kukui trees that abound in Manoa and Pololo, as on the same trees in Makiki. Again, is the "Survival of the Fittest" sufficient to explain their being kept within these extremely narrow limits since they were produced? One would at first suppose that, in the course of a few years, or in a few hundred years at the farthest, the three species would have been diffused throughout this area of only five or six square miles which is now divided between them.

We seek in vain for an explanation of these facts in the still further principle of variation, set forth by Herbert Spencer under the effects of change in use, and discussed by Prof. Cope under the names of "Acceleration and Retardation." This cause of accelerated variation has influence only where there is a difference in the "use, either compulsory or optional." If, on the one hand, the change is compulsory, it must be owing to a change in external circumstances. But in the case of these three species we are unable to find any difference in their circumstances requiring change. Their enemies are the same, the climate is the same, and they undoubtedly eat the same food, for the chief resort of all is the Kukui tree. If, on the other hand, the change in the use is optional, and without reference to change in circumstances, it belongs to the class of spontaneous variations, and does not explain why those of one type of variation (or of one kind of choice) should be brought together and limited to so small an area.

Relations of the Genera

The relations of the genera of Achatinellinæ involve problems of still greater interest, but more difficult to penetrate. The limits of this paper render it impossible to do more than to give some of the most striking facts, and indicate some of the questions that arise.

Through the varieties of *A. oviformis* and *B. Sowerbyana*, the genus *Achatinella* passes by minute gradations into *Bulimella*; but connections of this kind have not been noted between the other genera.

The family is divided into two natural groups of genera. The first group consists of seven genera: *Achatinella*, *Bulimella*, *Helicterella*, *Partulina*, *Newcombia*, *Laminella*, and *Auriculella*. These are all arboreal in their habits. In form they are either sinistral, or both dextral and sinistral. The second group consists of three genera: *Amastra*, *Leptachatina*, and *Carelia*. With but few exceptions, the species of *Amastra* and *Leptachatina* live on the ground and are of dextral form. I am not informed concerning the habits of *Carelia*, but the structure of the shell and its invariably dextral form show that it belongs to this group.

Of the second group, *Carelia* is found on Kauai, the most western of the Sandwich Islands. The two remaining genera are found on all the islands. The first, or arboreal group, is represented on all the islands except Kauai. The separate genera are more restricted in their distribution. Two are found only on Oahu, a third on Oahu and Molokai, a fourth on Molokai and Maui, and the remaining two on several islands.

The genus *Helix* is represented on all the islands. So far as I know, the species all live on the ground, and are all dextral in form. They are all small in size, with spire very much depressed, and have no trace of the peculiar twist in the columella which characterises the Achatinellinæ.

Why should nearly all the ground species be dextral, and many, if not a majority, of the arboreal species be sinistral? Does this fact point to one common origin for the arboreal genera, and a separate origin for the ground genera? Or are we to suppose that arboreal habits tend to produce sinistral forms? The few species of *Amastra* which are found on trees retain the dextral form that belongs to the allied species living on the ground.

Facilities needed for the Study of Variation of Species

I am fully persuaded that the study of allied forms in their geographical relations is one of the richest fields open to the naturalist. He may here reap a harvest of facts throwing light on many of the questions that are now occupying the special attention of the scientific world.

To afford suitable opportunity for such studies, it is necessary that certain sections of our museums should be devoted to the exhibition of objects in an arrangement more strictly geographical than anything that has yet been attempted. The leading feature in the arrangement adopted by Agassiz in the museum at Cambridge, Massachusetts, is the geographical grouping of objects; but for the fuller presentation of the curious facts of geographical distribution, it is further needed that in certain wisely chosen families the objects should be laid down in their actual geographical relations, as on a map. It is not necessary that the map on which they are arranged should be as mathematically correct as a nautical chart. It will be sufficient if cases are prepared, approximately representing the territory or territories chosen, with subdivisions representing the different localities in which the specimens have been found.

Collections for such a purpose should be made with scrupulous care. The locality of every object should be noted with great minuteness. In collecting shells at the Sandwich Islands, noting the name of the island is not sufficient, nor yet the name of the district. Each valley, with its area two or three miles in length, and but one or two miles in width, needs to be separately explored, and all the shells labelled with the name of the valley. To show the relations of the species to each other, as complete a series as possible should be obtained of the countless varieties.

For this kind of study the fauna of the Sandwich Islands is of peculiar interest, on account of the number of forms, and the variety of relations presented within a small compass.

JOHN T. GULICK

EVANS'S STONE IMPLEMENTS OF GREAT BRITAIN[*]

II.

ONE of the arguments usually relied upon in support of the belief in fluviatile, as opposed to diluvial, agency in the formation of the deposits in which the Stone Implements are found, is founded on the assumption that the constituents of these quaternary gravels are petrologically such, and only such, as belong to existing river basins; and this fact, Mr. Evans says, holds good in France and England, and cannot be too often reiterated. Without pausing to consider how far this argument might avail as against those who, like Dr. Buckland, believe in a simultaneous and universal cataclysm, it seems hardly applicable to the conditions under which the implement-bearing drifts are found; for if the term petrological is to be understood as meaning rocks found *in situ* in the river basins, and thus native to the soil, then it is not the fact that the constituents of the gravels in question belong to those basins; for we know that they are often largely made up—in one instance cited by Mr. Evans to the extent of 50 per cent.—of the quartzose stones known as Lickey pebbles, and rounded fragments of jasper, quartz, and other foreign rocks. Such rocks certainly do not belong petrologically, in the proper sense of that term, to the river basins in which they occur, but to strata of a far earlier date. As Dr. Buckland has shown, the quartzite pebbles are derived from the New Red sandstone beds in Warwickshire and Leicestershire, and were at some remote period forced over the escarpment of the Oolite into the south and east of England. Whether they were brought in before or after the present river valleys were formed is not very clear, nor perhaps very material. It is incontestable that they were transported from a great distance, and possibly by the same forces that brought the flint gravels; and it is equally certain, in several instances, that their transport cannot be attributed to rivers now in action, because those rivers flow, as at Brandon, towards the quarter from which the stones were brought.

Nor, if it were certain that the intrusion of these rocks dated back to the Glacial Epoch, as is usually supposed, or to some other very distant period, and had thus become denizens, if not natives of the soil, could the inference which is drawn from the absence of extraneous rocks be regarded as satisfactory.

The occurrence of alternate elevations and depressions of the land above or below the sea level, during the post-glacial times, has been suggested by several English writers; and if we suppose that a district comprising the south of England and the north of France, corresponding, or nearly so, with that in which no boulder clay is found, to be sufficiently depressed, and then invaded by a deluge, the argument drawn from petrological conditions will cease to apply; for no rocks are found in the drift gravels but such as belong to the supposed deluge basin. A deluge of short duration would not necessarily introduce any foreign rocks into the submerged area, but would sweep into hollows and valleys those that came in its way; and even should the submergence be of long continuance, as in some provinces of Holland, it would leave no more traces than those exhibited in our drift gravels. That such a partial deluge was both possible and probable is evident when it is considered that a depression of 600 ft. would perfectly well effect it; and as we have evidence that the land has risen in several places 30 ft. and more within the historical period, it is not difficult to believe that in the infinitely longer time that probably intervened after the Glacial Epoch the same process of elevation may have been going on for many ages.

The absence of all traces of a marine fauna, and the occasional presence of land and freshwater shells in these beds, are circumstances on which much stress is laid by the author; but when fully considered they hardly seem to warrant the inferences drawn from them. A marine fauna requires a marine flora for its sustenance, and unless the submergence had been of long duration this could not have existed. We find extensive marine deposits of older date, in which no marine organisms are ever seen; and if marine fossils are wanting in drift beds, those of the land and freshwater are usually equally wanting. We have, probably, hundreds of square miles of quaternary gravels, in which not a single specimen has ever been discovered. In those instances, comparatively rare, in which they occur in the implement-bearing beds, they are usually lying above the gravel, and may thus be ascribed to a later date; or if of an earlier date in some instances, their occurrence would not of necessity exclude diluvial action, as regards the gravels.

There is one interesting topic connected with these drifts, which Mr. Evans has not dealt with at any length, as, indeed, it barely came within the design of his work; but he seems to share the general opinion that the men who made and used the drift implements were contemporary with the hippopotamus, elephant, rhinoceros, and other animals, with whose remains they are often found associated. At present this is but a possibility, and it is an assumption founded on the fact of the bones and implements being often found in close proximity, but if, as seems probable, the implements were formed from stones found in the gravels in which they now rest, it can hardly be doubted that the bones were already in that gravel, and may have lain there for centuries. From their shattered and way-worn condition, they have evidently been subjected to much rougher usage than that which some of the flint implements have met with. But however this may have been, there can be no doubt, as Sir Charles Lyell has observed in the "Antiquity of Man," that "the fabrication of the implements must have preceded the reiterated degradation which resulted in the formation of the overlying beds;" a process for which vast periods must be allowed, and one which must have involved important geological changes. Amongst others we have very strong reasons to believe was the severance of our island from the Continent, an event, indeed, which, however brought about, could hardly have been unattended with important changes in the contour of the adjacent districts, and the courses of their rivers. When we contemplate the vast changes, geological, palæontological, and geographical, which our race seems to have survived, we are surprised to learn how very old we are, or, as Mr. Evans has better expressed it, the mind is almost lost in amazement at the vista of antiquity thus displayed.

It would seem, as might be expected, that notwithstanding the cosmopolitan character of these objects—for, as Mr. Evans's researches have shown, they are found in one form or other in every country on the face of the globe—certain forms are pretty well confined to certain localities, as if each of the tribes or families who used them had its own manufacture. The half-polished and polished celts of Norfolk, Suffolk, and Cambridgeshire vastly outnumber those which have been observed in all other parts of England, from which it would seem that these counties were more populous, or the people more advanced in the arts, than in the rest of the island, or possibly they may have been the manufacturing district of the period. As regards, however, the distribution of the drift implements, a far more suggestive and important circumstance is to be noticed. As Mr. Evans has observed, the district farthest north of the Thames in the gravels of which flint implements are at the present time known to have been found, is the basin of the River Ouse and its tributaries. They have, in fact, been found at one time or other, in every English county lying to the south-east of a line drawn from the Severn to the Great Ouse, corresponding thus far with the great escarpment of the oolite, but they have never been met beyond that line; and it is an interesting subject of speculation to what the dearth of these objects in the country lying to the north-west is to be attributed. If it was habitable and inhabited, it is difficult to imagine a reason for their absence, especially as in Yorkshire and Lincolnshire there is abundance of suitable chalk flint. This line of demarcation is not very much out of that which separates the boulder clay districts from those in which no boulder clay is met with. May it not have been the case, that when the implements were fashioned, Scotland and the north-western parts of England were still submerged beneath the glacial sea, and that on their emergence the south-east became in its turn depressed?

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[*] "The Ancient Stone Implements, Weapons, and Ornaments of Great Britain." By John Evans, F.R.S., F.S.A. (London: Longmans and Co., 1872.)

Notwithstanding all that has been written on the subject, there seems to be still much doubt as to the uses for which some, and no inconsiderable number, of these objects were designed. For all useful purposes it would have sufficed that the cutting edge of a celt should alone be polished and ground; yet it is often, indeed usually, found that the entire surfaces of the faces and the sides exhibit a polish which could only have been obtained by long and apparently profitless labour. And not only so, but many of these are very fragile, being slightly made, and of delicate workmanship, and others are of such small dimensions, that, as M. Boucher de Perthes pointed out, they never could have been available for any kind of hard work. Many of these exhibit no signs whatever of fracture or even of scratching, either at the butt or the edge, indications which could not possibly have been wanting had they ever been used for weapons or tools. Besides which, while many of the districts in which they are found contain abundance of rocks suitable for all ordinary purposes, these implements are often made from Asiatic jade, jadeite, tremolite, serpentine, green porphyry, nephrite, and other stones of beautiful colours, and capable of taking a high polish, many of which must have been brought from great distances, and would have been very costly both to import and to work. The museums in Brittany, and particularly that at Vannes, are very rich in jadeite implements of this kind, but they are also found frequently both in England and Scotland. That of which a figure is here given (Fig. 1) was found in Burwell Fen, Cambridgeshire, and is described by Mr. Evans as being exquisitely polished, and a mottled pale green colour; the material is of a hard diorite, and as both faces are highly polished the labour bestowed on the manufacture must have been immense.

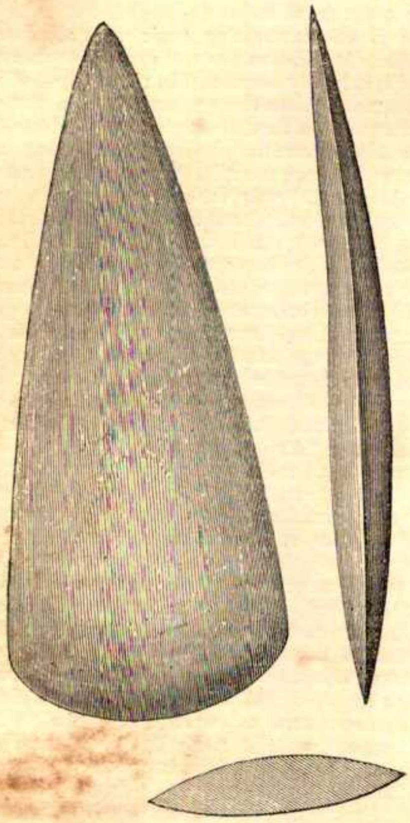
FIG. 1.—POLISHED CELT, BURWELL FEN, CAMBRIDGESHIRE

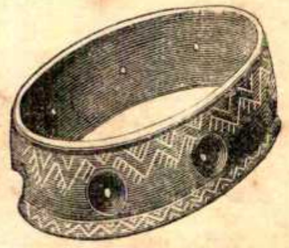
FIG. 2.—JET ARMLET, GUERNSEY

But if we conclude, as we must, with the author, that implements for which such beautiful and intractable materials were selected, could hardly have been in common use, we may indulge in some speculation as to what were the uses they were designed to serve, notwithstanding that, as Mr. Evans says, we have not sufficient ground for arriving at any trustworthy conclusion. M. Boucher de Perthes thought that they were deposited by the survivors in the graves of deceased friends, as useful to them on their resurrection, and he argued from this their belief in a future state. It seems, however, hardly probable that objects, many of which obviously could not be serviceable, should be placed in tombs under the belief that they would be so at some future date. In the absence of any more satisfactory explanation, it may be suggested that these things were intended by our remote predecessors to represent the deities whom they worshipped, and that by their varied sizes and shapes they indicated the ranks and orders of their idols. We may believe that men not having learned the art of representing the human or animal form, were obliged to content themselves with symbols of their divinities—it may be their Mars and Ceres—under the form of weapons of war, or instruments of agriculture. Nor is this so unlikely as it might otherwise appear, when we know that these celts are still objects of worship in India. Mr. Evans, quoting from the Proceedings of the Asiatic Society of Bengal, says that they are there venerated as sacred, and it is known that in a certain village in the Shewaroy hills some hundreds of polished celts, of varying sizes, resembling those found in England and Scotland, are preserved in a temple, arranged in rows. They are guarded with the utmost jealousy by the priests, each representing

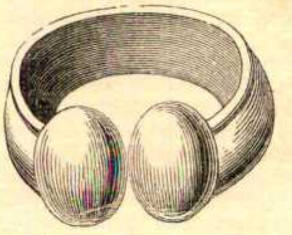
FIG. 3.—BRONZE ARMLET, GUERNSEY

some particular *swamy* or deity, and each receiving from time to time a dab of red or white paint, as a proof that the priest has performed before it the customary *poojah* or worship.

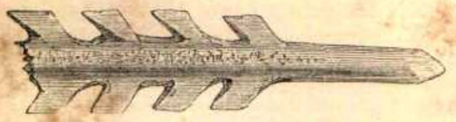

FIG. 4.—HARPOON HEAD, KENT'S CAVERN

This being so, the discovery of these implements in Europe may have some bearing upon an important ethnological question. We have good reason to believe that the dolmen-builders came, in the first instance, from India, for we find in Wilts and Berks, and elsewhere, exact counterparts of some megalithic structures, and those of a peculiar construction, which yet remain in the same Shewaroy district in which the celt worship is still practised. May we not then regard it as possible that the fabrication of polished implements, as well as the practice of dolmen building, originated in India, where they are still retained, and that these costly polished celts were brought hither by our Aryan ancestors, as the Israelites carried their Teraphim about with them, or as the Trojans, after the fall of their city, are represented in Virgil as carrying with them their household gods :—

> "Ilium in Italiam portans, victosque penates ;"

and that the worship was only abandoned here as men became enlightened, or were subjected to the dominion of some race of a different theology? Since we find abundant traces of the Aryan language in our own, and of their sepulchral architecture in our dolmens, why should we not find in our fields and fens some of their idols? It is quite consistent with, and in a certain sense confirmatory of, such a belief, that in almost every country in which these things are found, they are regarded by the common people with superstitious reverence, as if the practice of adoration had in the lapse of ages merged in a vague and faint tradition of sanctity.

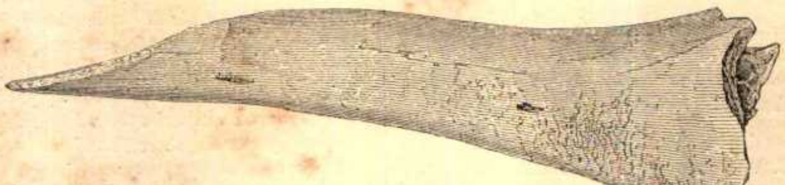

FIG. 5.—BONE AWL, KENT'S CAVERN. (1835.

Nor is it any objection to this hypothesis, but the reverse, that these implements are usually found in and about dolmens, as at Tumiac and Mont St. Michel, where nearly seventy highly polished celts of imported materials—Asiatic jade and hard tremolite—were found ranged in regular order. It has been usual with almost all people, in all ages, that those things which they most esteemed in life should rest with them in their graves ; and as we often find in our own country the priest's paten and chalice placed in his coffin, or the Anglo-Saxon's sword and shield laid beside him in the earth ; so, possibly, these Prehistoric men may have wished that the stone idols which, when living, they adored—the Lares and Penates of their time—should be laid beside them in their tombs.

But in pursuing the train of thought suggested by our author, we had well-nigh forgotten his book, and we have only space to congratulate all those who are interested in these researches—and they are now many—on the ample and valuable additions which he has made to this new and most interesting chapter in the history of our race.

NOTES

THE following officers have been elected for the Brighton Meeting of the British Association :—President-elect—Dr. William B. Carpenter, F.R.S. Vice-Presidents-elect—The Earl of Chichester, the Duke of Norfolk, the Duke of Richmond, the Duke of Devonshire, F.R.S., Sir John Lubbock, Bart., M.P., F.R.S., Dr. Sharpey, Sec. R.S., Mr. Joseph Prestwich, F.R.S., Pres. G.S. Section A : Mathematical and Physical Science. President—Warren De La Rue, F.R.S. Vice-Presidents—J. Norman Lockyer, F.R.S.; Lord Rosse, F.R.S; Prof. H. J. Stephen Smith, F.R.S. Secretaries—Prof. W. K. Clifford, R. A. Proctor, A. C. Ranyard. Section B : Chemical Science. President—Dr. J. Hall Gladstone, F.R.S. Vice-Presidents—F. A. Abel, F.R.S.; Prof. Williamson, F.R.S. Secretaries—Dr. Mills ; W. Chandler Roberts ; Dr. W. J. Russell, F.R.S.; T. Wood. Section C : Geology. President—R. A. C. Godwin-Austen, F.R.S. Vice-Presidents—Thomas Davidson, F.R.S.; Prof. P. M. Duncan, F.R.S.; Rev. T. Wiltshire. Secretaries—Henry Woodward, Louis C. Miall, G. Scott, William Topley. Section D : Biology. President—Sir John Lubbock, Bart., M.P., F.R.S. Vice-Presidents—John Ball, F.R.S.; Dr. Beddoe ; Prof. Flower, F.R.S.; Colonel A. Lane Fox ; J. Gwyn Jeffreys, F.R.S.; Dr. Burdon Sanderson, F.R.S. Department of Zoology and Botany. Sir John Lubbock, Bart., M.P., will preside. Secretaries—Prof. Thiselton Dyer ; H. T. Stainton, F.R.S. Department of Anatomy and Physiology. Dr. Burdon Sanderson, F.R.S., will preside. Secretaries—Dr. Gamgee, F.R.S.; E. Ray Lankester ; Dr. Rutherford ; Dr. Pye-Smith. Department of Anthropology. Colonel A. Lane Fox will preside. Secretaries—Dr. Charnock, F. W. Rudler, J. H. Lamprey. Section E : Geography. President—Francis Galton, F.R.S. Vice-Presidents—Clements R. Markham ; Major-General Sir Henry Rawlinson, Bart., F.R.S., Pres. R.G.S.; Major-General Strachey, F.R.S. Secretaries—H. W. Bates, A. Keith Johnston, Rev. J. Newton, J. H. Thomas. Section F : Economic Science and Statistics. President—Prof. Henry Fawcett, M.P. Vice-Presidents—R. Dudley Baxter, William Newmarch, F.R.S. Secretaries—J. G. Fitch, Edmund Macrory, Barclay Phillips. Section G : Mechanical Science. President—Frederick J. Bramwell, C.E. Vice-Presidents—John Hawkshaw, F.R.S.; C. W. Merrifield, F.R.S.; Charles B. Vignoles, F.R.S. Secretaries—H. Bauerman, J. Gamble. The First General Meeting will be held on Wednesday, August 14, at 8 P.M. precisely, when Prof. Sir William Thomson, F.R.S., will resign the Chair, and Dr. W. B. Carpenter, F.R.S., will assume the Presidency, and deliver an Address. On Thursday Evening, August 15, at 8 P.M., a Soirée ; on Friday Evening, August 16, at 8.30 P.M., a Discourse on Insect Metamorphosis, by Dr. P. M. Duncan, F.R.S.; on Monday Evening, August 19, at 8.30 P.M., a Discourse by Prof. Clifford; on Tuesday Evening, August 20, at 8 P.M., a Soirée; on Wednesday, August 21, the Concluding General Meeting will be held at 2.30 P.M.

THE next meeting of the American Association for the Advancement of Science will not be held at San Francisco, as previously announced, but at Dubuque, commencing August 21.

THE following telegram respecting the Livingstone Search Expedition has been forwarded from the office of the Submarine Telegraph Companies in India:—"ADEN, July 11.—Stanley arrived, and leaves to-day by French steamer for Suez, with Livingstone's son. Has letters from Livingstone to the Government and friends. Found Livingstone unwell, but determined to go farther on, and not return before completing perfectly his work. Stanley's men return and accompany him."

IT is stated in *L'Institut* that the Académie des Sciences of Paris, at its last two sittings, has been again discussing in secret whether it will do itself the honour of admitting Mr. Darwin as a corresponding member in the section of Zoology.

THE chair of Natural History at Owens College has been divided. Prof. W. C. Williamson, F.R.S., retains the charge of Animal Phisiology and Zoology and Botany; whilst Geology has been erected into an independent lectureship, and committed to Mr. W. Boyd-Dawkins, F.R.S., the curator of the Natural History Museum.

WE learn from *Les Mondes* that the French Budget Commission has opened provisionally to the Minister of Public Instruction a credit of 100,000 francs, to be appropriated to the collection of the special instruments necessary to the observation of the transit of Venus which takes place on December 8, 1874. The work of collection is being pursued with great activity at the Imperial Observatory, under the direction of M. Alphonse Martin.

THE Academy of Sciences in Bologna has announced that a prize of 1,200 *lire* (48*l.*), the "Aldini Prize," will be awarded to the author of the best scientific experimental essay on galvanism or dynamic electricity. Essays intended for the competition must be sent in between July 1, 1872, and June 30, 1874, and must be written in Italian, Latin, or French. They must be either written or printed; but, in the latter case, must not have been published previously to the two years above mentioned. Each essay is to bear a motto, and to be accompanied with an envelope stating the name of the author. They must be addressed to the Perpetual Secretary of the Academy of Sciences of the Bologna Institution.

THE *Melbourne Argus* states that valuable work is being performed with the great telescope at the Melbourne Observatory. At a recent meeting of the Royal Society, Mr. Ellery, the Government Astronomer, stated that some photographs of the moon had been obtained better than any he had any knowledge of. The picture of the moon taken in the telescope was about three inches in diameter, while the primary pictures of the photographs of the moon hitherto made public by Mr. De La Rue were only three-quarters or seven-eighths of an inch in diameter, though subsequently enlarged to something like two feet.

THE *Times of India* of June 21 prints at length the judgment of the Marine Court of Inquiry into the disasters attending the recent cyclone at Madras. The judgment contains the following chronicle of the weather changes during the three days preceding the cyclone:—On the afternoon of Monday, April 29, the wind shifted from the S.S.E. to the N.E., a bank of clouds forming to the S.E. The weather became gloomy, with lightning; the barometer falling slightly. On Tuesday morning the barometer had fallen, wind and sea had increased from the N. and E., with squalls, rain, and suspicious appearances generally, surf rising, and the current at the shipping changing to the S. During the night there were heavy squalls, with rain, thunder and lightning. It was blowing hard in gusts; sea increasing, and weather still getting worse. On Wednesday, May 1, the weather had a very wild appearance, a heavy squall with surf; wind about N.N.E. Very heavy squalls, with rain; fresh gale between the squalls. Towards afternoon it was blowing very hard; barometer falling. The barometric tide existed, but the mercury was lower in the tube, the current at the shipping setting stronger to the S. There appears to have been a break in the clouds to the N.E. in the evening, and the wind moderated, though still blowing hard, the barometer remaining depressed. Towards midnight on Wednesday the barometer commenced to fall rapidly, and the wind and sea increased to fury; several of the native vessels broke adrift, followed, after daylight, by the English vessels, to which this inquiry is directed. On consideration of the whole evidence, the court is "forced to a conclusion that during the day and night of Tuesday, April 30, the weather was such as should have induced any prudent man to take every precautionary measure for the safety of his ship; and that from Wednesday morning there should have been no reasonable doubt that the premonitory symptoms of a cyclone existed;" especially "at a season of the year when bad weather and cyclones are expected on this coast." Experience, the judgment declares, shows that the chances in favour of a vessel surviving a cyclone by putting to sea in time greatly preponderate. It will be seen from this that no blame whatever is attributed by the Court of Inquiry to Mr. Pogson.

THREE very remarkable paintings are now being exhibited at No. 7, Haymarket, called "Arctic Summer," "Crushed among the Icebergs," and "Arctic Wreckers." In the year 1869 the artist, Mr. Bradford, chartered the steamer *Panther*, of St. John's, Newfoundland, and, inviting Dr. Hayes, the well-known arctic explorer of America, to accompany him, sailed from the above port for the sole purpose of getting sketches of the hitherto unknown frozen north. Reaching the latitude of 76° N. in Melville Bay, he remained there until the middle of August, when the new ice began to form so fast that it was with great difficulty they could force their way through the pack. Two experienced photographers accompanied the expedition, and obtained most wonderful views of the arctic scenery. From the photographs, and his own sketches, Mr. Bradford has now produced these paintings, which must give a more perfect idea of the wonders of the arctic regions to those who have never visited them than any description could possibly do. It is difficult otherwise to realise the gigantic height of some of the ice-walls, the sea-fronts of the glaciers, from which the huge icebergs break off and drift southwards. Mr. Bradford will shortly publish a work, illustrated by over eighty of the finest photographs, showing the different phases of life in those regions—the great glaciers, fiords, mountains, Esquimaux life, icebergs, and the ice phenomena of Melville Bay; the edition will be limited to 250 copies.

THE first number is issued of a new monthly magazine, price 6*d.*, entitled *Grevillea*, a record of Cryptogamic Botany and its literature, edited by Mr. M. C. Cooke. It is intended as a medium of communication among cryptogamists, chronicling discoveries of new species, physiological observations, and other matters of interest, and will doubtless fill a useful place. In the present number, which is illustrated by a coloured plate, the fungologist, lichenologist, bryologist, algologist, and diatomologist will each find something to interest. If well supported by those interested in this branch of botany, it ought to become indispensable to all who wish to become *au courant* with the present position of cryptogamy.

PROFESSOR AGASSIZ'S SOUTH AMERICAN EXPEDITION*

II.

FOR reasons which I will explain presently, I would mention especially pebbles of a red porphyry, and others of a green compact epidote, as common in the Port San Antonio formation. The position of the Bay of San Mathias, its great length, and more particularly the depression or denudation at Port San Antonio, suggest the probability that the Rio Negro once poured its waters into this large gulf instead of opening directly into the ocean. I should add that while erratic pebbles occur in such abundance at San Mathias Bay, there are no hard rocks in place upon which the peculiar marks of glacial action could be perceived. Nor would these stratified banks of pebbles, even though unquestionably connected with the drift, afford in themselves any unmistakable evidence of glacial derivation.

As time and the circumstances of our vessel obliged me to renounce the hope I had cherished of seeing at least the mouth and the shore bluffs of the Rio Negro, Santa Cruz, and Gallagos Rivers, and also of visiting the Falklands, I could not connect my observations in San Mathias Bay with any other facts on the eastern coast of Patagonia or its outlying islands. But after rounding Cape Virgens we came into Possession Bay, where the geology along the shore was of a most interesting character. All along the northern shores of the Straits of Magellan the tertiary formation observed on the eastern shore of Patagonia is plainly distinguishable even from a distance by its horizontal beds, which are also visible upon the Fuegian coast. In Possession Bay we landed to examine more closely the character of the country, some of us with the intention of exploring more particularly the terraces above the shore bluffs; while others were bent upon a longer excursion to Mount Azmon and adjoining hills.

About a mile from the shore bluff I found, nearly 150 ft. above the sea level, a salt pool in which, to my great surprise, marine shells identical with those now living along the shore were abundant. They were in a perfect state of preservation, and many of them were alive; so that I gathered a number of specimens with the living animal, which I have preserved in alcohol. The most common were *Mytilus*, *Buccinum*, *Fissurella*, *Patella*, *Voluta*, &c., all found in apparently the same numerical relation as that in which they now exist in the sea below the cliff. The presence of this pool with its living inhabitants shows a very recent upheaval of the coast. The period at which it may have taken place it is hardly possible to determine without a more extensive survey. As the facts stand, it is a most valuable confirmation of Darwin's assertion of recent upheavals on this shore, published more than thirty years ago; though he attributes phenomena to this cause, and connects with it facts which had, in my opinion, a different origin and another significance. At the season of our visit to Possession Bay, in March, when autumn is approaching in this hemisphere, the pool was nearly dry, and the little water left in it was intensely saline. Dr. White has examined it chemically, and handed me the following report of his analysis:—"The specimen of water obtained from the pond at Possession Bay was found to contain magnesia, lime, sulphuric acid, chlorine, a small quantity of iron, and a trace of iodine. It was about 2½ times as dense as ordinary sea water, as shown both by hydrometric observation and by the total amount of chlorine present; organic matter in excess." The shores of the pond showed plainly that in the rainy season it is three or four feet deep, when no doubt the water is more like sea water than at the time of our visit. From the innumerable tracks of guanacos, it must be the constant resort of these animals, and, indeed, during the day we saw many of them moving in that direction. A more palpable evidence of upheaval has not, as far as I know, been observed before. Dead marine shells scattered over dry land are not always conclusive evidence of the former presence of the ocean, for they may have been dropped by birds or other animals; but a salt pond more than 100 ft. above the sea level, with the same shells alive as those now found on the shore, could only be produced by an upheaval. The land beyond the first shore bluff is horizontal. It rises in regular terraces to about 400 ft. above the level of the sea. This is also the general level of the country, the surface of which is much ravined and furrowed. I counted eight such terraces above the beach. They all consist of tertiary deposits; but upon the beach itself three lower levels may be distinguished, their relative age being marked by the presence or absence of vegetation on the sand. Upon the third terrace, a little above and more inward than the salt pool, at the height of at least 150 ft. above the sea, I found a distinct moraine, in which the scratched pebbles were mingled with the simply rounded ones in as large a proportion as in any front moraine in actual contact with a glacier. This moraine was arched, with the convexity turned northward, and the abrupt slope southward, showing that the motive power which had brought and left it there must have moved from the south in a northerly direction. Higher up, to a level of about 400 ft. above tide water, there are also erratics scattered over the plain. At the level of 400 ft.—the highest to which I ascended—I saw a number of large, angular boulders.

Here are facts, then, of great significance in close proximity, namely, a pool containing marine shells alive, more than 100 ft. above the level of the sea, showing a very recent rise of the tract of land it occupied; and an accumulation of pebbles and boulders having all the characters of a glacial moraine, resting upon one of numerous terraces, which seem to mark successive upheavals of the country. That these retreating levels only simulate the successive steps of a gradual upheaval, and are in fact no evidence of such an occurrence, is proved by the geological constitution of the ground, which is entirely made up of regular tertiary beds, without a trace of shore pebbles. Darwin, who has also observed the phenomena of subsidence and upheaval characteristic of this region, was led to believe that the drift was scattered over Patagonia by icebergs while the country was submerged. The moraine upon one of these terraces mentioned above shows, however, that the upheaval must have taken place before the dispersion of the drift, and not after. I say nothing here of Pourtales's very interesting discovery of an extensive field of extinct volcanoes to the north of Possession Bay, of which Mount Azmon is the largest, since he has already sent you an official report on the subject. His observations are among the most valuable results of our geological work. The suggestion presents itself at once that the upheaval of the region may be connected with the former activity of those volcanoes. Throughout the eastern part of the Straits of Magellan the shores exhibited tertiary formations such as we had traced along the Atlantic coast of Patagonia and in San Matthias Bay. Of course in deposits of this kind we could not expect to find any trace of smooth polished rocks.

The last localities of recent geological age which we examined carefully were Elizabeth and Magellan Islands. The latter is almost entirely made up of glacial drift, among which there are a good many large and small boulders with very smooth surfaces and characteristic scratches, and some of them are of the same red porphyry and epidote mentioned before. At Sandy Point large accumulations of boulders are scattered over the whole country, and the streets of the settlement are paved with them. They are easily observed in their natural position on the banks of the river, and in the cuts of the railroad leading to the coal mines. Here the drift is auriferous. Señor Viel, governor of the colony, gave me very fine specimens of the gold collected in the immediate neighbourhood of the settlement. I was also much interested in the coal deposit. There are two considerable seams of coal, one 6 ft. 6 in. thick, and another, 80 ft. higher up, 3 ft. thick. The few species of fossils which I obtained there in great quantity left upon me the impression that the coal is not tertiary, but belongs to the cretaceous formation. The most characteristic of these fossils is an oyster, of the type of the *Ostrea deltoidea*, forming beds many feet in thickness.

After passing Sandy Point the country assumes a completely different aspect. The mountains rise to great heights on both sides of the channel, in consequence of which the region may be compared to the Alps, even though the loftiest peaks, such as Mount Sarmiento, Mount Darwin, Mount Buckland, Mount Barney, only measure from 6,000 ft. to 7,000 ft. But as their base is washed by the ocean, and their slope is very steep, they appear much higher than they really are.

The neighbourhood of Sandy Point will ever be especially interesting to Swiss geologists, from the fact that it recalls many familiar scenes. Pourtales and I greatly enjoyed the comparison with home scenery. In his work on the "Rocks of the Two Hemispheres," and in his "Kosmos," Humboldt repeatedly alludes to the striking similarity of the features exhibited by the inorganic world in regions very distant from each other, and I only follow in his footsteps if I say that Sandy Point and the tracks north of it recalled to me the Jura and the more level country at its foot, while the higher ranges to the south reminded me of the Alps. The comparison might be carried into detail without exaggeration. The first chain in sight from the channel, in which the coal deposits are found, rises only to about 1,000 ft., and resembles the Neocomian hills skirting the western Jura; while the second chain, rising to about 2,500 ft., may be compared to Chaumont, or some other of the less elevated summits of the same range. Even the ravine leading to the coal mine brought back to me the gorges on Seyon, with its torrent; while the flats below stand in the same relation to the hills as the alluvial Pointe du Bied and the tertiary plain of Berex holds to the Jura. This resemblance is not simply superficial; it actually extends to the geological structure of the whole region. The higher mountains to the south, though recalling the Alps, should not be compared with the highest Swiss ranges, such as Mont Blanc, Mont Rosa, or the Bernese Oberland; they have more the character of the Osmonds. Mont Jura, for instance, when seen from the north, reminds one of the Niesen, or some of the conical heights rising above Meilleiries, such as the Cornette de Brise; when seen from the east it may be likened to the Untersberg, near Saltzburg. Mount Sarmiento, Mount Buckland, Mount Barney, and many others less known, have truly the character of the highest Alps. Mount Buckland resembles the Matterhorn very strikingly in form, except that its surface is entirely shrouded in ice.

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* Reprinted from the *New York Tribune*.

It was not till we rounded Cape Froward that I felt confident that the range of hills immediately in sight along the channel we followed had assumed their present appearance in consequence of abrasion by ice. Now, however, that I have seen the whole length of the Straits of Magellan, have passed through Smyth's Channel, and visited Chiloe, I am prepared to maintain that the whole southern extremity of the American Continent has been uniformly moulded by a continuous sheet of ice. Everywhere we saw the rounded undulating forms so well known to the students of glacial phenomena as *roches moutonnées*, combined with the polished surfaces scored by grooves and furrows running in one and the same direction; while rocks of unequal hardness, dykes traversing other rocks, slates on edges, were all cut to one level. In short, all the surface features of the Straits of Magellan have much the same aspect as the glaciated surfaces of the Northern Hemisphere. Whenever the furrows and scratches were well preserved their trend was northern.

I have recorded carefully every locality having a special interest in reference to those facts. I will here only mention a few of the most characteristic ones. The first unquestionable *roches moutonnées* I saw were upon the nearest coast opposite Cape Froward—as the English maps have it—where the rocks are bent and twisted, as those of the Dent du Midi and the Dent de Morchi. Cape Frouart—for such is the French name given by Frezier to the southernmost promontory of the continent—is itself rounded and polished, most especially on its south-west exposure, with rugged crests as above the Grimsel in the Nägeli's Grath. All the hills between Snug Harbor and Wood Bay are equally rounded and polished to their very top. Even the wooded part of the slope shows the characteristic undulations of glaciated hill-sides. Many hills and mountains east and west of Cape Holland exhibited the same aspect. I was particularly struck with the appearance of a gentle slope between Cape Holland and Point Coventry, the surface of which exhibited some naked knolls distinctly glaciated, while the wooded part of the hill had the same form. All these mountains recall the Vor Alps, such as the Moleson, the Faulhorn, the Rhigi, and the Pilates, rather than the Alps themselves, even when entirely covered with *névé* or ice. These rounded knolls and glaciated surfaces penetrate frequently into the narrow coves which open into the main channel, in a north-south direction, at right angles with the Straits themselves—thus showing that the grinding agent must have moved from the south northward or from the north southward, and not from east to west or from west to east, as the Straits mainly trend. In Port Gallant, I saw large and small pebbles, and large boulders, many at least 6 ft. in diameter, and one measuring 12 ft. by 6 ft. and 5 ft., well rounded, and more or less polished, with rectilinear scratches in different directions all over their surface—in fact, such as are only found in genuine ground moraines.

The whole of Fortescue Bay, with the exception of a small land beach, on which we found a Fuegian camp, is covered with erratics. Even within high and low water mark many pebbles still show glacial scratches, though they are constantly tossed to and fro by the tides. Pourtales had the good fortune to be the first to see genuine glacier scratches, above Port Gallant, upon polished rocks in place. It was upon the surface of a quartz dyke traversing talcose slate. The trend of the scratches was west-north-west. There are *roches moutonnées* all the way from Fortescue Bay to Jerome Point, Cross Mountain included. Jerome Point itself is well polished, especially on the south side. York River Valley, which trends northward, is also well polished on both sides. Between the last two ranges of Jerome Point, westward, there is a cove trending northward, in which the *roches moutonnées* are as characteristic as upon the sides and face of the whole Point. The gorge opposite is equally *moutonnée* on both sides, showing that the denudation has not yet been the work of an agent moving east-west or west-east through the main channel. The two heads of the narrowest part of the straits (El Morion and Cape Gnod) are beautifully polished and rounded. The last range of Jerome Point seems to show that the abrading cause acted from S.S.W. to N.N.E. In Borgia Bay the ground is covered with large pebbles and boulders, some of the largest of which are rounded, polished, and scratched. Pourtales and Kennedy ascended the peak marked 1,923 upon the Admiralty map of Borgia Bay, and found *roches moutonnées* to the height of about 1,500 ft., while higher up the rocks were broken into rugged ridges. The whole scenery reminded me of the Abschwartz, above the glacier of the Aar. Some of the polished surfaces resembled, in the most surprising manner, places represented in my works upon the glaciers, and might have served as models for the illustrations I published of the glacial phenomena in Switzerland more than thirty years ago. No promontory in the whole extent of the straits, seen from either its eastern or its western side, shows as probable a strike-side of the polishing agent as the north and south exposures; leading to the presumption that the planing-machine has moved north and south, even though every surface seems almost equally well polished. Nothing indicates the fitful action of icebergs. Glacier Bay has also been for me a most fruitful field of study; but of this more in detail later. In the harbour of Sholl Bay there are several concentric moraines marked by boulders and kelp, which may have been deposited by the great glacier on the opposite side of the channel.

With all the evidences of glacier action constantly before our eyes, the journey from Cape Frouart to Cape Tamar was nevertheless tantalising to me, because it gave no opportunity for tracing the facts in unbroken continuity. The course of the Straits of Magellan bearing mainly in an east-westerly direction cuts everywhere at right angles, the effects produced by the southern ice-shoes upon the solid foundation of the whole track. Only after we had rounded Cape Tamar and passed Sholl Bay did we enter a channel bearing in the same direction with the glacial erosion, and thus affording an opportunity of following connectedly on the opposite sides of the whole channel, as far as the Gulf of Pennas, the traces left by glaciers upon the surface of the rocks. Here, as in reference to the Straits of Magellan, I shall describe only such localities as have a marked interest, reserving more details for another occasion. The facts spoke so plainly that even those not familiar with them were struck by their distinctness. Following the inside route through Smyth's Channel to the Gulf of Pennas, we were all the way within touching distance of the rocky walls of those narrow passages, so that nothing could escape us, and as the intricacy of the channels forebade travelling by night, we lost nothing in that way.

The Andes proper begin at Cape Providence, within the Straits of Magellan, but their alpine character is not strikingly developed south of Union Sound, even though at the bottom of Glacier Sound very high mountains with large glaciers may be seen. Mount Burney may be compared to Mount Sarmiento; still, throughout Smyth's Channel, until coming into Collingwood Strait, through Victory Pass, the scenery is very much like that of the Straits. In the southern parts of Smyth's Channel, I for the first time noticed un unmistakable difference between the southern and northern exposures of the nearer ranges trending N.S. Here it became every hour more plain that the strike-side of the glacial agency was upon the southern slope, and the lee side upon the northern. As soon as the Cordillera of Sarmiento opens into view the grandeur of the range is fully displayed. From the highest mountains glaciers depend to the sea level, which may be fairly compared to the most impressive glaciers of Switzerland. Throughout this region, as well as in other parts of the Straits, the nomenclature of the islands and mountains, as adopted upon the Admiralty chart, has a character very pleasing to a scientific man, and very creditable to those who have wished to connect the memory of their distinguished contemporaries and friends with their own investigations. Indeed, the names of all the prominent men of England, distinguished for their devotion to science thirty-five or forty years ago, form now a part of the physical geography of these regions. With these are associated some foreign names which, however, are not always so happily applied, very eminent names being, in some instances, given to very insignificant localities. We had twice a beautiful view of Mount Burney; first coming up through Mayne Channel, where we had an opportunity of seeing the vast difference between its aspect when covered with snow to the very base, as represented by Dr. Cunningham, and as we saw it, with its upper part only shrouded in perpetual snow and ice. It will be long before the real level of perpetual snow is ascertained in these regions, as any boisterous day may change the appearance of a mountain range to an astonishing degree. The mountains to the north of Cape Providence, Mount Burney, the Cordillera of Sarmiento, and the mountain ranges east and north of Snowy Glacier, form part of one and the same chain, and are in reality the southern termination of the Andes. L. AGASSIZ

(To be continued.)

ON THE SPECTRUM OF THE GREAT NEBULA IN ORION, AND ON THE MOTIONS OF SOME STARS TOWARDS OR FROM THE EARTH*

IN my early observations of the spectrum presented by the gaseous nebulæ, the spectroscope with which I determined the coincidence of two of the bright lines respectively with a line of nitrogen and a line of hydrogen, was of insufficient dispersive power to show whether the brightest nebular line was double, as is the case with the corresponding line of nitrogen.

Subsequently I took some pains to determine this important point by using a spectroscope of greater dispersive power. I found, however, that the light furnished by the telescope of eight inches aperture, to which the spectroscope was attached, was too feeble, even in the case of the brightest nebulæ, to give the line with sufficient distinctness when a narrow slit was used. The results of this later examination are given in a paper I had the honour of presenting to the Royal Society in 1868. I there say† :—

"I expected that I might discover a duplicity in the line in the nebula corresponding to the two component lines of the line of nitrogen, but I was not able, after long and careful scrutiny, to see the line double. The line in the nebula was narrower than the double line of nitrogen; this latter line may have appeared broader in consequence of irradiation, as it was much brighter than the line in the nebula." When the spark was placed before the object-glass of the telescope, the light was so much weakened that one line only was visible in the spectroscope. "This line was the one which agrees in position with the line in the nebula, so that under these circumstances the spectrum of nitrogen appeared precisely similar to the spectra of those nebulæ, of which the light is apparently monochromatic. This resemblance was made more complete by the faintness of the line; from which cause it appeared narrower, and the separate existence of its two components could no longer be detected. When the line was observed simultaneously with that in the nebula, it was found to appear but a very little broader than that line." I also remark :—"The double line in the nitrogen-spectrum does not consist of sharply defined lines, but each component is nebulous, and remains of a greater width than the image of the slit ‡. The breadth of these lines appears to be connected with the conditions of tension and temperature of the gas. Plucker states that when an induction-spark of great heating-power is employed, the lines expand so as to unite and form an undivided band. Even when the duplicity exists, the eye ceases to have the power to distinguish the component lines, if the intensity of the light be greatly diminished." I state further :—"I incline to the belief that it [the line in the nebula] is not double."

One of the first investigations which I proposed to myself when, by the kindness of the Royal Society, I had at my command a much more powerful telescope, was the determination of the true character of the bright line in the spectrum of the nebula, which is apparently coincident with that of nitrogen. From various circumstances, chiefly connected with the alterations and adjustments of new instruments, I was not able to accomplish this task satisfactorily until within the last few months.

Description of Apparatus

It seems to me desirable to give a description of the spectroscopic apparatus with which the observations in this paper were made. In the former paper, to which I have already referred, I gave some reasons* to show that the ordinary method of comparison, by reflecting light into the spectroscope by means of a small prism placed before one half of the slit, is not satisfactory for very delicate observations unless certain precautions are taken. I then describe an arrangement for this purpose, which, with one or two modifications, is adopted in the collimator constructed for use with the Royal Society's telescope. I give the description from that paper† :—

"The following arrangement for admitting the light from the spark appeared to me to be free from the objections which have been referred to, and to be in all respects adapted to meet the requirements of the case. In place of the small prism, two pieces of silvered glass were securely fixed before the slit at an angle of 45°. In a direction at right angles to that of the slit, an opening of about $\frac{1}{10}$ inch was left between the pieces of glass for the passage of the pencils from the object-glass. By means of this arrangement, the spectrum of a star is seen accompanied by two spectra of comparison, one appearing above, and the other below it. As the reflecting surfaces are about 0·5 inch from the slit, and the rays from the spark are divergent, the light reflected from the pieces of glass will have encroached upon the pencils from the object-glass by the time they reach the slit, and the upper and lower spectra of comparison will appear to overlap to a small extent the spectrum formed by the light from the object-glass. This condition of things is of great assistance to the eye in forming a judgment as to the absolute coincidence or otherwise of lines. For the purpose of avoiding some inconveniences which would arise from glass of the ordinary thickness, pieces of the thin glass used for the covers of microscopic objects were carefully selected, and these were silvered by floating them upon the surface of a silvering solution. In order to ensure that the induction-spark should always preserve the same position relatively to the mirror, a piece of sheet gutta-percha was fixed above the silvered glass; in the plate of gutta-percha, at the proper place, a small hole was made of about $\frac{1}{20}$ inch in diameter. The ebonite clamp containing the electrodes is so fixed as to permit the point of separation of these to be adjusted exactly over the small hole in the gutta-percha. The adjustment of the parts of the apparatus was made by closing the end of the adapting-tube, by which the apparatus is attached to the telescope, with a diaphragm with a small central hole, before which a spirit-lamp was placed. When the lines from the induction-spark, in the two spectra of comparison, were seen to overlap exactly for a short distance the lines of sodium from the light of the lamp, the adjustment was considered perfect. The accuracy of adjustment has been confirmed by the exact coincidence of the three lines of magnesium with the component lines of *b* in the spectrum of the moon."

The modifications of this plan consist in the substitution of a thin silver plate polished on both surfaces for the pieces of silvered glass. The opposite side of the silver plate to that from which the terrestrial light is reflected to the slit reflects the images formed by the object-glass to the side of the tube where a suitable eye-piece is fixed. This arrangement forms a very convenient finder, for it is easy to cause the image of the star to disappear in the hole in the silver plate. When this is the case the line of light formed by the star falls on the slit, and its spectrum is visible in the spectroscope. This collimator is so constructed that, by means of a coupling screw, any one of three spectroscopes can be conveniently attached to it.

This apparatus performs admirably; but it seemed to me desirable, for observations of great delicacy, to be able to dispense with reflection, and to place the source of the light for comparison directly before the slit. Formerly I accomplished this object by placing the spark or vacuum-tube before the object-glass of the telescope. The great length of the present telescope renders this method inconvenient; but a more important objection arises from the great diminution of the light when the spark is removed to a distance of 15ft. from the slit. I therefore resolved to place the spark, or vacuum-tube, within the telescope at a moderate distance from the slit. For this purpose holes were drilled in the tube opposite to each other, at a distance of 2ft. 6in.

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* By William Huggins, LL.D., D.C.L., F.R.S. Paper read before the Royal Society, June 13, 1872

† Phil. Trans 1868, pp. 542, 543

‡ Phil. Trans. 1863, p. 13.

* Phil Trans. 1868, pp. 537, 538.

† Phil. Trans. 1868, p. 538.

within the principal focus. Before these holes short tubes were fixed with screws; in these tubes slide suitable holders for carrying electrodes or vacuum-tubes. The spark is thus brought at once nearly into the axis of the telescope. The final adjustment is made in the following manner:—A bright star is brought into the centre of the field of an ordinary eye-piece; the eye-piece is then pushed within the focus, when the wires or vacuum-tube can be seen across the circle of light formed by the star out of focus. The place of discharge between the electrodes or the middle of the capillary part of the vacuum-tube is then brought into the centre of the circle of light. The vacuum-tubes are covered with black paper, with the exception of a space about a ¼ inch long in the middle of the capillary part, through this small uncovered space alone can the light escape to reach the slit.

The accuracy of both methods of comparison, that by reflection and that by the spark within the tube, was tested by the comparison of the three bright lines of magnesium and the double line of sodium with the Fraunhofer lines *b* and D in the spectrum of the moon. I greatly prefer the latter method, because it is free from several delicate adjustments which are necessary when the light is reflected, and which are liable to be accidentally displaced.

Spectroscope A is furnished with a single prism of dense glass with a refracting angle 59° 42′, giving 5° 6′ from A to H.

Spectroscope B has two compound prisms of Mr. Grubb's construction, which move automatically to positions of minimum deviation for the different parts of the spectrum. Each prism gives about 9° 6′ for minimum deviation from A to H.

Spectroscope C is furnished with four similar prisms.

The small telescopes of the three spectroscopes are of the same size. Diameter of object-glass 1¼ inch; each is furnished with three eye-pieces magnifying 5·5, 9·2, and 16·0 diameters.

Spectrum of the Nebula of Orion

With spectroscopes A and B four* lines are seen.

First line.—With spectroscope B and eye-piece 1 and 2, the slit being made very narrow, this line was seen to be very narrow, of a width corresponding to the slit, and defined at both edges, and undoubtedly not durable. The line of nitrogen when compared with it appeared double, and each component nebulous and broader than the line of the nebula. This latter line was seen on several nights to be apparently coincident with the middle of the less refrangible line of the double line of nitrogen. This observation was on one night confirmed by observation with the more powerful spectroscope C.

The question suggests itself whether, under any conditions of pressure and temperature, the double line of the nitrogen-spectrum becomes single; and further, if this should be found to be the case, whether the line becomes single by the fading out of its more refrangible component, or in what other way the single line comes to occupy the position in the spectrum, not of the middle of the double line, but that of the less refrangible of the lines.

I stated in my former paper that when for any reason the light from the luminous nitrogen is greatly reduced in intensity, the double line under consideration is the last to disappear, and consequently a state of things may be found in which the light of nitrogen is sensibly monochromatic when examined with a narrow slit.† Under these circumstances the line of nitrogen appears narrower, and the separate components can be detected with difficulty, if at all.

I stated also that the breadth of the component lines appears to be connected with the conditions of density and temperature of the gas. As was to be expected from theoretical considerations, the lines become narrower and less nebulous as the pressure is diminished. My observations of this change seemed to show that the diminution of the breadth of the lines takes place chiefly at the outer sides of the lines, so that in the light from very rarefied gas the double line is narrower, but the space of separation between the components is not as much wider as would be the case if the lines had equally decreased in width on the sides towards each other.

When the pressure of the gas is reduced to about 15 inches of mercury, the line spectrum fades out to give place to Plucker's spectrum of the first order. During this process a state of things occurs when, for reasons already stated, the spectrum becomes *sensibly* monochromatic when viewed with a narrow slit and a spectroscope of several prisms. The line is narrower, and remains double, and has the characters described in the preceding paragraph.

As the pressure is diminished, the double line fades out entirely, and the spectrum of the second order gives place to the spectrum of the first order. When, however, the pressure becomes exceedingly small, from 0·1 inch to 0·05 inch, or less, of mercury, there is a condition of the discharge in which the line again appears, while the other lines remain very faint. Under these conditions I have always been able, though with some difficulty on account of the faint light when the necessary dispersive power (spectroscope B with second or third eye-piece) and a narrow slit are used, to see the line to be double, but it is narrower than when the gas is more dense, and may be easily mistaken for a single line. I have not yet been able to find a condition of luminous nitrogen in which the line has the same characters as those presented by the line in the nebula, where it is single and of the width of the slit.

Upon the whole I am still inclined to regard the line in the nebula as probably due to nitrogen.

If this should be found to be the case, and that the nebular line has originally the refrangibility of the middle of the double line of nitrogen, then we should have evidence that the nebula is moving from the earth. The amount of displacement of the nebular line from the middle of the nitrogen double line corresponds to a velocity of 55 miles per second from the earth. At the time of observation the part of the earth's orbital motion, which was from the nebula, was 14·9 miles per second. From the remaining 40 miles per second would have to be deducted the probable motion from the nebula due to the motion of the solar system in space. This estimation of the possible motion of the nebula can be regarded as only approximate.

If the want of accordance of the line in the nebula with the middle of the double line of nitrogen be due to a recession of the nebula in the line of sight, there should be a corresponding displacement of the third line as compared with that of hydrogen. For reasons which will be found in a subsequent paragraph, I have not been able to make this comparison with the necessary accuracy.

In my former paper* I gave reasons against supposing so large a motion in the nebula; these were based on the circumstance that the nebular line falls upon the double nitrogen line, which the present observations confirm. I was not then able to use a slit sufficiently narrow to show that the nebular line is single and not coincident with the middle of the double line of nitrogen.

I am still pursuing the investigation of the parts of this inquiry which remain unsettled.

Second line.—This line was found by my former comparison to be a little less refrangible than a strong line in the spectrum of barium. Three sets of measures give for this line a wave-length of 4,957 on Angstrom's scale; this would show that the line agrees nearly in position with a strong line of iron. At present I am not able to suggest to what substance this line belongs.

This line is also narrow and defined. I suspect that the brightness of this line relatively to the first line varies in different nebulæ.

Third and fourth line.—My former observations show that these lines agree in position with two lines of the spectrum of hydrogen, that at F and the line near G.

These lines are very narrow, and are defined; the hydrogen, therefore, must be at a low tension.

The brightness of these lines relatively to the first and second lines varies considerably in different nebulæ; and I suspect they may also vary in the same nebulæ at different times, and even in different parts of the same nebula, but at present I have not sufficient evidence on these points.† I regret that, in consequence of a continuance of bad weather, I have not yet been able to obtain decisive observations as to the possible motion of the nebula in the line of sight. With spectroscope B and eye-piece 2, the lines appear to be coincident with those of hydrogen. In consequence of the uncertainty of the character of the first line, which is single, while that of nitrogen is double, this determination can now only be made by means of the comparison of the third line with that of hydrogen. This third line becomes very faint from the great loss of light unavoidable in a spectroscope that gives a sufficient dispersive power, and the comparison can only be attempted when the sky is very clear and the nebula near the meridian.

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* The fourth line was first seen in nebula 18 H. IV. (Phil. Trans 1864, p. 441).

† Phil. Trans. 1868, pp 540-546 Observations on this point were subsequently made by Frankland and Lockyer (Proc. Roy. Soc. vol xvii. p. 453). It should be stated that they make no reference to my observations, though they refer to a purely hypothetical suggestion contained in the same paper.

* Phil. Trans. 1868, pp 542, 543.

† Since writing this sentence I have seen a note by Prof. D'Arrese in the "Astronomische Nachrichten," No. 1,885. Speaking of the nebula H. IV. 37, he says:—"Sein Spectrum ist ausser von Huggins bisher nur noch von Dr. H Vogel untersucht worden. In No 1,864, Ast. Nachr theilt Letzterer mit, trotz er im Jahre 1871, im Widerspruch mit Huggins' Angabe, die Linie Neb (3) = (2), bisweilen sogar (2) < (3) gefunden haben. Auch Huggins war dagegen im Jahre 1864 positiv (2) > (3) Ist Vogel's Beobachtung, wie ich nicht bezweifle, zuverlässig, so wird seine Vermuthung einer Veranderung hier in der That begrundet sein, denn diesen Winter, namentlich im

2. On the Motions of some Stars towards or from the Earth

In the early part of 1868 I had the honour of presenting to the Royal Society some observations on a small change of refrangibility which I had observed in a line in the spectrum of Sirius as compared with a line of hydrogen, from which it appeared that the star was moving from the earth with a velocity of about twenty-five miles per second, if the probable advance of the sun in space be taken into account.[*]

It is only within the last few months that I have found myself in possession of the necessary instrumental means to resume this inquiry, and since this time the prevalence of bad weather has left but few nights sufficiently fine for these delicate observations.

Some time was occupied in obtaining a perfectly trustworthy method of comparison of the spectra of stars with those of terrestrial substances, and it was not until I had arranged the spark within the tube, as described at the beginning of this note, that I felt confidence in the results of my observations.

It may be well to state some circumstances connected with these comparisons which necessarily make the numerical estimations given further on less accurate than I could wish. Even when spectroscope C, containing four compound prisms, and a magnifying power of 16 diameters are used, the amount of the change of refrangibility to be observed appears very small. The probable error of these estimations is therefore large, as a shift corresponding to five miles per second (about $\frac{1}{40}$ of the distance of D^1 to D^2), or even a somewhat greater velocity, could not be certainly observed. The difficulty arising from the apparent smallness of the change of refrangibility is greatly increased by some other circumstances. The star's light is faint when a narrow slit is used, and the lines, except on very fine nights, cannot be steadily seen, in consequence of the movements in our atmosphere. Further, when the slit is narrow, the clock's motion is not uniform enough to keep the spectrum steadily in view; for these reasons I found it necessary to adopt the method of estimation by comparing the shift with a wire of known thickness, or with the interval between a pair of close lines. I found that, under the circumstances, the use of a micrometer would have given the appearance only of greater accuracy. I wish it therefore to be understood that I regard the following estimations as provisional, as I hope, by means of apparatus now being constructed, to be able to get more accurate determinations of the velocity of rhe motions.

Sirius.—The comparison of the line at F with the corresponding line of hydrogen was made on several nights from January 18 to March 5. Spectroscope C and eye-pieces 2 and 3 were used. These observations confirm the conclusion arrived at in my former paper, that the star is moving from the earth; but they ascribe to the star a velocity smaller than that which I then obtained.

These observations on different days show a change of refrangibility corresponding to a velocity of from twenty-six miles to thirty-six miles per second. She part of the earth's orbital motion from the star varied on these days from ten miles to fourteen miles per second. We may take, therefore, eighteen to twenty-two miles per second as due to the star.

The difference of this estimate, which is probably below rather than in excess of the true current from that which I formerly made, may be due in part or entirely to the less perfect instruments then at my command. At the same time, if Sirius be moving in an elliptic orbit, as suggested by Dr. Peters, that part of the star's proper motion, which is then in the direction of the vesical ray, would constantly vary.[*]

Betelgeux (*α Orionis*).—In the early observations of Dr. Miller and myself on this star, we found that there are no strong lines coincident with the hydrogen lines at C and F. The line H α falls on the less refrangible side of a group of strong lines, and H β occurs in the space between two groups of strong lines, where the lines are faint. On one night of unusual steadiness in the air, when the finer lines in the star's spectrum were seen with more than ordinary distinctness, I was able with the more powerful instruments now at my command to see a narrow defined line in the red apparently coincident with H α, and a similar line at the position of H β. These lines are much less intense than the lines C and F in the solar spectrum; there are certainly no bright lines in the star's spectrum at these places.

The most suitable lines in this star for comparison with terrestrial substances for ascertaining the star's motion are the lines of sodium and of magnesium. The double character of the one line agreeing exactly with that of sodium, and the further circumstance that the more refrangible of the lines is the stronger one, as is the case in spectrum of sodium and in the solar spectrum, and the relative distances from each other and comparative brightness of the three lines, which correspond precisely to the triple group of magnesium, can allow of doubt that these lines in the star are really produced by the vapours of these substances existing there, and that we may therefore safely take any small displacement of either set of lines to show a motion of the star towards or from the earth. The lines due to sodium are perhaps more intense, but are as narrow and defined as the lines D_1, D_2 in the solar spectrum; they fall, however, within a group of very fine lines; this circumstance may possibly account for the nebulous character which has been assigned to them by Father Secchi.

The bright lines of sodium were compared with spectroscope B and eye-piece 3; they appeared to fall very slightly above the pair in the star, showing that the stellar lines had been degraded by the star's motion from the earth. The amount of displacement was estimated at about one-fifth of the distance of D_1 from D_2, which is probably rather smaller than the true amount. This estimation would give a velocity of separation of thirty-seven miles per second. At the time of observation the earth was moving from the star at about fifteen miles per second, leaving twenty-two miles to be due to the star.

When magnesium was compared, a shift in the elevation, and corresponding in extent to about the same velocity of recession, was observed; but in consequence of other lines in the star at this place, the former estimation, based on the displacement of the lines of sodium, was considered to be more satisfactory.[†]

Rigel.—The lines of hydrogen are strong in the spectrum of this star, and are suitable for comparison.

The line of H β is not so broad as it appears in the speculum of Sirius, but is stronger than F in the solar spectrum: this line was compared by means of spectroscope C and eye-pieces 2 and 3. The line of terrestrial hydrogen falls above the middle of the line in the star; the star is therefore receding from the earth. The velocity of recession may be estimated as rather smaller than Sirius, probably about thirty miles per second, the earth at the time of observation moving from the star with a velocity of fifteen miles, leaving about fifteen miles as due to the star. This estimate is probably rather smaller than the true velocity of the star.

Februar und Marz 1872, fand ich wiederum, so wie es Huggins fruher gesehen hat, unzweifelhaft (2) > (3) Die relative Intensitat der drei Lichtarten habe ich mehrfach in Zahlen geschatzt und erhielt z. B. in den letzten Nachten:

	Marz 6.	Marz 13.
(1)	100	100
(2)	58	63
(3)	49	52

[*] Phil. Trans. 1868, pp. 529-550 As a curious instance in which later methods of observations have been partially anticipated, a reference may be made to an ingenious paper in the Philosophical Transactions for 1783, vol. lxxiv, by the Rev. John Mitchell, entitled "On the means of discovering the Distance, Magnitude, &c., of the Fixed Stars, in consequence of the Diminution of the Velocity of their Light." The author suggests that by the use of a prism "we might be able to discover diminutions in the velocity of light as perhaps a hundredth, a two hundredth, a five hundredth, one even thousandth part of the whole" But he then goes on to reason on the production of this diminished velocity by the attraction produced on the material particles of light by the matter of the stars, and that the diminutions stated above would be "occasioned by spheres whose diameter should be to the sun, provided they were of the same density, in the several proportions of 70, 50, 30, and to 22 to 1 respectively."

[*] H. Vogel at Bothkamp seems to have repeated my observations on Sirius with the necessary care. He says (Astron. Nachr. No 1864):—"Mit der eben beschriebenen Anordnung gelang es Herrn Dr. Lohse und mir am 22 März (1871) bei ganz vorzuglicher Luft die Nichtcoincidenz der drei Wasserstofflinien H α, H β, und H γ, der Geissler'schen Rohre mit den entsprechenden Linien des Siriusspectrums zu sehen...... mit Berucksichtigung der Geschwindigkeit der Erde zur Zeit der Beobachtung berechnet sich die Geschwindigkeit mit welcher sich Sirius von der Erde bewegt zu 10 0 Meilen in der Secunde, wogegen Procyon sich 13 8 Meilen in der Secunde von unserer Erde entfernen wurde."

[†] I had the pleasure on one evening of showing the displacement of the lines in Sirius and α Orionis to Mr Christie, First Assistant at the Greenwich Observatory.

Castor.—The spectra of the two component stars of this double star blend in the spectroscope into one spectrum. The line H β is rather broad, nearly as much so as the same line in the spectrum of Sirius.

The narrow line of rarefied hydrogen was compared in spectroscope B with eye-piece 3; it appeared to fall on the more refrangible side of the middle of the line in the star, leaving more of the dark line on the side towards the red. The shift seemed to be rather greater than that in Sirius, and may probably be taken at from 40 to 45 miles per second; but the earth's orbital motion was nearly 17 from the star, thus leaving about 25 miles for the apparent velocity of the star. This result rests at present on observations on one night only, but they seemed at the time to be satisfactory.

Regulus.—The line at F rather broad. The corresponding line of hydrogen falls on the more refrangible side of the middle of the dark line in the star. The air was unfavourable on all the evenings of comparison; a rough estimate gives a velocity of from 12 to 17 miles for the velocity of recession between the star and the sun.

β *and* δ *Leonis.*—These stars were compared with hydrogen; they appear to be moving from the earth, but the want of steadiness in the air prevented me from making a satisfactory estimate of their velocity. I suspected their motion to be smaller than that of Regulus.

β, γ, δ, ϵ, ζ *Ursæ Majoris.*—All these stars have similar spectra, in which the line F is strong, though there are small differences in the breadth of the line. They were compared with hydrogen, and appear to be moving from our system with about the same velocity. Probably their motion may be taken to be not far from 30 miles per second. The earth's motion at the time of observation was from 9 miles to 13 miles for these stars, leaving a probable velocity of recession of 17 to 20 miles per second. In the case of the double star ζ, the spectrum consisted of the light of both stars.

η *Ursæ Majoris* was also compared with hydrogen. I believe it shows a motion from the earth, but the observations of this star are at present less satisfactory.

α *Virginis and* α *Coronæ Borealis*—These stars were compared with hydrogen. I suspect that they are receding, but I have not had nights sufficiently fine to enable me to make satisfactory observations of these stars.

In addition to these stars some observations (which are less satisfactory on account of the unfavourable state of the weather at the time) appear to show that the stars Procyon, Capella, and possibly Aldebaran, are moving from the earth.

The stars which follow have a motion of approach.

Arcturus.—In the spectrum of this star the lines of hydrogen, of magnesium, and of sodium are sufficiently distinct for comparison. I found the comparison could be most satisfactorily made with magnesium.

The bright lines of magnesium fall on the less refrangible side of the corresponding dark lines in the star's spectrum, showing that the star is approaching the earth. I estimated the shift at about $\frac{1}{6}$ to $\frac{1}{4}$ of the interval between Mg_2 and Mg_3; this amount of displacement would indicate a velocity of approach of 50 miles per second. To this velocity must be added the earth's orbital motion from the star of 5·25 miles per second, increasing the star's motion to 55 miles per second.

When I can get favourable weather, I hope to obtain independent estimations from the lines of sodium and of hydrogen.

α *Lyræ.*—In the spectrum of Vega the line corresponding to H β is strong and broad. Comparisons were made on several nights, but on one evening only was the air favourable. The observations are accordant in showing that the narrow bright line from a Geissler's tube falls on the less refrangible side of the middle of the line in the star, thus leaving more of the line on the side towards the violet. The estimations give a motion of approach between the earth and the star of from 40 to 50 miles per second, to which must be added 3·9 miles after the earth's motion from the star.

α *Cygni.*—The hydrogen line at F in the spectrum of this star is narrower than in the spectrum of Sirius and of α Lyræ, though probably rather broader than the same line in the solar spectrum. I have at present observations made on two evenings only, on both of which the state of the air was unfavourable, of the comparison of this line with that of terrestrial hydrogen. They give to the star a motion of approach of about 30 miles per second, which would have to be increased by 9 miles, the velocity at the time of the earth from the star.

Pollux.—The lines of magnesium and those of sodium are very distinct in the spectrum of this star. As the air was not very steady at the time of my observations, I found it more satisfactory to use for comparison the lines of magnesium, which are rather stronger than those of sodium. The three lines of magnesium appeared to be less refrangible than the corresponding dark lines in the spectrum of the star by about one-sixth of the interval from Mg_2 to Mg_3. This estimation would represent a velocity of approach equal to about 32 miles per second. The earth's motion from the star was 17·5 miles, which increases the apparent velocity of approach to 49 miles per second. On one evening only was the air favourable enough for a numerical estimate, but the observations were entered in my observatory-book as very satisfactory.

α *Ursæ Majoris.*—The spectrum of this star is very different from the spectra of the other bright stars of this constellation. The line at F is not so strong, while the lines at *b* are more distinct, and are sufficiently strong for comparison with the bright lines of magnesium. The bright lines of this metal fall on the less refrangible side of the dark lines, and show a motion of approach of from 35 to 50 miles per second. The earth's motion of 11·8 miles from the star must be allowed for.

γ *Leonis and* ϵ *Bootis.*—In both these double stars the compared spectrum due to the light of both important stars were observed. Both stars are most conveniently compared with magnesium. I do not consider my observations of these stars as quite satisfactory, but they seem to show a movement of approach; but further observations are desirable.

The stars γ *Cygni*, α *Pegasi*, γ *Pegasi*, and α *Andromedæ* were compared with hydrogen on one night only. It is probable that these stars are approaching the earth, but I wish to re-observe them before any numerical estimate is given of their motion.

γ *Cassiopeiæ*—On two nights I compared the bright lines which are present in its spectrum at C and F with the bright lines of terrestrial hydrogen. The coincidence appeared nearly perfect in spectroscope C with eye-pieces 2 and 3; but on the night of least definition I suspected a minute displacement of the bright line towards the red when compared with H β. As the earth's orbital motion from the star at the time was very small, about 3·25 miles per second, which corresponds to a shift that could not be detected in the spectroscope, it seems probable that γ Cassiopeiæ has a small motion of recession.

In the calculation of the estimated velocities the wave-lengths employed are those given by Angstrom in his "Recherches sur le spectre solaire," Upsal, 1868. The velocity of light was taken at 185,000 miles per second.

The velocities of approach and of recession which have been assigned to the stars in this paper represent the whole of the motion in the line of sight which exists between them and the sun. As we know that the sun is moving in space, a certain part of these observed velocities must be due to the solar motion. I have not attempted to make this correction, because, though the direction of the sun's motion seems to be satisfactorily ascertained, any estimate that can be made at present of the actual velocity with which he is advancing must rest upon suppositions, more or less arbitrary, of the average distance of stars of different magnitudes. It seems not improbable that this part of the stars' motions may be larger than would result from Otto Struve's calculations, which give, on the supposition that the average parallax of a star of the first magnitude is equal to 0″·209, a velocity but little greater than one-fourth of the earth's annual motion in its orbit.

It will be observed that, speaking generally, the stars which the spectroscope shows to be moving from the earth (Sirius, Betelgeux, Rigel, Procyon) are situated in a part of the heavens opposite to Hercules, towards which the sun is advancing; while the stars in the neighbourhood of this region, as Arcturus, Vega, α Cygni, show a motion of approach. There are in the stars already observed exceptions to this general statement; and there are some other considerations which appear to show that the sun's motion in space is not the only, or even in all cases, as it may be found, the chief cause of the observed proper motions of the stars.

There can be little doubt but that in the observed stellar movements we have to do with two other independent motions, namely, a movement common to certain groups of stars, and also a motion peculiar to each star.

Mr. Proctor has brought to light strong evidence in favour of the drift of stars in groups having a community of motion, by his graphical investigation of the proper motions of all the stars in the catalogues of Mr. Main and Mr. Stone.[*] The probability of the stars being collected into such systems was early suggested by Mitchell and the elder Herschel.[†] One of the most remarkable instances pointed out by Mr. Proctor are the stars β, γ, δ, ϵ of the Great Bear, which have a community of proper motions [‡] while α and η of the same constellation have a proper motion in the opposite direction. Now, the spectroscopic observations show that the stars β, γ, δ, ϵ, ζ have also a common motion of recession while the star α is approaching the earth. The star η indeed appears to be moving from us, but it is too far from α to be regarded as a companion to that star.

TABLE I.—Stars moving from Sun

Star.	Compared with	Apparent motion	Earth's motion.	Motion from sun.
Sirius . . .	H	26 to 36	—10 to 14	18 to 22
Betelgeux . .	Na	37	—15	22
Rigel, . . .	H	30	—15	15
Castor . . .	H	40 to 45	—17	23 to 28
Regulus. . .	H	30 to 35	—18	12 to 17
β Ursæ Maj .	H	30	— 9 to 13	17 to 21
γ ,, ,, .	H			
δ ,, ,, .	H			
ϵ ,, ,, .	H			
ζ ,, ,, .	H			
β Leonis . .	H			
δ Leonis . .	H			
η Ursæ Maj. .	H			
α Virginis . .	H			
α Coronæ B. .	H			
Procyon . .	H			
Capella . . .	H			
Aldebaran ? .	Mg			
γ Cassiopeiæ .	H			

TABLE II.—Stars approaching the Sun

Star.	Compared with	Apparent motion.	Earth's motion.	Motion towards sun.
Arcturus . .	Mg	50	+ 5	55
Vega . . .	H	40 to 50	+39	44 to 54
α Cygni. . .	H	30	+ 9	39
Pollux . . .	Mg	32	+17	49
α Ursæ Maj. .	Mg	35 to 50	+11	46 to 60
γ Leonis . .	Mg			
ϵ Bootis. . .	Mg			
γ Cygni. . .	H			
α Pegasi . .	H			
γ Pegasi ? . .	H			
α Andromedæ.	H			

Although it was not to be expected that a concurrence would always be found between the proper motions which indicate the apparent motions at right angles to the line of sight and the radial motions as discovered by the spectroscope, still it is interesting to remark that in the case of the stars Castor and Pollux, one of which is approaching and the other receding, their proper motions also are different in direction and in amount; and further, that γ Leonis, which has an opposite radial motion to α and β of the same constellation, differs from these stars in the direction of its proper motion.

It scarcely needs remark that the difference in breadth of the line H β in different stars affords us information of the difference of density of the gas by which the lines of absorption are produced. A discussion of the observations in reference to this point, and to other considerations on the physical condition of the stars and nebulæ, I prefer to reserve for the present.

EXCURSION OF THE GEOLOGISTS' ASSOCIATION TO GUILDFORD AND CHILWORTH, JUNE 1

THE party first proceeded to examine the section of the "Woolwich and Reading Beds," just north of the station. This section was described by Mr. Prestwich in 1850 (see Quarterly Journal Geological Society, vol. vi. p. 260, fig. 6) not long after it had been exposed by the railway-cutting. A year ago it was laid bare afresh when widening the railroad; but already the slipping of the clays has obliterated some points of interest. Traces of the shell beds, with *Cyrena* and *Ostrea*, below the representatives of the "Oldhaven beds," are to be found at the base of a telegraph post, 104 yards south of the road bridge; and the underlying mottled clays, with a dip of 4° to the north, are easily recognised for about 190 yards to the south, where a small valley (about 50 yards across) has been formed by denudation out of the sand and lowest green sandy clays resting on the Chalk, which forms the northern foot of the Hogsback or Surrey range. Here the Chalk is seen to be traversed in every direction with fissures, often "slickensided," horizontally or nearly so, some empty, some filled with vein flint, and some with loamy stuff. Nodules and occasional thin laminæ of flint follow the dip of about 6° to the north, and many are in a crushed condition. Bands of marly chalk also lie on the same plane. Some Echinoderms were met with. The party then proceeded to visit the much larger excavation in the Chalk at the entrance of the railway tunnel. Here the dip, well marked by flints and marly bands, is about 12° to the north. Fossils (Sponges, Echinoderms, Inoceramus, &c.) abound in this pit. The usual chalcedonic and quartzy interiors of hollow flints attracted notice, and Prof. R. Jones drew attention to facts that seemed to him to bear evidence of flint being a pseudomorph after chalk. They next visited a quarry in the Lower Greensand, on the escarpment overlooking the pathway to Losely. In this section of those Neocomian beds known as the Bargate-Stone, the waterworn sand of quartz, ironstone, lydite, and hard green silicates, is so largely mixed with calcareous fragments (the *débris* of shell beds, polyzoan reefs, &c.) that it is here and there cemented together hard and compact enough to serve as a building stone and road-metal. Mr. Meyer here directed attention to the horizon at which he obtained an unrolled tooth of *Iguanodon*, indicating the existence of this great Dinosaur at, perhaps, the latest period to which any of its remains as yet known belong. The "false-bedding" of the sands—due to the southward set of prevalent tides and currents, and the probable origin of their materials from the "old palæozoic ridge or shoal," were also studied, and the formation of the escarpment, with the correlative parallel cracks and fissures of the strata. The party then crossed the Ferry, where St. Catherine's Spring issues, beneath the hill, from a little cave in the red-orange-tinted sand. Here for thirty feet at least the Guildford gap has been found by boring to be occupied by bouldered chalk and other detritus due to the destructive, and yet conservative, agencies of nature. The soft irony beds of the Lower Greensand were next met with, and followed followed for about a mile, until a short field-lane, crossing the Gault and Upper Greensand, led into the Chalk-marl quarry below Warren Farm. Here the loss of the clay beds (Gault) from below, by their having been squeezed out along the southern side, had allowed the hard marl-rock to subside inwards and suddenly at the escarpment, and to rest at high angle (70° and more), whilst the Chalk of the hill range above dips only 5° or 6°. As the hard rock bands, here quarried for lime, are followed end-on along the strike (open to-day), the backs of lower beds form one side of this deep narrow pit; and the truncated edges of these somewhat bent and much fissured strata warn the instructed eye of the danger of standing either below them or above them, lest either rain or drought should detach their clinging surfaces from the sloping bed-plane. Large Ammonites and Nautili are the chief fossils met with here; but *Pecten Beaveri* and *Terebratula* are also found. In an old excavation in the lane *Siphonia* has been found in the representative of the Upper Greensand which is overlain by dark-green sandy clay and Gault, turned up at a high angle (and probably squeezed out) in the breadth of a few yards, before the iron-sands are reached on returning to the hill-side. The party next came to the foot of St. Martha's Hill, or Martyrs' Hill. Before mounting this hill of sand, seamed irregularly with ironstone, some of the geologists descended the Halfpenny Hatch lane, leading down towards the East Shalford bottom, and saw a section of sand and calcareous sandstone, with a fuller's earth band and pebbly beds, similar to those in the quarry on the other (western) side of Guildford. The underground structure of South-Eastern England is connected with that of the Boulonnais, of Belgium, the Ardennes, and Westphalia; and the folds and ridges of palæozoic rocks, that in those countries bear up, either at the surface or just beneath the Chalk, or the attenuated Oolites, valuable coal-beds, are continued through, in a broad sweeping line, underneath parts of Surrey, Kent, and Sussex, until visible again near Frome, in the Bristol coal-area, in North Devon, South Wales, and the South of Ireland. The old faults and fissures affecting this linear tract of old strata had long before the Coal-period raised and depressed the lands and sea-beds; and, as a great spur of the old Scandinavian lands, this tract afforded ground for the littoral growth of the jungles that formed the coal on its oscillating borders and in its lagoons, now shut up by bars, and now losing their marsh features by influx of the sea. Succeeding ages still brought oscillations and changes, until the Jurassic seas crept over this old ridge or shoal, and the Cretaceous seas quite buried it, at first in sands and ultimately by the calcareous ooze of oceanic depths. But again another contracting crush of the earth's crust operated on the old weak lines, and the buried ridge slowly uprose, and its coating of thick strata were worn off by sea and rain, making pebbles and sand for the Lower Tertiaries; and still rising, it was at length laid bare in the Franco-Belgian and the Bristol areas; whilst our Wealden valleys of elevation, and those of Kingsclere, Shalbourn, and Pewsey, show where its uneven back approaches near the soil.

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[*] See "Preliminary Paper on certain Drifting Motions of Stars," Proc Roy Soc vol. xviii, p 169.

[†] Sir William Herschel writes:—"Mr. Mitchell's admirable idea of the stars being collected into systems appears to be extremely well founded, and is every day more confirmed by observations, though this does not take away the probability of many stars being still as it were solitary, or, if I may use the expression, intersystematical. . . A star, or sun such as ours, may have a proper motion within its own system of stars; while at the same time the whole starry system to which it belongs may have another proper motion totally different in quantity and direction" Herschel further says, "and should there be found in any particular part of the heavens a concurrence of proper motions of quite a different direction, we shall then begin to form some conjectures which stars may possibly belong to ours, and which to other systems."—Phil Trans 1783, pp. 276, 277.

[‡] Mr. Proctor, speaking of these stars, says —"Their drift is, I think, most significant. If, in truth, the parallelism and equality of motion are to be regarded as accidental, the coincidence is one of most remarkable character. But such an interpretation can hardly be looked upon as admissible when we remember that the peculiarity is only one of a series of instances, some of which are scarcely less striking."—"Other Worlds than Ours," p. 269, and paper in Proc. Roy. Soc. vol. xviii. p. 170.

SOCIETIES AND ACADEMIES

London

Royal Society, June 20.—"On the 26-day Period of the Earth's Magnetic Force," by Mr. J. A. Broun, F.R.S.

Referring to the Astronomer Royal's important communication on this subject, the author confesses that, projecting his results for the horizontal force, he cannot agree in his final conclusions from them. In his paper he limits himself wholly to the observations of the horizontal force, as he has found that element, when accurately corrected for temperature, best fitted to show the period in question.

As far as the existence of a period of near 26 days is concerned, he thinks there cannot be the slightest doubt; the examination of great masses of observations has confirmed his belief; but we know nothing certainly as to its cause. It appears to be most probably connected with the sun's rotation; but in what way this may act nothing is known. The single periods show great breaks, and what may be termed *accidental* minima, in opposition to the minima belonging to the period; these accidental are connected with great disturbance, probably allied to the solar eruptions, or to causes which generally produce spots and protuberances. We might suppose that the sun during its rotation produces an action on the magnetic or electric ether in motion, which, as far as it acts on our magnet, may be supposed in greater quantity or more condensed in certain parts of the earth's orbit, and in certain years; and, as has been supposed in the case of the frequency of the solar spots, this ether may also be acted on by the planet, and produce an irregularity in the length of a few successive periods. These suppositions are made merely to show that we are perhaps not in possession of all the conditions of the problem, without which perfect exactness in the calculations is impossible.

In conclusion, he refers those interested in the subject to plate xxvii. in the Transactions of the Royal Society of Edinburgh, vol. xxii. where the daily means of horizontal force are projected for four stations on the earth's surface, all of which agree in showing the same movements, some of which have an amplitude of ·002 of the whole horizontal force (the Astronomer Royal's result for 1870 gives a *mean* value of nearly the half of this), and with intervals of about 26 days.

Paris

Academy of Sciences, July 1.—M. M. Marie read a memoir on some general properties of the imaginary envelope of the conjugates of a plane place.—M. H. Resal communicated general equations of the movements of a solid body referred to its movable axes; and M. Montucci forwarded a note describing an experiment for the appreciation of the resistance of a sheet of brass to atmospheric pressure.—M. J. Bourget presented a memoir on the mathematical theory of the movement of a cord, one of the extremities of which possesses a given movement.—M. G. Tissandier communicated a notice of an optical phenomenon observed during a balloon ascent, describing a case in which the shadow of the balloon was thrown distinctly upon a white cloud, and surrounded by a pale elliptical halo, exhibiting the colours of the rainbow.—M. Faye communicated a letter from M. Tacchini noticing the occurrence of magnesium in the chromosphere of the sun.—M. J. A. Brown presented a note on the simultaneity of barometric variations between the tropics.—General Morin communicated an extract from a letter by M. Vinson describing a severe cyclone which followed the aurora australis of Feb. 4, 1872, at Reunion.—M. W. de Fonvielle gave an account of observations made during the ascents of the balloon "Lea," in which he refers to the above-mentioned note by M. Tissandier, giving the credit of the first observation of the halo round the shadow of balloons to Mr. Glaisher, and especially to the oscillation and rotation of balloons.—M. L. Sollier forwarded a note on the destruction of *Phylloxera vastatrix* by means of a decoction of tobacco.—M. C. Bernard presented a fourth note by M. Paul Bert on the influence exerted by changes of barometric pressure upon the phenomena of life; and M. Wurtz communicated a third note, by M. Oré, on the question whether strychnine is to be regarded as an antidote to chloral.—M. Decaisne communicated an interesting paper by MM. Van Tieghem and Le Monnier, "On the Polymorphism of the Reproductive Organs in the mucorine genus *Mortierella*."—M. Leymerie presented a brief reply to a note by M. Garrigou on the constitution of the Pyrenees.

BOOKS RECEIVED

ENGLISH.—Town Geology: Rev. C. Kingsley (Strahan and Co.).—The Life of Richard Trevithick, vol. i.: F. Trevithick (E. and F. Spon).—Health and Comfort in House Building: J. Drysdale and J. W. Hayward (E. and F. Spon).—Nautical Surveying: J. K. Laughton (Longmans).—Sewer Gas, and how to keep it out of Houses: O. Reynolds (Macmillan).

FOREIGN.—Zeitschrift für Biologie: Pettenkofer, Radlkofer, and Vogt, Band 7, Heft 3, 4, Band 8, Heft 1.—Abhandlungen des Naturwissenschaftlichen Vereins zu Bremen Band 3, Heft 1.—Die Echinoiden der oesterreichisch-hungarischen oberen Tertiarablagerungen: Dr. Laube.—Die Erforschung des Süden-polar Gebietes: Dr. G. Neumayer.—Zur Kenntniss der Chlorophyllfarbestoffe: Dr. G. Kraus.—Jahrbuch der k. k. geologischen Reichsanstalt zu Wien, Jan.-March.—Notizblatt des Vereins fur Erdkunde: L. Ewald.—Zur Morphologie des Saugethier-Schadels: Dr. J. C. G. Lucae.

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ERRATA.—Vol. v., p. 167, col. 8, l. 20, for "sufficient from" read "sufficient heat from;" p. 168, col. 1, l. 11, for "inorganic" read "organic."

THURSDAY, JULY 25, 1872

THE LAST ATTACK ON DARWINISM*

THIS volume, which in bulk, general appearance, and typography bears a close resemblance to the earlier editions of the "Origin of Species," seems got up to stand by its side on the bookshelf, an ever ready antidote to the pernicious doctrines of Mr. Darwin and his supporters. After a careful perusal we must confess that it may seriously damage Mr. Darwin's reputation with those who have never read his works; but we are quite sure that no one who has studied the "Origin of Species," and been convinced of the general accuracy of its statements and conclusions, will have their convictions at all shaken by Dr. Bree's argument. As, however, it is just the work to be read by those who have only a second-hand knowledge of Mr. Darwin's works, we feel it to be a duty to call attention to the very careless manner in which the book is written,—its numerous errors, misrepresentations, and misconceptions, and its extensive use of declamation and opinion as sufficient answers to Mr. Darwin's elaborate observations, carefully selected facts, and cautious inductions.

In a work of purely adverse criticism, the first duty of an author is to quote his opponent's words with scrupulous accuracy. Yet, in the very first page of his book, Dr. Bree misquotes Dr. Hooker; at p. 3 and again at p. 9 he repeats this misquotation; and he devotes eight pages to proving that what Dr. Hooker did not say is erroneous. The quotation is from the Presidential Address at Norwich. The words actually used, and to be found in the authoritative report, are:—"So far from Natural Selection being a thing of the past, it is an accepted doctrine with *almost* every philosophical naturalist; including, it will always be understood, a considerable proportion who are not prepared to assent that it accounts for all that Mr. Darwin assigns to it." Dr. Bree omits the word *almost*, and then sets himself to convict Dr. Hooker of misrepresentation, by showing that with *some* "philosophical naturalists" it is not an accepted doctrine.

On p. 2 Dr. Bree makes a misstatement, almost equally glaring, of another author's view. He says, "And Mr. St. George Mivart has proved, and I think incontestably, that it (Natural Selection) *has not a basis of truth;*" and refers the reader to "Genesis of Species," 1871. But in this volume we find (at p. 5) the author's statement, that the object of his book is "to maintain the position that 'Natural Selection' *acts, and, indeed, must act*, but that still, in order that we may be able to account for the production of known kinds of animals and plants, it requires to be supplemented by the action of some other natural law or laws yet to be discovered."

A little further on Dr. Bree discusses Herbert Spencer's "First Principles;" and how far he is likely to elucidate that philosopher's views may be seen by the following curious blunder. At p. 48 he tries to explain to his readers what Spencer means by "the integration of matter," and quotes the following passage from his "First Principles:"—

> "Every mass, from a grain of sand to a planet, radiates heat to other masses, and absorbs heat radiated by other masses; and in so far as it does the one it becomes integrated, while in so far as it does the other it becomes disintegrated."

Dr. Bree appears to have been afraid that his readers would hardly be of sufficient mental calibre to comprehend this passage. He therefore elucidates it as follows: "Integration of matter, therefore, is the absorption of heat, and heat, we are told by Tyndall, endorsed by Spencer, is 'tremulous motion'—therefore, integration of matter is the absorption of motion." We think Dr. Bree has hardly done justice to his readers by merely turning Spencer's statement topsy-turvy, and showing them that a "good rule will work both ways;" he should further have illustrated the subject by what that philosopher terms a concrete example, and explained that, in his view, water is *integrated*, when, by absorbing heat, it changes into steam, and *disintegrated* when by radiating heat it becomes solid ice!

If the supposed fallacies of such men as Hooker and Spencer, who, in Dr. Bree's opinion, are mere satellites of Darwin, are thus ruthlessly exposed, we can hardly expect the chief conspirator himself to receive much mercy. In his "Animals and Plants under Domestication," vol. ii. pp. 250-255, Mr. Darwin carefully discusses the various views as to the causes of variability, and arrives at the conclusion that variability is *not* an ultimate fact necessarily contingent on reproduction (p. 253), and that variability of every kind is directly or indirectly caused by changed conditions of life (p. 255). Dr. Bree, however, referring to the same chapter of the same work, gives his view of the writer's meaning in the following passage:—"But Mr. Darwin goes further. He says there is an inherent tendency in the constitution of the organism to vary, independent of, but modified by, its conditions." At p. 191 Dr. Bree states, as if on Mr. Darwin's authority, "that tortoise-shell cats are so coloured as a rule only in the males;" and at p. 192, that Mr. Darwin "does not believe" exactly what Mr. Darwin says he does believe. But these are only errors of the pen in the haste of argumentative composition; a less excusable mistake is made at p. 212, where, after quoting a passage from Mr. Darwin about mimicry, Dr. Bree says:—"This passage implies that an insect can imitate the organisation of another insect, by means of a knowledge that such organisation is safer from enemies than that in which nature had clothed it. A more unsound, unphilosophical, unproved, reckless statement is not to be found, &c. &c. . . . It is only just to say that the above theory did not originate with Mr. Darwin. It is the *sole production* of the fertile brain of *Mr. Wallace*." Here we have a misrepresentation and a misstatement. No expression of Mr. Darwin or myself can be taken to mean that we believed in a voluntary knowing imitation of the organisation of one insect by another. In my article on "Mimicry" I have expressly disclaimed this view. As to the latter part of the quotation, the first words of Mr. Darwin's paragraph headed "Mimicry," and which Dr. Bree must have had before his eyes, are:—"This principle was first made clear in an admirable paper by *Mr. Bates!*" A little farther on, my theory of birds' nests and the colour of female birds is noticed with strong disapproval; and a crushing array of facts is adduced as being opposed to my statement that "when both sexes are coloured in a strikingly conspicuous manner the nest is of such a nature as to conceal the sitting bird." The whitethroat, thrush, snipe, skylark, and hedge-sparrow are adduced as opposed to my views; but as they must all be coloured in a *strikingly conspicuous* manner if they are to be of any use to Dr. Bree or his hypothetical schoolboy, the reason why they are cited remains a mystery to me. Two pages farther on we have more misquotations or blunders. At p. 229 we are told that Nitzsch's "feather tracts" are those parts of the body which have the skin uncovered! while at p. 230 we find that it is the brilliant rays *absorbed* by feathers that produce the vivid, varied, and beautiful colouring of birds! At p. 259 it is stated that "inconceivably minute changes" are alone utilised by natural selection—a misrepresentation which no word of mine or Mr. Darwin's will justify. At p. 261 we have this passage:—"Mr. Wallace adopts Mr. Darwin's view, that there is no such thing as instinct at all, in the sense in which we understand the word. He considers it the 'result of small contingent consequences, as produced by natural selection.'" As the "he" in this sentence appears to refer to Mr. Wallace, and the last ten words are given as a quotation, I felt rather ashamed of myself for writing what I could not the least understand. But a careful examination of my paper shows me that I have said nothing about the "result of contingent consequences;" neither can I find anything of the kind in Mr. Darwin's writings on "instinct." We must pass it over, therefore, as one of the ingenious paraphrases by which Dr. Bree endeavours to elucidate a difficult subject.

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* "An Exposition of Fallacies in the Hypothesis of Mr Darwin." By C R Bree, M.D., F Z S, Senior Physician to the Essex and Colchester Hospital (London: Longmans, Green, and Co, 1872.)

In a large folded frontispiece we have "The Descent of Man, after Darwin's Theory," and this is explained at p. 325; but here, too, Mr. Darwin has not been read aright, for "man's ancient ancestor, with cocked ears, tail, prehensile feet, both sexes bearded and hirsute, males with great canine teeth," is placed between Marsupials and Lemurs, whereas Darwin places it after the origin of the catarrhine monkeys, in fact, at Fig. 15 of Dr. Bree's diagram. Our author makes a great point of this, and says:—"From such a Darwinian creation were descended the lowest of the quadrumana, the lemurs."

At p. 331 we have another of our author's enigmatical sentences:—"If an optician makes an object-glass, he does so in reference to the objective, the lens." I had previously imagined that the objective *was* the object-glass; but at p. 351 I was still more puzzled by reference to the "final law of the pendulum" and the "final law of the screw"—two things I had never before heard of.

We think we have now shown that this book contains so large a number of errors, misrepresentations, and misconceptions as to render it quite untrustworthy. We proceed to give a few instances of the author's copious use of declamation, assertion, and opinion, of doubtful facts and illogical arguments.

Of declamation and assertion we have an abundance, the following being a favourable specimen:—"The system of Darwin is eminently illogical, and must fall. It is an hypothesis which draws large but unsound deductions from the rare and abnormal deviations, leaving the real field untouched and unexplored. It is founded upon the exceptions, not the rules of nature. It is utterly opposed to design, to the teachings of animal mechanics, to the grand and beautiful and everlasting proofs upon which the teleologist loves to dwell. It is a cold, unsound, unphilosophic, degrading system of assumed probabilities, which, if true, would be ten times more wonderful than anything assumed or believed by the most strict and rigid disciple of special creation. Nay, still further, if proved in every point to be true, it would still leave the fact of special creation in all its wonderful mystery. The organic cannot be formed from the inorganic; nor could the organic, if it were so formed, be endowed by any physical force with the laws and properties of life. Go on still in speculation, and I ask, Whence the inorganic—its beginning, its ending, its grand and inexplicable laws, which the physicist in vain attempts to correlate with the vital? Whence gravitation, and what the sidereal system and its movements? the Spirit that breathes through illimitable space, and lives through an eternity of time?"

A large portion of the volume is occupied with quotations from Agassiz, Haughton, Flourens, Owen, and other opponents of Darwinism; and Dr. Bree complains that these authors have hardly been noticed and not replied to by Darwin or his supporters. But the reason of this is explained in the pages of the present work (where we may suppose their best passages are quoted), by the almost entire absence of argument directed to the essential points of Mr. Darwin's theories, and the immense preponderance of loose assertions, in support of which no evidence is given. Thus, Agassiz asserts that "the differences" among domestic animals "are not of the same kind as the differences we observe among wild animals;" that "the differences we observe among wild animals are the result of a *creative power;*" that "domestication *never* produces forms which are self-perpetuating;" that "*at all times* the world has been inhabited by as great a diversity of animals as exists now;" and other similar assertions, almost all of which are controverted by accumulated facts in the works of Mr. Darwin. Chapter xxviii. is entirely devoted to an account of Agassiz's views of design, and supposed *proofs* of a creative mind at work in every step of the development of a group of animals. The facts will appear to most naturalists thoroughly consistent with the theory of evolution and that of natural selection; while the arguments involve a view of the Deity which most philosophical thinkers will find it hard to accept. Agassiz compares the Creator to an engineer, and speaks of Him as "*constantly* and *thoughtfully working* among the *complicated structures* that He has made." This idea is not that of an *omniscient* Creator, but of some inferior being with an intellect vastly superior to man's, but yet limited. "*Thoughtfully working*" implies effort to understand and overcome difficulties; whereas *thought* at all, as we think, is utterly opposed to the conception of omniscience.

Another chapter is devoted to Prof. Haughton's theory of "Least Action in Nature;" and here, again, all the established facts are perfectly consistent with, and almost necessary deductions from, evolution and natural selection. But it is the mere wide general assertions which Dr. Bree quotes with greatest approval as destructive of Darwinism. Thus: "There is no evidence in nature of birds with imperfect wings; no proof of a succession of blunders before perfection was attained. All is perfect, and all was always perfect." And again: "In every arrangement of bones, muscles, joints, and parts of animals, the motion *must* be such as it would be on the hypothesis that the muscles were a living, intelligent thing, trying to save itself trouble." This last may be true, but it is certainly not necessarily true; and as to imperfect wings, what are those of the Cassowary and Apteryx, which have no known function whatever?

The article of M. Flourens against Darwin is given in an appendix, and his facts as to the crossing of quadrupeds are said to be absolutely fatal to the whole theory of natural selection. But these facts are of a very imperfect and scanty character, and are almost wholly negative; and they are fully noticed in Mr. Darwin's elaborate discussion of the difficult question of hybridity, although Dr. Bree assures his readers that these facts were "never contradicted or *even noticed by Mr. Darwin!*" Under the heading "Flourens," in the index to "Animals and Plants under Domestication," are four references, and the works, "Longévité Humaine" and "De l'Instinct," are referred to; while Dr. Bree himself seems to be unaware of the existence of anything but the "Criticism on Darwin," which has been long ago most admirably answered by Prof. Huxley.

We will now give a few examples of the facts and arguments adduced by Dr. Bree himself. At p. 90, he tells us that Mr. Darwin "has given figures of different sized skulls and jaw-bones, scapulæ and clavicles (of pigeons), differing just as much from each other as the same bones in different sized Englishmen would do; and nothing more!" And on the next page he assures us that a Colchester pigeon-fancier told him, that if he allowed his short-beaked tumblers to fly out of doors they would revert to a state of nature, and that, *in a few weeks*, the beautiful small beaks would be as long and as coarse as those of any other bird! On which Dr. Bree triumphantly remarks—"Of course they would." At p. 131 he tells us, that although young song birds will learn other birds' notes with which they may be associated, yet *if kept quite alone* they will sing their own natural song, "*as several who have tried the experiment assure me.*" This is directly opposed to the experiments on this very point of Daines Barrington, quoted by me in "Contributions to the Theory of Natural Selection," 2nd Ed. p. 221, and it would therefore have been a valuable contribution to our knowledge of this difficult subject if the experiments alluded to had been given in detail, not vaguely referred to. At p. 143 it is stated that the bees' cell "is one of the finest examples in nature of what is termed the principle of 'least action;' that is to say, the greatest amount of space is gained by the least amount of material." This is certainly not true, for the cell being suspended from *the top* and *equally thick throughout*, must be too strong at bottom if strong enough at top. There is therefore waste of material. This objection was published nine years ago, in the "Annals of Natural History" for October 1863, and it has never been answered.

On the imperfection of the geological record Dr. Bree is very strong. He says that Mr. Darwin "asks us to imagine that an ape-like man became evolved in the lower tertiaries, the remains of which or of his descendants have never been discovered. Such a demand upon the credulity of mankind was never, I believe, before seriously made, unless we were told that geese were transmuted barnacles" (p. 180). This is, of course, a sufficient answer to Sir Charles Lyell's careful investigation of the subject, and especially to his most suggestive table of old fossil mammals, given in the twentieth chapter of his "Elements of Geology."

Mr. Mivart and Prof. Owen are both applauded so far as they oppose Darwin, but as both of them believe in some form of development, they are, in Dr. Bree's opinion, almost equally involved in error. Mr. Mivart's doctrine of evolution, he thinks, cannot stand, and "looks too much like Mr. Tegetmeir's pigeons, made to order." It is, however, no doubt offered with the best intentions, "as a means of reconciling scientific and religious thought,"—"two lines which, Mr. Spencer remarks, are running parallel *and* gradually approaching each other!" (We doubt the accuracy of this quotation from Mr. Spencer, but we are near the end of the book and have learnt not to expect accuracy.) Prof. Owen has, in Dr. Bree's humble opinion, "surrendered the outposts of our defence to the believers in the Darwinian hypothesis." As to Sir Charles Lyell, the charm of his works is gone for Dr. Bree, and he reflects with melancholy what the future will think of the great geologist's transmutation of thought, and with regret that such a man could, "in the maturity of his age and fame, have forsaken the 'principles' of his youth, of his manhood, and of his prime." The researches of M. Gaudry in Greece are of no use whatever; for the various forms of elephant, rhinoceros, horse, and pig, which he and Sir Charles Lyell believe to be intermediate forms, differ no more from one another than do English from Americans, and only prove a "slight variation!"

These are the kind of observations, this the kind of reasoning, by which Dr. Bree thinks to stem the tide of belief in Darwinism. At p. 269, Prof. Huxley is severely criticised for having written the following passage: "The mixture of ignorance and insolence which *at first* characterised a large proportion of the attacks with which Mr. Darwin was assailed, is no longer the sad distinction of anti-Darwinian criticism." This, Dr. Bree, with his usual curious logic, asserts is manifestly untrue, *because* some of the highest men in science, such as Agassiz, Flourens, Owen, Haughton, &c., oppose Darwinism. Why then did Dr. Bree not let well alone—leave the battle in the hands of these redoubted champions, and not give Prof. Huxley the opportunity of retracting his statement, on the ground that although the *insolence* of the first opponents of Darwinism may have vanished, their *ignorance* has returned?

In conclusion, I must again repeat that the only reason for devoting so much space to a book so little worthy of its title or its author, is the wish to warn such as are not well acquainted with Mr. Darwin's works from implicitly relying either on Dr. Bree's facts and arguments, or on the accuracy of his representation of those of Mr. Darwin and his supporters.

ALFRED R. WALLACE

OUR BOOK SHELF

Experimental Chemistry. Founded on the work of Dr. J. A. Stöckhardt. By C. W. Heaton, F.C.S. (London: Bell and Daldy.)

MANY students of chemistry have had reason to be grateful to Dr. Stöckhardt for his work on the Principles of Chemistry. For many years it was almost the only representative of its class; for it enabled students to acquire a considerable and useful knowledge of chemistry by teaching them to work experimentally at the subject, instead of merely reading about it. One of the great merits of his book, and which also belongs to the volume now under consideration, is that, although the number of experiments described is large and well selected, yet they do not necessarily require anything but extremely simple apparatus. This work, therefore, we believe will be found useful to a numerous and increasing class of students, who, though hindered by limited means and want of opportunity, wish to acquire some knowledge of chemistry.

The work is divided into four parts: the first treating of the General Principles; the second on the non-metallic elements; the third on the metals; and the fourth on Organic Chemistry. With the commencement of Part I. we confessed we were rather startled. The student is at once taken into a sort of half discussion as to whether matter has any existence or not, and the conclusion come to is that the problem will probably be for ever in dispute. This to a young beginner would scarcely impress him with the definite and unchangeable facts of Physical and Chemical Science.

Part I., on General Principles, is, we think, too advanced and complicated for the class of students by whom it is likely to be used. In fact, we should imagine that a student commencing the book and working by himself would find this part very up-hill work. For instance, before having studied any of the properties of the elements, he has to become acquainted with the various methods of fixing the atomic weights, the classification by atomicity, variations of atomicity, isomorphism, &c. In our opinion it would be almost better for a student to commence at the second part, that is, with the study of the non-metallic elements, in doing this, however, some little reference to Part I. would be necessary for the explanation of the meaning of symbols, &c., and he might then return to the complete study of Part I. The first part contains a number of definitions, several of which are not so good as they might be. It is said, for instance, that in a mixture the properties of the different ingredients are always perceptible. Gunpowder is given as an instance of a mixture; but in this the yellow colour of the sulphur and the white colour of the potassic nitrate are certainly not perceptible. Again, the definition of an acid is the following:—"An acid is composed of hydrogen with one of those radicles (p. 86) which are called acid radicles. The hydrogen can be replaced by metals, in which case one of the compounds called salts is formed. Acids redden litmus, and are commonly sour." On referring to p. 86, the exact definition of an acid radicle is not to be found; it is, as nearly as can be expressed, according to the author's ideas, the residue of an acid from which the hydrogen is abstracted. The definition of an acid, then, seems to be a body that contains hydrogen replaceable by metals, which is sometimes sour and reddens litmus. Surely definitions of a rather more definite and complete character might have been selected. Further in the book (p. 298) the author thinks it is often more convenient to regard the inorganic acids as hydrates, that is containing the radicle hydroxyl (HO), and of course uses this radicle throughout the organic acids. If the student accepts the two definitions he will have a double set of radicles, which would probably lead to much confusion.

The second part of the book is devoted to the non-metallic elements, the properties of which are studied by means of simple and instructive experiments, which are generally well described; the same is also the case with the next part, on the metals, and we then pass to the organic section of the work.

The field of Organic Chemistry is now so large that in the small space here devoted to it, a brief description only can be given of some of the more important compounds. It is also difficult in this section to arrange experiments which can easily be performed by students. It is thus necessary to confine the description of such classes of substances as the alcohols, the aldehydes, acids, haloid ethers, &c., to a very few pages. The arrangement, too, is peculiar, the experimental part of the organic work beginning with the study of cellulin, starch, sugar, &c., passing afterwards to the study of the more simple compounds, such as ethylic alcohol, acetic acid, &c.,—which seems rather like reversing the order of things. In a subsequent edition it would, perhaps, be better to adopt the modern system of classification, which would probably give the student a far better and more comprehensive knowledge of the subject. The book is, on the whole, one which, with a little reservation, can be safely recommended to students who wish to study Chemistry in the experimental way rather than simply to cram it up by reading. There is some room for improvement in the woodcuts, which in some instances are not artistic, and might be replaced by engravings of more modern and convenient apparatus.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Ocean Currents

I HAVE just read Mr. Ferrel's letter (NATURE, June 13), in which he refers to mine of April 25, and the proof therein adduced by me to show the physical impossibility of oceanic circulation being the result of differences of specific gravity. Unless Mr. Ferrel means (and I hardly think he does) that six foot-pounds of energy can perform 9,025 foot-pounds of work provided only sufficient time be allowed in which to perform that work, then I do not suppose there is any reader who may have glanced over my article on the subject who will not readily admit that Mr. Ferrel's reasoning has no direct bearing whatever on my argument.

The slope from the equator to latitude 60° is six feet. The total amount of work which gravity can perform upon a pound of water in carrying it down this slope is, of course, six foot-pounds. And this holds equally true whether the pound of water moves down the slope in, say one month, or takes 1,000 years to perform the journey; because the amount of work performed by gravity depends not upon the time which a body takes in descending, but upon the distance through which the body descends. In the present case six feet is the distance, consequently six foot-pounds is the amount of work performed upon the pound of water in its passage down the slope from the equator to lat. 60°.

Mr. Ferrel assumes that the velocity of the general movement of the water advocated by Dr. Carpenter does not exceed one mile per day, and that consequently the resistance to motion must be small. Undoubtedly the slower the motion the less the resistance; but so far as the argument under consideration is concerned, it is a matter of indifference whether we suppose the velocity of the water to be a mile per minute, a mile per day, or only a mile in 1,000,000 years; because it is found that when the water from the equatorial regions reaches, say, lat. 60°, it, as a matter of fact, is not moving eastwards relatively to the earth's surface with a velocity of several hundreds of feet per second, but with a velocity of only a few feet per second, perhaps not more than three feet at the utmost. In this case the water has lost 760 feet per second of velocity which it possessed when it left the equator. Each pound of water has therefore lost 9,025 foot pounds of energy. What has become of all this energy? It has all been consumed in overcoming resistance during the motion of the water from the equator to lat. 60°. But, be it observed, it has been consumed, not in overcoming resistance to the poleward motion of the water, but in overcoming the resistance to eastward motion. The energy consumed is deflecting energy, not impelling energy.

According to Dr. Carpenter's theory the pound of water has in virtue of gravity only six foot-pounds of energy to carry it from the equator to lat. 60° against all the resistances to its poleward motion; but it so happens that before the water reaches lat. 60° no less than 9,025 foot-pounds of energy is consumed in overcoming resistance to eastward motion. But if it requires 9,025 foot-pounds of energy to overcome the resistance to eastward motion, how is it possible that Dr. Carpenter's six foot-pounds of energy can overcome the resistance to the poleward motion? The velocity of the motion of the water polewards is as great as, if not greater than, the velocity of the motion eastwards, consequently the resistance to the motion of the water poleward must be as great as the resistance to the motion eastward. But if so, then the work of the resistances to poleward motion is 1,500 times greater than the work of gravity. The work of gravity being only six foot-pounds, whereas the work of the resistances is 9,025 foot-pounds.

One of two things must therefore follow as a necessary consequence: (1) either the work of the resistances to poleward motion is 1,500 times greater than the work of gravity, or (2) the work of the resistances to poleward motion is 1,500 times less than the work of the resistances to eastward motion. But either conclusion is equally fatal to the gravitation theory.

It seems to me that until the advocates of this theory manage to escape from this dilemma, it is needless to argue further on the matter. For, unless it can be shown that the work of the resistances is not greater than the work of gravity, the much disputed question as to whether or not difference of specific gravity can be the cause of a *general* interchange of equatorial and polar water must be regarded as finally settled in the negative.

I cannot help thinking but that Mr. Ferrel is misled by his supposed analogy between a slope produced by the influence of the attraction of the moon and that produced by difference of specific gravity. Although a slope of 9 feet in a quadrant resulting from difference of specific gravity is insufficient to produce motion of the water, nevertheless, the sea will easily regain its level after the attractive force of the moon is withdrawn, even though the height to which the surface of the ocean is raised may not exceed a single inch. The reason of the difference in the two cases must be obvious to any one who will reflect on the matter. I have already in my paper in the Phil. Mag. for Oct. 1871 alluded to this reason, and will have occasion again to refer to it at greater length.

I may notice that by a typographical error in my article the velocity of rotation at lat. 60° was stated to be 773 feet per second instead of 763 feet per second.

Edinburgh, July 18 JAMES CROLL

The Melbourne Telescope

MR. ELLERY has been so good as to send me an enlargement of the lunar photograph taken with the great Melbourne telescope, to which you allude at p. 228, No. 142 of your Journal. This picture, Mr. Ellery tells me, was taken on the second evening of trial; it is very beautiful, although not so *critically* sharp as several I have obtained with my Newtonian equatorial of 13 in. aperture, and a little more than 10 ft. focal length. This sharpness, however, is a mere question of the shadiness of the atmosphere; and I feel persuaded that pictures will be taken with the Melbourne telescope far surpassing any hitherto procured. In my telescope the focal image varies from 1 in. to $1\frac{2}{10}$ in. in diameter, according to the distance of the moon from the earth. The primary picture of the Melbourne telescope (an enlargement of which has been sent to me) is $3\frac{3}{10}$ in. in diameter; hence the structure of the collodion and minute defects in it are of much less importance than when smaller instruments are used.

The employment of the great Melbourne telescope for astronomical photography cannot fail to be of great advantage to astronomy, and I should be very glad to see a similar instrument at work in England, notwithstanding its too much abused climate.

WARREN DE LA RUE

P.S.—As soon as the Melbourne picture has been mounted and protected, I will place it in the Astronomical Society's rooms for inspection.

On the Rigidity of the Earth and the Liquidity of Lavas.

IN his letter upon the Rigidity of the Earth and the Liquidity of Lavas in the number of NATURE for July 11, Dr. Sterry Hunt has replied to my challenge to propose an explanation of the connection between mountain ranges and trains of volcanoes consistent with a rigid globe, other than that to which I refer it, viz. the production of fusion through a diminution of pressure due to the partial support of the mountains by the lateral thrust which has upraised them. Dr. Hunt suggests that liquefaction may take place beneath such ranges, through *increased* pressure promoting the liquefaction of the water-impregnated mass; and quotes the late Archdeacon Pratt as maintaining the existence of a greater pressure beneath mountain ranges.

In reply, I may be allowed to ask Dr. Hunt for a reference to such an expression of Pratt's opinion. I cannot call to mind any passage of his to that effect. The result of his calculations of the attraction of the Himalayas upon the plumb-line showed that they do not attract so much as they ought to do, and he explained this by supposing a deficiency of matter beneath the mountains. His own explanation of the phenomena, as given in the fourth edition of his Theory of the Earth, finished very shortly before his lamented death, is, "that the varieties we see in mountains and plains and ocean beds in the earth's surface, have arisen from the earth having been once a fluid or semifluid mass, and that in solidifying the mass has contracted unequally, so as to form hollows where the contraction has been greatest, into which water flowed and formed seas and oceans, and to leave high table-lands and mountain-ranges where the contra tion has been less." (He speaks here of contraction in the vertical direction.) A geologist will, I suppose, receive this as a very incomplete explanation; but the material point is that the Archdeacon was led to adopt it because he had discovered a deficiency of matter beneath the Himalayas. This seems incompatible with Dr. Hunt's view (both with regard to Pratt's opinion on the subject, and with regard to the fact itself) that there is an increased pressure beneath mountain ranges.

It will now appear that my "speculation" upon the origin of volcanic action was suggested by the proved deficiency of matter, and consequently probable diminution of pressure; and not that the idea of diminished pressure was invented to account for volcanic action. I have merely proposed a connection between lateral pressure and diminished density which seems most natural, namely, that the same pressure which upraised the mountains continues partially to support them. And I cannot see how it can do otherwise. For the abutments of the mountains having approached by contraction of the crust, cannot again recede without expansion, which cannot take place. Dr. Hunt's view of the liquefaction of lavas, to my mind, requires explanation. Admitting that pressure promotes aqueous liquefaction in heated rocks, when rocks so liquefied began to rise in a volcanic mass, would they not be brought under diminished pressure, and would they not become immediately solidified, so that they could not reach the surface in a fluid state?

Moreover, since liquefaction, according to this view, is increased by pressure, the interior parts of the earth being under greater pressure than the more superficial strata, ought, at least to that depth where water is present, to be more liquid, and this would be incompatible with the supposition of a rigid globe which Dr. Hunt favours.

Harlton, Cambridge O. FISHER

The Method of Least Squares

WILL you allow me to call the attention of Mr. J. W. L. Glaisher to the following sentence from Encke, Berliner Jahrbuch 1853, p. 311. "Ich werde mir deshalb erlauben, völlig dem Gange den Lagrange genommen hat folgend, wie konnte man sich erdreisten, bei der ungemeinen Klarheit, Einfachheit und Tiefe des grossen Meisters, eine irgend bedeutende Anderung vorzunehmen, den Theil der Abhandlung hier wiederzugeben, welcher den Beweis fur das arithmetische Mittel enthalt, und selbst Satze, die im Grunde schon die Methode der kleinsten Quadrate in sich begreifen." Also to article 17, Corollary, of the Memoir of Lagrange.

This is not the place to discuss the doctrine of the Method of Least Squares; but I may say that in my judgment the method rests on the assumption of the principle of the arithmetical mean, an assumption which is justified by an universal experience.

Having made this assumption, the rule that the sum of the squares of the remaining errors is a minimum follows very simply: *vide* the late Memoir of Hansen, art. 3.

With regard to the practical application of the Method of Least Squares, I think the whole honour of its introduction belongs to Gauss. The rules which he and his scholar Encke have given for the application of the method, and for executing the numerical operations, are so complete and perfect that but little more can be desired.

Washington, July 4 ASAPH HALL

Solar Rainbow

ON the 10th instant at about half-past seven in the evening I saw part of a well-defined rainbow about 5° west of the zenith, the convexity of the bow towards the setting sun, which at the time was about 3° above the horizon. Light clouds were passing beneath the bow. There was no rain.

Brighton, July 15 GEORGE DINNOW

Hive Bees *v.* Mechanism

I HAVE never followed Huber through his wonderful researches into the astounding working proceedings of hive bees—that elder Huber, who, by the way, aided by so admirable a spouse, brought his researches to so successful an issue, notwithstanding his blindness. Hence my excuse, if what I attempt to describe as being original to my own sense of observation, prove not so to others. I think it is conceded universally that amongst other leguminous plants peas have ever been secure from cross fertilisation, one variety with the other, in so far as natural influences, insect agency, &c., are concerned. Our stocks of garden peas, though known to run weedy and grow inferior when cultivated too long upon one kind of soil, very rarely, if ever, sport or vary as other plants placed in juxtaposition of species, especially varieties, are known to do. So decidedly has this fact been confirmed, that invariably sweet peas, even when it is desirable to grow them true to name, are sown in rows, side by side, whites, scarlets, blues, &c., with the utmost impunity. And this is wholly owing to the fact that the floral envelopes are so securely wrapped around the pistil and the stamens, that these parts cannot be reached without the exertion of more power than the strength bees and similar winged insects are supposed ordinarily to possess; unless, indeed, mechanism be called to their aid—a science in itself, but which, nevertheless, has been resorted to in the instance to which I would direct attention. Here then the hive-bees methodically bare the stamens by sheer mechanical force, and rob each of its load of pollen by sense of touch alone. And this they do in this wise. Alighting on the *alæ* or "wings" of each bloom, they first of all press their heads up under the base of that part of the papilionaceous corolla called the "standard," or *vexillum*, and extract what nectar they find. Then, with their little heads firmly pressed therein, and holding fast by the four fore legs, they exert their power, thus artificially contrived, by treading down both the aforesaid wings and the "keel," or *carina*, which so securely envelopes the sexual organs, that they protrude, so that the anthers are laid bare, when they generally rub the pollen off the stamens on to their hairy bellies, only occasionally using one hinder leg to aid them. It will be seen that they do this with their hinder legs and body, at a time when their heads are entirely hidden from view. I have tested these parts in regard to the pressure needed to disclose the pollen thus, and find that a pressure of half to three quarter ounce is necessary; and computing the weight of an individual bee to be about the sixteenth of an ounce, we see what an amount of power must be exerted in this hind-before, or blindfold manner, by these interesting little creatures.

I should add, however, though I have been a selecter of sweet peas for more than a dozen years in other parts of the country, as the selection sold by some seedsmen with my signature attached confirms, I nevertheless have not previously believed in any power possessed by insect agency to thus destroy selections. Here, at Valentines, however, being only seven miles from London, it would appear that the bees, like town sparrows, are unusually "wise in their generation," and that, owing to scarcity of honey-yielding materials, they are driven to such wonderful feats as I have explained.

WILLIAM EARLEY

The Gardens, Valentines

The Red Rocks

IF the peroxide of iron was deposited (as in the Swedish lakes) as brown hydrous peroxide, and if long boiling in the laboratory may be considered analogous to evaporation in an inland sea, then it would appear from the following extract from Watts's "Dictionary of Chemistry" that there is no difficulty in accounting for the colour of the red rocks:—

"A remarkable insoluble modification of ferric hydrate is produced by boiling the ordinary yellow hydrate to $2\,Fe_2O_3\,3\,H_2O$ (precipitated from the chloride by ammonia) in water for seven or eight hours. The colour then changes from ochre-yellow to brick-red, and the hydrate thus altered is scarcely acted upon by strong boiling nitric acid, and but very slowly by hydrochloric acid. In acetic acid, or dilute nitric or hydrochloric acid it dissolves, forming a red liquid, which is clear by transmitted, but turbid by reflected light; is precipitated by the smallest quantity of an alkali salt or a sulphate; and on addition of strong nitric or hydrochloric acid, yields a red granular precipitate, which redissolves on diluting the liquid with water."

The change of colour from brown to red is readily obtained by boiling the hydrate in a flask for several hours, as described above. The change is gradual, and before becoming finally red the precipitate is of a chocolate colour, corresponding with that sometimes observed in the red sandstones.

Small pieces of white sandstone, introduced into the flask during the boiling, are of course coloured red, and resemble red sandstone when taken out and dried. R. D. P.

Instantaneousness of Lightning

DURING a recent night thunderstorm I got out my colour-top, with the usual disc of so-called primary colours arranged to blend into grey or white on rotation, in order to show to my children the instantaneousness of the lightning, and that by its light the disc would, as I had no doubt, appear stationary in one or several successive positions according to the character of the flash, as it does by the light of an ordinary electric spark from a Leyden jar or induction coil. On trying the experiment, however, by turning the disc (about forty times in a second) at a window in a dark room opposite to the cloud in and from which the discharges were taking place, I found that this was only very partially the case. When the direct stroke was actually visible, or only slightly veiled by cloud, the effect I looked for was produced, the bands of colour standing out clear and apparently motionless; but at other times during the apparently (to the eye) prolonged flash, the colours blended so as to indicate a continuous fainter light in addition to the occasional instantaneous appearance of definite colour and form due to the intermittent light of the discharge. Of course I satisfied myself that there was no other light to account for this, the night and the room being very dark in the intervals of the flashes, and I repeated the experiment in another night storm (on the 11th) with just the same result. The effect appears to be due to the retention of light in the cloud by phosphorescence, and, so far as I can find on inquiry, does not seem to have been noticed before.

If my view is correct, it would explain the fact that distant lightning at night, when no cloud is in sight, is apparently so much sharper than when nearer.

A propos to the subject, a letter to the *Times* describing the storm of the 11th inst., speaks of the lightning "flickering with a perceptible rustle"—a curious instance of transference of impression from one sense to another, the visual sensation of flickering being exactly analogous to the auditory sensation of rustling. B. W. SMITH

Hampstead, July 20

Severe Thunderstorm

A HEAVY thunderstorm passed over this neighbourhood this afternoon. One of the flashes was followed by a smart "snap" like that of the discharge of a large Leyden jar, or the explosion of a heavy percussion cap. The interval between this noise and the crash of the thunder was about half or three-quarters of a second. I have not unfrequently noticed a hissing noise immediately preceding a violent clap of thunder, but never anything so distinctly resembling the sound of the ordinary electric spark.

ALEX. BEAZELEY

Willesden Lane, Kilburn, July 23

TECHNICAL EDUCATION

ON Saturday last a meeting was held at the rooms of the Society of Arts, of scientific men and others interested in the promotion of technical education among the working classes, and especially among the skilled artisans of this country. The chair was taken by H R.H. Prince Arthur, who showed in the remarks which he addressed to the meeting an admirable appreciation of the extreme value of the subject to the future prosperity of the country. After the Secretary of the Society of Arts had explained the proposal of Captain Donnelly that the Society should establish technological examinations for the purpose of testing the practical knowledge and skill required in the application of the scientific principles involved in each art and manufacture, H.R.H. said:—

"The Society of Arts are endeavouring to encourage among those who are practically employed in various industries the study of art, and an accurate knowledge of its application in each branch of manufacture. The Society will not, however, attempt to teach the practice of those arts. Their object is rather to lay a sound foundation of all the principles on which those practices may be carried out to the best advantage. The Society wish and desire to give certificates, prizes, and scholarships to those who show that to practical skill as workmen they have added an accurate knowledge of natural principles in those matters. I am certain the necessity and advantage of combining scientific principles with practice will be seen by all. I only wonder that we are but just beginning to undertake this task. The machinery for these technological examinations already exists, and, in fact, the skeleton of industrial universities is ready to our hands. What we now require is funds to clothe it with and give it life, and to enable it to carry out its work—that is, to obtain sufficient prizes to encourage and reward the deserving youth of this country. If others would only do what Sir Joseph Whitworth has done by his noble endowment for mechanical engineering, we might soon hope to see our most sanguine expectations realised."

Little by little the country is gradually waking up to understanding the principles on which alone its future greatness as a manufacturing nation can rest, Many will probably echo H.R.H.'s wonder that "we are but just beginning to undertake this task."

VOLCANOES OF CENTRAL FRANCE

THE conduct of the war against the Goths having brought Procopius into Campania, he is thus led to give an account of Vesuvius. In the text before me the mountain is called Bebios, and ash is said to have fallen at Byzantium; but undoubtedly Vesuvius is meant, and there seem to be good reasons for suspecting that "Byzantium" should be "Beneventum." Both Besuios (= Vesuios) and Beneventum could be so written as easily to be mistaken for the names now in the text. I cannot indeed pretend to say when, I must leave that to the critics.

Procopius, "De Bello Gothico," l. ii. c. 4: "At that time Mount Vesuvius was roaring, but there was no eruption; however, in consequence of the roaring, an eruption was confidently expected and the surrounding population were in great fear. This mountain is about seventy stadii from Naples, bearing northwards from that place (*τετραμμένον αὐτῇ πρὸς Βορρᾶν ἄνεμον*). [The real bearing is about 10° S. of E. Is he confounding Naples with Stabiæ, where Pliny died?]* It is naturally escarped,—wide-spreading below, but the top steep and very rugged; and on the summit of Vesuvius, in the centre, there is a cavern, which appears to be remarkably deep, and it would seem as if it extended quite to the extremities (*ἔσχατα*) of the mountain: and any one who is bold enough to look down may see fire in it. It is of no consequence to the inhabitants when no flame is seen above it; but when the mountain sends forth an explosive roaring it generally happens that not long afterwards a vast quantity of dust is thrown up, and if this destructive shower meets a traveller on the road he can by no means escape with his life; and if it descends on houses, they fall, crushed down by the mass of ash; and if a violent wind happens to be blowing, the ash is carried to a great height, out of people's sight, and is transported wherever the wind may carry it, and it may fall in some country at a very great distance. And they say that formerly, when it fell in Byzantium [*qu.* Beneventum?], the people there were so alarmed that they have made it a rule (*ἔγνωσαν*) in general assembly from that day to this to propitiate the deity by yearly supplications (*λιταῖς*). At another time (they say) it fell in Tripolis in Lybia. And they say that this roaring first (?) took place a hundred or more years ago, and that latterly it occurred much more frequently, but with decreasing violence.* They say, too, that when Vesuvius happens to eject this dust, the country where it falls is sure to produce abundant crops. On this mountain especially, beyond all others, the air is very light, and highly conducive to health, and time out of mind medical men have unhesitatingly sent thither their consumptive patients. Such, in the main, are the circumstances in connection with Vesuvius."

Vesuvius lies about 40° 47′ N. by 14° 26′ E., Byzantium about 41° N. by 28° 59′ E.; so a difference of 14° 33′ of longitude on the parallel of 41°, or, by a rough estimate, a distance of 750 miles. Is it possible that ashes ever fell at that distance in such quantities as to terrify the people, and drive them to institute Rogations? Is it likely that a professional historiographer, a resident in the very town where the alarm is said to have occurred, and where the Rogations had been instituted and constantly observed, could have first heard of these facts in Italy and from Italians? If this is scarcely credible, it suggests that we should read Beneventum. It is about thirty miles from the mountain—a distance not so great but that such events might have occurred there. He who wishes to have a vivid picture of them, let him read Pliny's second letter to Tacitus (vi. 20) describing what happened to himself at Misenum. The erroneous reading is somewhat countenanced by the fact that the statement seems intended to be an instance of great distance; whereas probably the heavy fall of ash and the consequent Rogations alone caused Beneventum to be mentioned, and Tripolis was the only case of distance.

While, therefore, the geologists are searching for ash at Vienne, the archæologists might oblige us by searching the annals of the principal Sees round Vesuvius, especially Beneventum. There were also Neapolis, Surrentum, Salernum, and perhaps other Sees, which our bishop might have held, but the scattered rays of light seem best to converge on Beneventum. At a much later date it is described as an Imperial vicariat; did it bear any such relation to the empire in the middle of the fifth century? Did it possess walls, a spacious forum, and a resident nobility? Could people have seen, or have fancied they saw, deer taking refuge in the forum? Is there any record or mention of a Bishop Mamertus, or of some intrusive bishop who may have been Mamertus? Labbe's "Sacrosancta Concilia" (at the British Museum or the London Library) might be consulted to see whether any hint is given of Mamertus having been in Italy, either in the letters of Leo, or (more probably) in those of his successor, Hilary (iv. pp. 1032—1047).

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* It has occurred to me to suggest πρὸς Βάριον ὅρμον for πρὸς Βορρᾶν ἄνεμον This would not be an unnatural reference in the secretary of a Byzantine general, if the Greek emperors already used that port for military purposes.

* My translation of this passage is open to criticism. I have even ventured to read ἀπισχυρισάμενον for ἀπισχυρισάμενοι, and I give an unusual sense to the word, by analogy to ἀποπλουτέω, ἀπομανθάνω, ἀποκραιπαλίζομαι &c The context seems to me imperatively to demand it. The Latin translation renders the text, as it stands, thus:—"According to them it is now a hundred years or more since the prior roaring occurred; the memory of the other is much more recent; moreover they affirm that it cannot but be, that, &c." Can θᾶσσον be translated by "more recent"? and why such violent asseveration about a simple agricultural fact? The difference does not affect our question, but it is of some importance in its bearing on the History of Vesuvius.

Procopius got his information during the first Gothic war (A D. 535—540); the second lasted from A.D. 544 to A.D. 548. He died about A.D. 565, at the age of 60. Ought the "century or more ago" to be computed from the date of his information, or from the time when he wrote? This might make a difference of 10 or 20 years. If the former, then the commencement (?) and violence of the eruptions are thrown back to the early part of the fifth century. In any case they must have preceded rather than followed the interval from 455 to 463. Where did Prof. Daubeny get his date of A.D. 472? Certainly not here.

It is therefore very likely that about A.D. 455 Vesuvius may have been in a state of violent activity. In that year Rome was sacked. These are precisely the conditions which the hypothesis requires. Procopius was told that the Rogations were occasioned by such eruptions at about that date. If, then, Mamertus was the sole (?) author of them, he must have been present where the Terrors occurred. Is there positive evidence of the fact? If no positive evidence, for or against, can be found, it will then be time enough to go into other questions of probability. Here for the present I conclude.

HENRY NORTON

WATER ANALYSIS

II.

IN the last article[*] we assumed that the object of estimating the organic carbon and nitrogen has been fully understood by the reader; but in order to render all chance of our meaning being misunderstood in the remarks which are to follow, it will be as well to briefly capitulate the reasons which render these determinations so valuable. Carbon and nitrogen form with hydrogen the principal constituents of all organised bodies, and hence are found in all the excreta of animals. Nitrogen is found in animals to a much greater extent than in plants, while in the inorganic world this latter element is scarcely found at all, and carbon only in the form of carbon, coal or carbonic acid, none of which bodies are likely to be found in water except the last, the first two being insoluble. It hence follows that if a large quantity, or indeed any quantity, of carbon and nitrogen be found in water, their source can only have been organic in its nature, and if the proportion of nitrogen to carbon be more than one to five, the source is almost absolutely certain to have been animal.

Accordingly, when we find these two elements existing in waters in the proportions just indicated, we are justified in assuming the presence of some form of animal or organic contamination in them; and of all forms in which this contamination can exist, sewage is the most probable. It will thus be seen that when chemists assert that a water is contaminated with sewage, they do so on grounds the truth of which is easily demonstrable. That such waters containing effete animal matters are injurious, no arguments of ours will be required to prove; no persons of authority in sanitary matters have presumed to assert that such waters are harmless.

With regard to the method of obtaining evidence as to this contamination, however, considerable difference of opinion exists, and in the former paper we have endeavoured to show the worthlessness of the early processes of estimating organic matter by ignition of the residue, and by treatment with permanganate of potash; and though this last process is condemned by all, it is still in use.

We have also given reasons for regarding with distrust the results obtained by the use of Chapman, Wanklyn, and Smith's method of indirect determination of organic matter from the amount of ammonia evolved by the water, as we maintain that it cannot be shown that the ammonia evolved bears any distinct relation to the amount of organic matter present, and that with many waters it is difficult to obtain an accurate estimate of the ammonia thus evolved.

The process we have now to consider bases its claims to confidence on the fact that it gives an absolute determination of the quantity of carbon and nitrogen present in the water, and that it does do this can, we think, be proved without much difficulty.

The process is based upon the fact that when a body containing carbon and nitrogen is heated in contact with cupric oxide to bright redness, the carbon is converted into carbonic anhydride at the expense of a portion of the oxygen of the cupric oxide, while the nitrogen is liberated, partly in the free state and partly in the forms of its lower oxides, the quantity of these latter being reduced as much as possible by causing them to pass over the surface of red-hot metallic copper, which abstracts the oxygen and leaves the nitrogen free In other words, the substances are obtained and estimated by what is known to chemists as a "combustion."

As far back as 1864 an unsuccessful attempt had been made by Weltzien to apply this process to the estimation of carbon in water. The failure was due to the very minute quantities available for estimation, and to the fact that the water was rendered acid with sulphuric acid before evaporation, a proceeding which directly tended to vitiate the results, even had no other obstacle intervened, as the acid gradually concentrated during evaporation, ultimately became sufficiently strong to char and decompose much of the organic matter present.

It was not until Dr. Hermann Sprengel had placed in the hands of chemists a new and powerful means of research in the shape of the admirable air-pump which bears his name, that it was possible to estimate directly the minute amounts of carbon and nitrogen which water, as a rule, contains.

In March 1868 Messrs. Frankland and Armstrong published a method of water analysis which they had elaborated, after eighteen months' work at the subject.

A quantity of water proportionate to the amount of ammonia found,[*] and varying from one litre in the case of a town water-supply, to 100 cubic centimetres in the case of a much polluted water, is introduced into a flask, and 15 cubic centimetres of a saturated solution of sulphurous anhydride are added, and the water boiled briskly for three minutes; the water is then removed from the source of heat, and a portion introduced into a hemispherical lipless glass dish of about 100 to 120 cubic centimetres capacity; this is placed on a steam bath, and to the first dishful two or three drops of a moderately strong solution of ferric chloride are added. Should the water leave but a small solid residue, or contain little calcic carbonate, a few drops of a solution of sodic sulphite should also be added. The dish is then covered with a cap of filtering paper stretched over a ring of cane, and the evaporation continued, the rest of the water being kept warm in the flask, and added from time to time to the dish. The laboratory in which this operation is performed should be kept free from dust, and no ammonia should ever be allowed in it.

The rationale of the process so far is very simple, and is as follows:—The first boiling with sulphurous acid expels the free carbonic acid in the water, and also any that may exist in combination as calcic carbonate, and during the evaporation the remaining sulphurous acid reduces and expels the nitrous and nitric acids present, in which the ferric chloride greatly helps it; the sulphuric acid which is thus formed is neutralised by the calcic sulphite (formed during the preliminary expulsion of carbonic acid) with liberation of fresh sulphurous acid, or should no calcic salt be present, the sodic sulphite is added to effect this.

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[*] See NATURE, vol. vi. p. 104.

[*] If the ammonia be less than 0·1 part per 100,000, 1 litre should be used, if more than 1·0, a hundred cubic centimetres or less. (See Sutton's "Volumetric Analysis." J. A Churchill, New Burlington Street, 2nd Edition, 1871, pp 246-295.)

As soon as the whole of the water is evaporated, the residue is carefully detached from the dish by means of a flexible steel spatula, and thoroughly mixed with some fine cupric oxide; it is then introduced into a stout piece of combustion tube about 430 millimetres long, one end of which has been closed, after having been very carefully cleansed, and which has about 30 millimetres of its length filled up with coarsely granulated cupric oxide.[*] After the mixed residue and cupric oxide have been introduced, the tube is filled up to within 100 millimetres of the open end with granular cupric oxide; a tightly-rolled cylinder of copper gauze covered with sheet copper, and which has been recently ignited and cooled in a current of hydrogen, is then put in, lastly a few millimetres more of granular cupric oxide, and the tube is then drawn out in the blowpipe flame, put into a combustion furnace, connected with a sprengel pump, and, while the anterior part of the tube is being heated to redness, thoroughly exhausted of air. The pump is then stopped, and the heat gradually carried backwards until the whole of the combustion tube has been heated to redness. Any gas which may come off is collected from the bottom of the Sprengel pump over mercury, and as soon as the gas ceases to be evolved, the furnace is allowed to cool slightly, and the tube again exhausted. The gas pumped off is transferred to an apparatus for the analysis of gases, and measured, after the absence of sulphurous anhydride has been insured by the introduction into the gas of a drop of a solution of dipotassic dichromate. The carbonic anhydride is then absorbed by potassic hydrate, and the gas again measured, the difference being the carbonic anhydride. To the residual gas a minute bubble of oxygen is added to decompose the nitric oxide, should any be present, and after the excess of oxygen has been absorbed by pyrogallic acid, the gas again measured consists of nitrogen; to this half the difference between the two last readings is added, as this represents the nitrogen which had existed as nitric oxide, and the result is the total nitrogen.

All the above data are reduced to measures of weight by the use of the formula. Log. $\frac{\cdot 0012562}{(1 + 00367t)760}$ which gives the weight of cubic centimetres of nitrogen in grams. This table of logarithms is carried out for each tenth of a degree from 0° to 30°C.

To those uninitiated in gas analysis, the above may sound very complicated; but in practice it is found extremely simple. The whole analysis, including the calculations, can easily be carried out in half an hour, and the combustion itself need not occupy more, as a rule, than forty-five minutes.

The value of the above process is at once perceived when it is remembered that by it 0·0000005 gram. carbon and 0·000001 gram. nitrogen are distinctly measurable quantities. The methods of analysis involving the use of gasometric measurements are by very far the most accurate in the whole range of a science whose very foundations rest on the possibility of accurately weighing and measuring varying quantities. The great capabilities of this method of inquiry have received a splendid demonstration in the researches of Sir Benjamin Brodie on Ozone; and Profs. Williamson and Russell, recognising the peculiar excellence of these methods, have endeavoured with considerable success to make them applicable to a more extended range of work, in connection with which it will only be necessary to remind the reader of the careful determinations of the atomic weights of cobalt and nickel made by the latter chemist.

Notwithstanding these advantages, the application of this method to the determination of the organic constituents of water has encountered the most strenuous opposition in many quarters.

But until a more absolutely certain method of determining the quantities of carbon and nitrogen shall have proved Frankland and Armstrong's process to be in error, it would, indeed, be the height of folly to adopt in its place a method which, like the Albumenoid process, only professes to give a fraction of the nitrogen present (and has absolutely failed to prove that it even gives any known fraction whatever); while it does not even attempt to estimate the carbon at all. Again, over 60 per cent. of the errors given by Frankland and Armstrong in their paper are minus errors, and thus directly tend to favour the Albumenoid process when analysis of waters by the two methods are compared; for the Albumenoid process almost invariably gives less quantities of ammonia than Frankland and Armstrong's does of nitrogen. It must be borne in mind that these absolute errors are almost certainly due to errors in weighing the small quantities of substances used to test the process, quantities so small that they are admitted by the objectors to be only about one-tenth of those usually used in organic analysis.

But in the case of water no weighing at all occurs. A measured quantity of water is taken, and the quantity of carbon and nitrogen which happen to exist in that measure is ascertained, whether it be small or great.

Again, it has been urged that ammonia is lost during the evaporation. This was found to be the case; but it was a difficulty only requiring to be known in order to be remedied. Accordingly, solution of salts of ammonia made acid with sulphurous acid in one case, and with metaphosphoric acid in the other (the last acid replaces the first when sewage is operated on), were evaporated and the nitrogen determined, and from the loss found two tables[*] constructed, in which each alternate term was an absolute determination, and the intermediate ones calculations. The wonderful constancy with which the numbers alter for each strength of solution shows how accurate the determinations must have been in order to obtain them.

Another valuable proof of the trustworthiness of a process is to be found in the degree of agreement existing between duplicate determinations made with it; and when the process is examined in this way it certainly passes the test in the most satisfactory manner, and the following examples will show how well duplicate determinations agree with each other:—

		Parts per 100,000.	
		Organic Carbon	Organic Nitrogen.
Grand Junction Water Company . .	1	·185	·030
,, ,, ,, . .	2	·172	·030
East London ,, . ,, . . .	1	·157	·026
,, ,, ,, . . .	2	·148	·030
New River ,, ,, . . .	1	·239	·042
,, ,, ,, . . .	2	·231	·042
The Don at Alford	1	·115	·024
,, ,, ...	2	·112	·026
Lady Well Spring (Dundee) . . .	1	·029	·035
,, ,, ,, . . .	2	·023	·033

Indeed, the numbers given in the Registrar-General's monthly reports exhibit so remarkable an agreement among themselves that any unprejudiced judge must admit the accuracy of the method by which they were obtained. We shall consider the remainder of the subject in a concluding article.

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[*] Made by igniting and oxidising short pieces of copper wire; it can be obtained from dealers in chemicals.

[*] See Sutton's "Volumetric Analysis," p. 276, 2nd Edition.

THE BLIND FISHES OF THE MAMMOTH CAVE AND THEIR ALLIES[*]

THE blind fish of the Mammoth Cave has from its discovery been regarded with curiosity by all who have heard of its existence, while anatomists and physiologists have considered it as one of those singular animals whose special anatomy must be studied in order to understand correctly facts that have been demonstrated from other sources ; and, in these days of the Darwinian and development theories, the little blind fish is called forth to give its testimony, pro or con.

Before touching upon this point, however, we must call attention to the structure of the fish and its allies, and to others that are either partially or totally blind.

In the lancelet (*Branchiostoma*) and the hag (*Myxine*) the eye is described " as simple in form as that of a leech, consisting simply of a skin follicle coated by a dark pigment, which receives the end of a nerve from the brain." Such an eye speck as this structure gives would only answer for the simple perception of light. In the young[†] of the lampreys (*Petromyzon*) the eye is very small and placed in a fold of the skin of the head, and probably of little use, as these young remain buried in the sand ; but as they attain maturity, and, with it, the parasitic habits of the adult, their eyes are developed to a fair size, thus reversing the general rule in the class.

In most other fishes the eyes are developed to a full and even remarkable extent as to size and perfection of sight in water. In *Anableps*, or the so-called four-eyed fish of the fresh waters of Central and South America, which belongs to a closely allied family with our blind fish, the *Cyprinodontidæ*, the eyes are not only fully developed, but are divided into an upper and lower portion in such a way, by an opaque horizontal line, as to give the effect of two pupils, by which the fish probably sees as well when following its prey on the surface with its eyes out of water, as when under water. But it is in the interesting family of Cat fishes (*Siluridæ*) that we find the most singular arrangement of eyes in perfect adaptation to the diversified modes of life of the numerous species. In this family the eyes assume nearly every possible modification from partial and even total blindness to perfectly developed eyes, and these organs are placed in almost every conceivable position in a fish's head ; from the ordinary large eyes on the side, to small ones on top of the head, enabling the fish to see only what is above ; to the oval eyes on the side, in some just back of the mouth, situated in such a way that the fish can only see what is in close proximity to its jaws or even below them. Many genera of this family found in South America,[‡] Africa,[§] and Asia,[‖] have the eyes so small and buried under the skin, or protected by folds or cartilage, as evidently to be of no more use than simply to distinguish light from darkness.

Among the most interesting forms of this family, in this respect, is the genus described by Prof. Cope under the name of *Gronias nigrilabris*. This fish is very closely allied to our common bull pout or horned pout, and of about the same size (ten inches in length). It was taken in the Conestoga river in Lancaster Co., Pennsylvania, where it is " occasionally caught by fishermen and is supposed to issue from a subterranean stream said to traverse the limestone in that part of Lancaster Co., and discharge into the Conestoga." We quote the following from Prof. Cope's remarks on the fish .[*]—

" Two specimens of this fish present an interesting condition of the rudimental eyes. On the left side of both a small perforation exists in the corium, which is closed by the epidermis, representing a rudimental cornea ; on the other the corium is complete. Here the eyeball exists as a very small cartilaginous sphere with thick walls, concealed by the muscles and fibrous tissue attached, and filled by a minute nucleus of pigment. On the other the sphere is larger and thinner walled, the thinnest portion adherent to the corneal spot above mentioned ; there is a lining of pigment. It is scarcely collapsed in one, in the other so closely as to give a tripodal section. Here we have an interesting transitional condition in one and the same animal, with regard to a peculiarity which has at the same time physiological and systematic significance, and is one of the comparatively few cases where the physiological appropriateness of a generic modification can be demonstrated. It is therefore not subject to the difficulty under which the advocates of natural selection labour, when necessitated to explain a structure as being a step in the advance towards, or in the recession from, any *unknown* modification needful to the existence of the species. In the present case observation on the species in a state of nature may furnish interesting results. In no specimen has a trace of anything representing the lens been found."

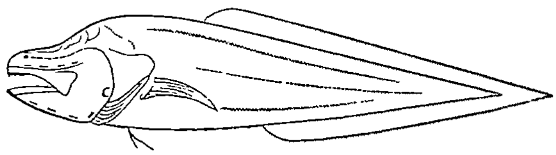

Fig 1.—Cuban Blind Fish : Lucifuga subterraneus.

When we remember that the lens of the eye in *Amblyopsis* has been found, even though the eye is less developed in all its parts than in *Gronias*, it is probable that a careful microscopical examination would show its existence in this genus also.

It is interesting to note that this fish is black above (lighter on the sides and white below), notwithstanding its supposed subterranean habits, and that all the other members of the family having rudimentary or covered eyes are also dark coloured, while the blind fishes of the Mammoth Cave and of the caves in Cuba are nearly colourless. This want of colour in the latter fishes has been considered as due to their subterranean life. If this be the cause, why should the blind cat-fishes retain the colours characteristic of the other members of the family living in open waters ?

The fishes which in a general way, so far as blindness, tactile sense, and mode of life are concerned, come the nearest to the blind fishes of the Mammoth Cave, are those described by Prof. Poey[†] under the names of *Lucifuga subterraneus* and *L. dentatus*.[‡] These fishes having the broad, flattened, fleshy head, with minute cilia, without external eyes, and inhabiting caves so similar in structure to the Mammoth Cave, make a comparison of them with the fishes of the Mammoth Cave most interesting. This is greatly enhanced by the fact that the Cuban fishes belong to a family of essentially marine habit, quite far removed from *Amblyopsis*. The fresh water ling (*Lota*), belonging to the same great group of fishes (though to a distinct family or sub-family) containing the cod on the one hand and the Cuban blind fish on the other, is probably the nearest fresh water relative of the Cuban fish, but the nearest representative yet known is the marine genus *Brotula*, one species of which is found in the Carribean Sea.

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[*] Reprinted from the *American Naturalist*, a sequel to "The Blind Crustacea of the Mammoth Cave." See NATURE, vol. v. pp. 445, 484.

[†] These young lampreys have been described under the generic name of Ammocœtes, and it was not until 1856, when Prof Muller discovered the fact of a metamorphosis in the lampreys, that their true position was ascertained. Prof. Muller has traced the history of the common European species, and shown that it is three or four years in attaining its perfect form. With this fact before us and with the early stages of the Myxinoids still unknown, have we not some reason for suspecting that the lancelet may yet prove to be a larval form of the Myxinoids, notwithstanding that it is said to lay eggs? Why should we not suspect the existence in the very lowest vertebrates of something akin to alternate generations, or of larvæ capable of reproduction? Without having any facts to support such an assumption, except that, on general principles, the young of Myxine would probably be very much like Branchiostoma, and that its young is not known, while Branchiostoma has only been found in waters where some species of Myxinoid exists, I think that before the position of the lancelet is firmly established, we must know the embryology of the Myxinoids, for should the lancelet prove not to be the young of the Myxinoids, it must necessarily form a distinct class of animals, perhaps as near to the molluscs as to the vertebrates.

[‡] *Pimelodus cyclopium* of Humboldt, *Helogenes*, *Agoniosus* and other genera.

[§] *Eutropius congensis*.

[‖] *Ailia*, *Shilbichthys*, *Bagroides*, and other genera.

[*] Proceedings of the Academy of Natural Sciences of Philadelphia for 1864, p 231

[†] Memorias sobre la Historia Natural de la Isla de Cuba, por Felipe Poey. Tomo 2, pp. 95-114 Pls 9, 10, 11 Habana, 1856-8.

[‡] This species was afterwards referred to the genus *Stygicola* Gill, on account of the presence of palatine teeth which are wanting in the other species. There are also several other good characters, to judge from the figures of the head, skull, and brain given by Poey, that would warrant the reference of the fish to a distinct genus from *L. subterraneus*.

In the Cuban blind fish we find ciliary appendages on the head and body quite distinctly developed, evidently of the same character as those of *Amblyopsis*, and answering the purpose of tactile organs. These cilia are in the form of small but plainly visible protuberances (reminding one of the single fleshy protuberance over the opercular opening just back of the head in *Amblyopsis*). There are eight of these on top of the head of a specimen I hastily examined, received from Prof. Poey by the Museum of Comparative Zoology, and quite a number arranged in three rows on each side of the body, showing that tactile sense is well developed in this fish: though it is rather singular that the barbels on the jaws, so usually developed as organs of touch in the cod family and its allies, are entirely wanting in this fish.

The brain of *Lucifuga subterraneus*, as represented by the figures of Poey, differs very much from that of *L. dentatus* and of *Amblyopsis*. In all, the optic lobes are as largely developed as in allied fishes provided with well developed eyes. In *Lucifuga subterraneus* the cerebral lobes are separated by quite a space from the round optic lobes, which are represented as a little larger than the cerebral lobes, and also of greater diameter than the cerebellum; this latter being more developed laterally than in either *L. dentatus* or *Amblyopsis*. The three divisions of the brain are represented, from a top view, as nearly complete circles (without division into right and left lobes), of which that representing the optic lobes is slightly the largest. In *L. dentatus* the procencephalon and the optic lobes are represented as divided into right and left lobes, as in *Amblyopsis*, and the cerebellum does not extend laterally over the medulla oblongata as in *L. subterraneus*, but, as in *Amblyopsis*, is not so broad as the medulla, and, projecting forwards, covers a much larger portion of the optic lobes than is the case in *L. subterraneus*.

The Cuban blind fish has the body, cheeks, and opercular bones covered with scales. As in *Amblyopsis*, the eyes exist, but are so imbedded in the flesh of the head as to be of no use. The outline cut here given (Fig. 1), copied from Poey, is very characteristic of the form of the fish, but does not exhibit the fleshy cilia or details of scaling.

The first notice that I can find of the Mammoth Cave blind fish is that contained in the "Proceedings of the Academy of Natural Sciences of Philadelphia," vol. i. p. 175, where is recorded the presentation of a specimen to the Academy by W. T. Craige, M.D., at the meeting held on May 24, 1842, in the following words:—

"A white, eyeless crayfish (*Astacus Bartoni?*) and a small white fish, also eyeless (presumed to belong to a sub-genus of *Silurus*), both taken from a small stream called the River Styx in the Mammoth Cave, Kentucky, about two and one-half miles from the entrance."

Dr. DeKay, in his "Natural History of New York Fishes," p. 187, published in 1842, describes the fish, from a poor specimen in the Cabinet of the Lyceum of Natural History of New York, under the name of *Amblyopsis*[*] *spelæus*.[†] De Kay's description is on the whole so characteristic of the fish as to leave no doubt as to the species he had before him, though the statement that it has eight rays supporting the branchiostegal membrane (instead of six), and that the eyes are "large" but under the skin, must have been due the bad condition of his specimen, and to his taking the fatty layer covering the minute eyes for the eyes themselves, as pointed out by Prof. Wyman. Dr. DeKay places the genus with the Siluridæ (cat-fishes), but at the same time questions its connection with the family, and says that it will probably form the type of a new family. In 1843 Prof. Jeffries Wyman[*] gave an account of the dissection of a specimen in which he could not find a trace of the eye or of the optic nerve, probably owing to the condition of the specimen, as he afterwards[†] found the eye spots, and made out the structure of the eye. When describing the brain, Prof. Wyman calls attention to the fact of the optic lobes being as well developed as in allied fishes with well developed eyes, and asks if this fact does not indicate that the optic lobes are the seat of other functions as well as of that of sight. He also calls attention to the papillæ on the head as tactile organs furnished with nerves from the fifth pair.

Dr. Theo. Tellkampf[‡] was the first to point out the existence of the rudimentary eyes from dissections made by himself and Prof. J. Muller, and to state that they can be detected in some specimens as black spots under the skin by means of a powerful lens. Prof. Wyman afterwards detected the eye through the skin in several specimens. Dr. Tellkampf also was the first to call attention to the "folds on the head, as undoubtedly serving as organs of touch, as numerous fine nerves lead from the trigeminal nerve to them and to the head generally."

It is also to Dr. Tellkampf that we are indebted for the first figure of the fish,[§] and for figures illustrating the brain and internal organs. The descriptions of the anatomy of the fish by Drs. Tellkampf and Wyman are all that have ever been written on the subject of any importance, with the exception of the description of the eye by Dr. Dalton, whose paper, in the *New York Medical Times*, vol. ii. p. 354, I have not seen. Prof. Poey gives a comparison of portions of the structure with that of the Cuban blind fishes.

Dr. Tellkampf proposed the name of *Heteropygii*[‖] for the family of which, at the time, a single species from the Mammoth Cave was the only known representative, and makes a comparison of the characters with those of *Aphredoderus Sayanus*, a fish found only in the fresh waters of the United States, and belonging to the old family of Percoids, but now considered as representing a family by itself, though closely allied to the North American breams (*Pomotis*), and having the anal opening under the throat, as in the blind fish.

Dr. Storer,[¶] not knowing of Dr. Tellkampf's paper, proposed the name of *Hypsæidæ*, for the blind fish, and placed it between the minnow and the pickerel families, in the order of Malacopterygian, or soft-rayed fishes. According to the system adopted by Dr. Gunther, it stands as closely allied to the minnows, *Cyprinodontidæ* (many of which are viviparous and have the single ovary and general character of the blind fish), and the shiners, *Cyprinidæ*, of the order of Physostomi. Dr. Tellkampf, in discussing the relations of the family, points out its many resemblances to the family of *Clupesoces*, and its differences from the Siluroids, Cyprinodontes, and Clupeoids, with which it has more or less affinity, real or supposed. Prof. Cope in his paper on the Classification

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[*] Obtuse vision.

[†] Of a cave.

[*] *Silliman's Journal*, vol 45, p 94.

[†] Proceedings Boston Soc. Nat Hist., vol. 4, p 395. 1853

[‡] Muller's Archiv fur Anat., 1844, p 392 Reprinted in the New York *Journal of Medicine* for July, 1845 p. 84, with plate.

[§] The only other figures of the species, that I am aware of, are the simple outlines given in Poey's "Mem de Cuba," the woodcut in Wood's "Illustrated Natural History," and the cut in Tenney's "Zoology." None of these figures are very satisfactory

[‖] From the advanced position of the terminus of the intestine being so different from the position which it has in ordinary fishes

[¶] "Synopsis of the Fishes of North America," published in 1846.

of Fishes* places the *Amblyopsis* in the order of Haplomi with the shore minnows, pickerel and mud fish, and in an article on the Wyandotte Cave,† he says that the Cyprinodontes (shore minnows) are its nearest allies. This arrangement by Prof. Cope places the Haplomi between the order containing the herrings and that containing the electric eel of South America, all included with the garpike, dog fish of the fresh waters (*Amia*), cat fishes, suckers, and eels proper, &c. in the division of Physostomi as limited by him.

Prof. Agassiz‡ in 1851 stated that the blind fish was an aberrant form of the Cyprinodontes.

Thus all those authors who have expressed an opinion as to the position which the fish should hold in the natural system, have come to the same conclusions as to the great group, division, or order, into which it should be placed. For all the terms used above, when reduced to any one system, bring *Amblyopsis* into the same general position in the system; its nearest allies being the minnows, pickerels, shiners, and herrings; and unless a careful study of its skeleton should prove to the contrary, we must, from present data, consider the family containing *Amblyopsis* as more nearly allied to the Cyprinodontes, or our common minnows having teeth on the jaws, than to any other family, differing from them principally by the structure of the several parts of the alimentary canal and the forward position of its termination.

I have thus far mentioned only one species of blind fish from the cave, the *Amblyopsis spelæus*. The waters of the cave not only contain another species of blind fish, differing from *Amblyopsis* in several particulars, especially by its smaller size and by its being without ventral fins, which I have identified as the *Typhlichthys subterraneus* of Dr. Girard; but also a fish with well developed eyes, as proved by the account given by Dr. Tellkampf, and by the drawing of a fish found by Prof. Wyman, in 1856, in the stomach of an *Amblyopsis* he was dissecting. It is very much to be regretted that the specimen is not now to be found, and that it was so much acted on by the gastric juice as to destroy all external characters by which it could be identified from the drawing which Prof. Wyman made of it, which is of about natural size. Dr. Tellkampf remarks on the fish with eyes are as follows:—

"Besides the colourless blind fish, there are also others found in the cave, which are black, commonly known by the name of mud fish. I saw a dark-coloured fish in the water, but did not succeed in catching it. The latter are said to have eyes, and are entirely dissimilar to the blind fish."

The name "mud fish," given to this fish with eyes, and the statement that it is of a dark colour, together with the drawing by Prof. Wyman of the fish found in the stomach of the blind fish, showing the position of the dorsal fin to be the same as in the fish commonly called mud fish in the fresh waters of the Middle, Western, and Southern States, perhaps indicates the fish with eyes to be a species of *Melanura*. This fish is called mud fish from the habit of burying itself in the mud, tail first, to the depth of two to four inches, and of remaining buried in the mud in our western ditches during a time of drought. This habit, perhaps, in a measure fits it for a subterranean life. The occurrence of a fish belonging to the same family with the blind fish, but with well developed eyes, in the subterranean streams in Alabama, however, renders it probable that the cave fish with eyes may be the same or an allied species, and the drawing by Prof. Wyman would answer equally as well for it.

The fact that the *Amblyopsis* succeeded in catching a fish of probably very rapid and darting movements, shows that the tactile sense is well developed, and that the blind fish must be very active in the pursuit of its prey; probably guided by the movement which the latter makes in the water so sensibly influencing the delicate tactile organs of the blind fish that it is enabled to follow rapidly; while the pursued, not having the sense of touch so fully developed, is constantly encountering obstacles in the darkness.

In describing the habits of the blind fish, Dr. Tellkampf says:—

"It is found solitary, and is very difficult to be caught, since it requires the greatest caution to bring the net beneath them without driving them away. At the slightest motion of the water they dart off a short distance and usually stop. Then is the time to follow them rapidly with a net and lift them out of the water. They are mostly found near stones or rocks which lie upon the bottom, but seldom near the surface of the water."

Prof. Cope, in describing the habits of the blind fish which he obtained in a stream that passes into the Wyandotte Cave, though he entered it by means of a well in the vicinity of the cave, says that—

"If these *Amblyopses* be not alarmed they come to the surface to feed, and swim in full sight like white aquatic ghosts. They are then easily taken by the hand or net, if perfect silence be preserved, for they are unconscious of the presence of an enemy except through the sense of hearing. This sense is, however, evidently very acute, for at any noise they turn suddenly downward, and hide beneath stones, &c., at the bottom. They must take much of their food near the surface, as the life of the depths is apparently very sparse. This habit is rendered easy by the structure of the fish, for the mouth is directed upwards, and the head is very flat above, thus allowing the mouth to be at the surface."

The blind fish has a single ovary, in common with several genera of viviparous Cyprinodontes. In three female specimens of *Amblyopsis* which I have opened, the ovary was distended with large eggs, but no signs of the embryo could be traced. In these three specimens it was the right ovary that was developed, and this was by the side of the stomach, and did not extend beyond it. The number of eggs contained in the ovary was not far from one hundred in the specimen examined. As the embryos develop, the mass probably pushes further back in the cavity and also extends the abdominal walls. That this fish is viviparous is proved by the statement made by Mr. Thompson before the Belfast Natural History Society* that one of the blind fishes from the cave, four and a half inches long, "was put in water as soon as captured, where it gave birth to nearly twenty young, which swam about for some time, but soon died. These, with the exception of one or two, were carefully preserved, and fifteen of them are now before us (at the meeting, I wish they were here); they were each four lines in length."

It is singular that no mention is made regarding these young, as to the presence or absence of eyes, and, as if it was fated that this important point should remain unnoticed as long as possible, it is equally singular that Dr. Steindachner omitted to examine some very young specimens which he received from a friend a few months since and sent to the Vienna Museum, where they will remain unexamined until he returns there. I saw the Doctor only a week after these, to me, interesting specimens had been sent abroad, and he was as grieved as I was disappointed at my being just too late to take advantage of them.

At what time the young are born has never been stated, but judging from such data as I can at present command, I think that it must be during the months of September and October. Specimens collected during those months would probably contain embryos in various stages of development, the examination of which would undoubtedly lead to most interesting results.

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* *American Naturalist*, vol. v. p. 579, 1871.

† *Indianapolis Daily Journal* of September 5, 1871. Reprinted in Ann. Mag. Nat. Hist., Nov. 1871.

‡ *Silliman's Journal*, p. 128.

* Annals and Mag. of Natural History, vol. xiii. p. 112, 1844.

The largest specimens I have seen of *Amblyopsis* are several males and females, each from four to four and a half inches in length, which seems to be about as large as the fish grows, though Dr. Gunther mentions a specimen in the British Museum of five inches in length. The largest specimen captured of late years is said to have been taken during the summer of 1871, and sold for ten dollars to a person who was so desirous of securing the precious morsel that he had it cooked for his supper. The smallest specimen I have seen was one and nine-tenths inches in length.

F. W. PUTNAM

NOTES

ADDRESSES of sympathy with Dr. Hooker, in the difficult position in which he is placed with regard to the management of Kew Gardens, are flowing in on all sides, from those interested in the maintenance of Kew as a scientific establishment. At a meeting of the leading botanists and horticulturists held last week at the office of the *Gardener's Chronicle*, resolutions were unanimously passed expressing the sympathy of the meeting with Dr. Hooker, and in favour of an address to Mr. Gladstone, calling attention to the eminent services rendered by him to science, and pointing out that it is absolutely essential for the efficient management of the establishment at Kew that the Director must have complete control over the subordinate officials, free from any interference in matters of detail from his official superiors. The same course has been followed by the Council of the Royal Horticultural Society, and by its various committees. At a Council of the Royal Botanic Society, specially summoned for the purpose on Saturday last, similar resolutions were passed. The Council of the Meteorological Society has sent to Mr. Gladstone, Lord Derby, Sir John Lubbock, and Dr. Hooker, copies of a resolution in the same direction.

AN interesting *soirée* was held at the Royal Albert Hall on Thursday evening last, under the auspices of the Society of Telegraph Engineers, with the aid of the Postal Telegraph Department. A model of every kind of telegraph instrument which has been generally used for commercial purposes from the opening of telegraphs to the present time was exhibited. Each apparatus was connected up with wires proceeding from the central hall to the galleries, and thus the actual practical working of the telegraph system was made apparent to those present in a very effective manner. A descriptive lecture was at the same time given by Mr. W. H. Preece, of the Postal Telegraph Department, who lucidly explained the action of the electric current in producing the simple elementary signals The admirable manner in which the lecturer made himself heard in every part of the vast assembly was a general subject of remark. The most interesting feature of the entertainment was the direct and instantaneous working with India. Kurrachee, the terminus of the Indo-European line in India, a distance of upwards of 5,000 miles, was the town selected, and, in reply to a message of inquiry, that station said, "Here, Kurrachee," and followed it up with the announcement that "locusts are swarming in Scinde." After the conversations held with India from the Albert Hall were concluded, the Grand Vizier of Persia, who was at Teheran, sent to the Albert Hall a warm message of congratulation to the Prince of Wales.

Les Mondes speaks with contempt and surprise of the treatment which science meets with at the hands of the British Government, referring particularly to the refusal of the latter to grant a sum for the purpose of enabling scientific men to make marine explorations on the coasts of Europe and India, and to the conduct of Mr. Ayrton towards Dr. Hooker. The latter is spoken of as a gentleman "eminent, talented, universally honoured for his integrity, loved for his courteous manner and the goodness of his heart, and who has devoted to the service of the State a life not only laborious but illustrious."

THE new French Association for the Advancement of Science intends to hold its first meeting at Bordeaux in the month of September next, commencing on the 5th.

WE are sorry to see the following paragraph in *Les Mondes* of July 18:—"No one has yet responded to the appeal we made to the French *savants* to take part in force (*en nombre*) in the Brighton meeting, which opens on August 14. We shall not organise this scientific excursion unless we are able to count upon a sufficient number of adherents. There is no time to lose." We hope it is not too late, and that something like a representative party may be yet organised from among all classes of French men of science. We are sure we can promise they will be heartily welcomed and hospitably treated at Brighton.

THE Annual Meeting of the British Medical Association will be held in Birmingham on the 6th, 7th, 8th, and 9th days of August next.

MR. W. A. TILDEN, D.Sc. Lond., Demonstrator of Practical Chemistry to the Pharmaceutical Society, has been appointed Chemical Master in Clifton College.

BY the provisions of the late Dr. William J. Walker's foundation, two prizes are annually offered by the Boston Society of Natural History for the best memoirs, written in the English language, on subjects proposed by a Committee appointed by the Council. For the best memoir presented, a prize of sixty dollars may be awarded; if, however, the memoir be one of marked merit, the amount may be increased, at the discretion of the Committee, to one hundred dollars. For the memoir next in value a sum not exceeding fifty dollars may be given; but neither of these prizes is to be awarded unless the papers under consideration are deemed of adequate merits. Memoirs offered in competition for these prizes must be forwarded on or before April 1, 1873, prepaid, and addressed, "Boston Society of Natural History, for the Committee on the Walker Prizes, Boston, Mass." Each memoir must be accompanied by a sealed envelope enclosing the author's name, and subscribed by a motto corresponding to one borne by the manuscript. The subject of the Annual Prize of 1873 will be "On the Development and Transformations of the Common House Fly."

THE Academy of Sciences and Belles Lettres of Caen has offered a prize of 4,000 francs for a paper on "The part played by leaves in vegetation." What is wanted is an account of exact experiments and new facts calculated to clear up, invalidate, confirm, or modify the doubtful points in the received theories. Papers must be sent in before December 31, 1875, addressed to M. Travers, secretary of the Academy, Caen.

ON Thursday last Mr. Thomas Baring, M.P., F.R.S., distributed prizes and certificates to the students who had passed the examinations connected with the educational courses of lectures delivered at the London Institution during the past session. In the examination on Prof. Huxley's lectures, "On the Physiology of Bodily Motion and Consciousness," the first prize was gained by H. B. Hyde, jun.; the second by Miss Caroline Lloyd; and the third by A. J. Wallis. In the examination on Dr. Odling's course, "On Elementary Chemistry," the first, second, and third prizes were carried off by H. Louis, A. J. Richardson, and Miss Eleanor F. Garrett. In the examination connected with the lectures "On the Theory of Music," delivered by Mr. E. J. Hopkins, the first prize was obtained by Alfred Hare, and the second by Miss Frances S. Voysey. In the examination on Prof. Bentley's course, "On the Classification of Plants," Miss Eleanor F. Garrett gained the first prize, Miss Elizabeth J. Garrett the second, and L. T. Thorne the third. Sixty-six Pass Certificates were granted, and no fewer than twenty-two young ladies were among the successful candidates.

WE are glad to see from the report on the teaching of Practical Physiology in the London schools, which appears in the *Medical Times* of Saturday, that the means and method for teaching this subject to University College, the only one yet reported on, are in every way satisfactory.

Harper's Weekly records the death of Mr. Coleman T. Robinson, of New York, a gentleman well known for the attention he has paid to the subject of American lepidoptera. A man of wealth, and able to gratify his tastes in this direction, he acquired, by his own efforts and by purchase, a very extensive cabinet, embracing over twenty thousand specimens, which not long ago he presented to the American Museum of Natural History in the Central Park. Mr. Robinson published quite extensively upon American lepidoptera—a paper upon the sphinges of Cuba being perhaps the most important. In some of his labours he was associated with Mr. Grote.

WE hear from America of the death of Mr. Robert Swift, who had paid great attention to terrestrial and marine mollusca. His valuable cabinet of shells will be presented by his daughter to the Philadelphia Academy of Sciences. He had already given to the Museum of the Smithsonian Institution an extensive series of specimens of the birds of St. Thomas and Porto Rico.

A MONUMENT has been erected to Boerhaave, the great naturalist and physician, at Leyden in Holland. The statue is 11 ft. 8 in. high, and stands on a pedestal of 10 ft. in height. The figure represents him as a professor lecturing.

WE learn from the *Times of India* of June 28 that the *Madras Mail* and other journals in the Madras Presidency are altogether discontented with the recent judgment of the Court of Inquiry as to the cyclone, to which we referred last week. The *Mail* has subjected the judgment to a searching criticism, and, after comparing it with the evidence, concludes that the Court throughout has erred egregiously on the side of severity. It seems to be thought that the Board of Trade will either reverse or modify the Court's judgment.

DR. E. DECAISNE, in a note communicated to the Academy of Sciences on the 3rd of June, shows that from the triple point of view of the fecundity of marriages, of the absolute number of births, and of the excess of births over deaths, France occupies the lowest place of all the European States. In Prussia 100 marriages give 460 children; in France the same number of marriages give only 300 children. In Prussia the number of births for each 100 of the population generally is 3·98 per annum; in France there are only 2·55. In Prussia the excess of births over deaths in each million of the population is 13,000 per annum, whilst in France it only amounts to 2,400. "If we admit," says Dr. Decaisne, "as a conclusion from the above figures, that the doubling of the population of France, despoiled of two of its finest provinces, and by unheard of disasters, will require 170 years to be effected, whilst that of Prussia requires only 42 years, Great Britain 52, and Russia 66, some estimate may be made of the amount of the evil that has befallen this country." He does not hesitate to say that, as the first step towards the restoration of the former power of France, the first thing that should occupy her statesmen is the reconstitution, the reorganisation of human life, and everyone should throw aside that false patriotism which has been the cause of so much ill.

AT its last session the Congress of the United States made an appropriation of 75,000 dollars for the continuation of Professor Hayden's geological exploration of the Territories; and on the 15th of June the Professor had already established his camp at Ogden, Utah, preparatory to prosecuting his researches. The present session seems likely to be marked by still further additions to our knowledge of the physical and natural history of the central and western regions of the United States.

ACCORDING to the *Yale College Courant*, a new era in the educational development of the Japanese has been entered upon in the opening of an exhibition of curiosities of nature and art in Yedo in the beginning of April last. The formation of collections of this kind is usually characteristic of an advanced stage of culture; and in imitating the European and American example in this respect the Japanese show their great superiority to the Chinese and other Oriental nations. The exhibition referred to was opened in a temple sacred to the spirit of Confucius, and situated in the grounds of the old Chinese college. This institution was the chief seat in Japan of the study of Chinese literature, but has been closed for some years, as the study of the Chinese has now become obsolete. The exhibition, to which a charge for admission of about two cents was made, was projected by the Japanese themselves; and although small, yet, according to the writer in the *Courant*, it was really very good and well selected. The specimens were those mainly pertaining to the fauna and flora of Japan, embracing reptiles, fishes, insects, and birds, the last being well stuffed and mounted. Specimens of timber, in polished slabs, were exhibited; and the cases of insects were filled with a very great variety of species. To the wonders of nature were added numerous art curiosities, mainly of old and rare patterns of articles of lacquered bronze.

A CORRESPONDENT of the *Chemical News* asks why the Cavendish Society has for years ceased to publish works of scientific value, and suggests that some of the works of its founders and officers—Faraday, Graham, Hofmann, Daubeny, Miller, Stenhouse—as well as others, are so scarce, that their publication by the Society would be a boon to many, and would remunerate it for its trouble in publishing them.

GENERAL MYER, the indefatigable head of the United States Signal Service, has proposed, according to *Harper's Weekly*, to take the occasion of various balloon ascensions during the present season, especially from Boston, to make observations in regard to the temperature, barometric pressure, and the currents of the higher altitudes of the atmosphere. Sergeant Schaeffer, of the corps, has been detailed for this purpose, and has been in training for some time preparatory to his important work.

WE learn from the Fifth Annual Report of the Peabody Institute of Baltimore, for the year ending June 1, that upwards of 13,000 dollars have been spent on books and binding, and that during the year 120 lectures have been delivered at the Institute —30 being popular lectures, and 90 what are called class lectures, designed for more minute instruction in special branches of knowledge." Of the popular lectures Prof. W. H. Miles gave ten—mostly geological—on such subjects as "Revelations of the Microscope and of the Deep-Sea Soundings," "Coral and the Coral Islands," "Glaciers, Rivers, and Oceans," "The Geological History of Man," &c. The class lectures are divided into six courses, of which two are scientific—the one consisting of twenty lectures on Physiology by Prof. F. T. Miles, of Maryland University, and the other twenty lectures on Sound and Heat by Prof. H. C. White, of St. John's College, Baltimore. The average number in each class, independent of single admission, was thirty-seven, the charge for a course of twenty lectures being only 3 dols. During the year 3,883 volumes besides pamphlets have been added to the library, the number of readers having been 2,582.

JUDGING from the Report of the Exeter Science School in connection with the Government Department of Science and Art, it seems altogether in a very satisfactory condition. There are classes for Physical Geography, Mathematics, Acoustics, Light and Heat, Chemistry, Animal and Vegetable Physiology, Geology, Mineralogy, Metallurgy, Botany, Building Construction, Theoretical Mechanics, Machine Construction, and Drawing. From the number who have passed the examinations, it would appear that the classes must have been well attended, and several of the students have most creditably distinguished themselves.

THE following is a list of the electric lights in England and France with the dates at which they were erected:—Dungeness, Jan. 1862; Cape La Heve, France, South Light, Dec. 1863, North Light, Nov. 1866; Cape Grisnez, France, Feb. 1869; Souter Point, England, Jan. 1871; South Foreland, 2 lights, Jan. 1872. It is interesting to see that England took the lead in this matter of the adaptation of electric illumination to lighthouse purposes, and it must also be remembered that although the first electric light was only erected in 1862, yet that in 1859 experiments were made under the supervision of the late Prof. Faraday which were very successful.

MR. WILLIAM F. DENNING, of Bristol, writes us that the sun's surface has recently been in a very disturbed condition. On observing the sun on the afternoon of the 12th instant with an old 4in. metallic-mirror reflecting telescope, he noticed a large scattered group of spots in the north-eastern quadrant of the disc. This group contained no less than twenty-seven individual spots, one of which was of considerable dimensions. It was situated on the eastern portion of the group, and was constituted of three well-defined umbræ and a large irregular penumbra, which on the east side was very dark, and on the exterior edge pierced with a train of minute dark spots. In the other quadrants Mr. Denning noticed four groups and one isolated spot surrounded by penumbra in the north-western quadrant. These groups (though insignificant in regard to the dimensions of the spots which composed them) contained twenty-one spots in all; so that, including the large cluster before referred to, there were forty-eight dark spots seen altogether. Several groups of faculæ were also perceptible in the vicinity of the margin of the disc.

THE first number of the *Journal of the Society of Telegraph Engineers* contains, besides a list of members, and the rules and regulations, a record of the proceedings of the Society since its formation, including reports of the papers read, and the discussions which followed. The members already number about 280, and among them are the names of some of the most eminent scientific men of the time, the President being Charles W. Siemens, and the Vice-Presidents Lord Lindsay and Frank I. Scudamore, C.B. The society "is established for the general advancement of Electrical and Telegraphic Science, and more praticularly for facilitating the exchange of information and ideas among its members," and consists of members, associates, students, and honorary members. Besides the President's address, in which he justly maintains that such a special society "is necessary for the more rapid development of a new and important branch of applied science," the report contains a paper by Mr. R. S. Culley on Automatic Telegraphs, and a sketch of the Progress of Sea Telegraphy by Captain Colomb, R.N. The latter half of the volume consists of "Abstracts and Extracts," bearing on the department with which the society is concerned.

A NOVEL kind of magazine has made its unpretending appearance—*Loose Leaves*, a magazine conducted at the Church Stretton Private Asylum. It is written almost entirely by members of the asylum, and we have seen many madder publications proceeding from those who consider themselves sane. As an effort to occupy the minds of the unfortunate inmates of such establishments, the attempt is commendable, and worthy of all success and imitation.

HISTORICAL ECLIPSES

MR. J. R. HIND, writing from Mr. Bishop's Observatory, Twickenham, to the *Times*, supplies the following interesting sketch of the Eclipses recorded in History:—

"It is well understood that the historical eclipses, especially those of the sun, have an important bearing upon our knowledge of the elements of the moon's motion, as affording the means of testing the accuracy of those elements when carried back to very remote times. I send you a brief account of some results I have deduced in a systematic examination of these eclipses, making only such a selection therefrom as may possibly possess interest for the general reader. I shall omit any reference to the purely astronomical conclusions to which I have been led, which would be out of place in your columns, and, indeed, would extend this communication beyond reasonable limits. It may, however, be desirable to state that I have employed the last value of the secular acceleration of the moon's mean motion given by Prof. Hansen, of Gotha, the author of the latest lunar tables, and have combined other important elements as determined by him with the results of M. Leverrier's tables of the sun. From recent investigations it appears by no means improbable that we may have to rely wholly upon the ancient eclipses in fixing the true amount of acceleration in the motion of our satellite.

"I shall follow the chronological order in the subjoined remarks upon some of the better known eclipses of history. These form a part only of the phenomena I have rigorously examined upon the same system of calculation.

"1. The Nineveh Eclipse of B.C. 763, June 15.—The discovery of the record of this eclipse on one of the Nineveh tablets in the British Museum was announced by Sir Henry Rawlinson in the *Athenæum* of May 18, 1867, to which I refer for details of its bearing on the sacred and profane history of the period. In the actual state of our knowledge it is the *terminus 'a quo* for researches on the historical eclipses, and I believe I am correct in saying its value in an astronomical point of view is greater than that attaching to the famous eclipse predicted by Thales to the Ionians, as mentioned by Herodotus. The underlining of the inscription appears to indicate a phenomenon of unusual character, or that the eclipse was total in or near Nineveh. Adopting for the position of the city the longitude and latitude deduced by the Astronomer Royal for the pyramid of Nimrud, I find the calculated southern limit of totality would pass a few miles south of Nineveh, leaving a very large partial eclipse at that city. Very trifling corrections in the lunar elements employed would suffice to bring the total eclipse over it. In this longitude the duration of totality on the central line would be 4m. 20s., the middle of the eclipse at half-past 9 local time.

"2. The Eclipse of B.C. 689, January 11.—The idea that the retrogression of the shadow on 'the dial of Ahaz' during the illness of Hezekiah may have been connected with a solar eclipse has given rise to much discussion, and several writers have endeavoured to point out how the occurrence might thus be explained. Of the eclipses to which attention has been directed, the above has perhaps appeared the more probable. It was an annular eclipse, and at Jerusalem the sun would present the form of a luminous ring for 7½ minutes, the middle at 10h. 18m. In Babylon it would have the same appearance for seven minutes. It seems hardly probable that the eclipse could have occurred much later in the day, though more than one author has considered the circumstance essential for the explanation of the retrograde motion of the shadow on the ancient form of sun-dial by an eclipse. I must leave the reader to judge how far the expression 'the wonder done in the land' may relate to such a phenomenon, which is, of course, a very rare one in a particular locality.

"3. The eclipse of Thales, B.C. 585, May 28.—This eclipse, which, as Herodotus informs us, terminated the six years' war between the Medes and Lydians under Cyaxares and Alyattes, when during a battle 'day was suddenly turned into night,' has greatly exercised the chronologist and the astronomer, and although, misled by imperfect tables of the lunar motions, they have fixed upon other eclipses from time to time, it has been known for some years past that the date distinctly assigned by Pliny (the fourth year of the 48th Olympiad) is the correct one. My new calculation throws the shadow precisely over the tract of country where with the greatest probability it has been supposed the contending armies were situated, and in addition it indicates a circumstance which I believe has not resulted from any previous calculation, and which may not be without its chronological import, viz., that the eclipse was total in Nineveh for between three and four minutes shortly before sunset. The date of the final destruction of Nineveh is closely connected with the eclipse of Thales.

"4. The Eclipse of Xerxes, B.C. 478, February 17.—Much difficulty has been experienced by chronologists with regard to an eclipse which occurred, according to Herodotus, in the early spring, when Xerxes was setting out from Sardis on his expedition against Greece. It is certain there was no such phenomenon in the year B C. 480, to which this event is usually referred, and in examining the eclipses about this period I have found only one that can apply. There is no doubt that the sun was very largely eclipsed at Sardis on the morning of February 17, B.C. 478. A direct calculation for this place shows that more than 94-100ths of the sun's diameter would be covered, the greatest phase ten minutes after 11, local time. The eclipse was annular, and Sardis appears to have been just outside the annulus. One other eclipse only was visible in eastern Europe about this year, it occurred B C. 479, October 2, and has been considered to be the one which occurred at the time Cleombrotus consulted the oracles at Sparta. Its magnitude there is found to have been about 6-10ths, the greatest eclipse at 0h. 50m. If the eclipse of B.C. 478 be truly the one recorded by the historian, the date of the battle of Salamis will be required to be brought down two years.

"5. The Eclipse of Agathocles, B C. 310, August 15 (Diodorus, Justin)—On the morning after the fleet of Agathocles sailed from Syracuse for Africa, the historian tells us the sun was eclipsed to such a degree (*tantum fit solis deliquium*) that 'the stars everywhere appeared as at night. Though Agathocles could hardly have been more than 100 miles from Syracuse, it is uncertain in which direction he had sailed, or whether he was rounding Sicily on the north or south side, and this circumstance detracts from the scientific value of the record. My calculation throws a central line near the African coast, so that the fleet, if sailing southwards, would be near the northern limit of totality.

"6. The Eclipse on the Passage of the Rubicon by Cæsar (Dion), B.C. 51, March 7.—This would appear to have been a very notable phenomenon on the Rubicon and in Northern Italy generally. The eclipse was annular, and the annular phase continued 6m. 30s. At Rome there would be a partial eclipse, about three-fourths of the sun's diameter being covered. A line drawn from 9° 24′ E., and 43° 26′ N., to 14° 39′ E., and 46° 15′ N., will define the course of the central eclipse across Italy, and the ring-formed appearance of the sun would extend to about 1° 35′ north and south of this line. The Rubicon would be placed about midway between the central line and the southern limit. Near Ariminum the middle of the eclipse occurred at 0h. 50m. By some writers (including the Abbé du Fresnoy, in his valuable 'Tablettes Chronologiques,') the eclipse is dated B C. 50; the above, however, is the correct year.

"A great eclipse has been referred to the year B C 43 or 44, soon after the death of Julius Cæsar, and it is instanced by Baron de Zach and M. Arago as the first annular eclipse upon record. Calculation shows that there could not have been an eclipse, annular or otherwise, visible in Italy in either of those years, nor, indeed, for several years before or after. The phenomenon alluded to was, no doubt, of a meteorological character, and this would appear from the passage in Suetonius, one of the authors quoted upon the subject.

"7. The Eclipse of Herod (Josephus).—The lunar eclipse which I take to be the one recorded by the Jewish historian during Herod's last illness occurred B C. 1, January 9. On this occasion the moon passed nearly centrally through the earth's shadow, entering in at 11h. 23m. P.M. mean time at Jerusalem, and emerging at 2h. 57m. A.M. on the 10th; the total eclipse continued 1m. 39s. This is the date recognised by Calvisius and recently supported by Mr. Bosanquet. An eclipse in B C. 4 on the night between March 12-13, which other chronologists have supposed to be the one referred to, was partial only, and did not commence till 1 A.M.; little more than half the moon's diameter was immersed in the earth's shadow at greatest phase.

"8. The Eclipse of Phlegon in the 202nd Olympiad (Eusebius) A.D. 29, November 24.—Total on a line crossing the Black Sea rather west of Odessa in Sinope, thence near the site of Nineveh to the Persian Gulf. At Jerusalem a partial eclipse; about 11.10 A M. eight-tenths of the sun's diameter would be covered; at Heliopolis (Baalbec) also partial—nine-tenths. At a point on the central line near Sinope the totality would continue $1\frac{1}{2}$ minutes. Humboldt mentions that this eclipse had been calculated by Wurm, but I have not met with his results. It is the only solar eclipse that could have been visible in Jerusalem during the period usually fixed for the ministry of Christ.

"The moon was eclipsed on the generally received date of the Crucifixion, A.D. 33, April 3. I find she had emerged from the earth's dark shadow a quarter of an hour before she rose at Jerusalem (6 36 P.M.): but the penumbra continued upon her disc for an hour afterwards.

"9. The Eclipse of 113, May 31.—Kepler, after endeavouring to ascertain the date of a total eclipse mentioned by Plutarch as having 'recently occurred about noon,' when the darkness was like that of night, and stars were seen in all directions, states he had found none which accorded better with the description than the above On submitting it to calculation on the modern elements, the central line appears to have passed too far north—over central Germany. I have not succeeded in discovering the date of this eclipse, though I have accurately examined several at the close of the first and beginning of the second century.

"10. The Eclipse of 418, July 19.—Very large at Constantinople, according to Philostorgius, who relates that at the eighth hour of the day the sun was so far eclipsed that the stars appeared, and a comet which had not been previously perceived became visible during the obscurity, and was watched for more than four months afterwards. According to my calculation the central line passed somewhat to the south of Constantinople, where ninety-five hundredths of the sun's diameter would be covered. At a very short distance below that point the eclipse would be total. This is the second occasion upon which the discovery of a comet during a total, or nearly total, eclipse of the sun is recorded in history.

"11. The Eclipse of 671, December 7, on the attempted removal of the pulpit of Mahomet from Medina.—Prof Ockley, in his 'History of the Saracens,' mentions on the authority of several Arabian writers, a large solar eclipse which occurred about the 52nd year of the Hegira. The Caliph Moawiyah having formed the intention of removing the Prophet's pulpit from Medina to his residence at Damascus, his people proceeded to do so, 'when immediately to their great surprise and astonishment the sun was eclipsed to that degree that the stars appeared.' Baron de Zach refers the eclipse to 674, October 4, but in this he is certainly mistaken—I believe through a wrong assumption as regards the moon's latitude. The correct date would appear to be 671, December 7. The eclipse of this day was annular on the central line. At Medina the greatest phase occurred at 10h. 43m., when 85-100ths of the sun's diameter would be obscured. In the clear skies of that part of the world such a degree of eclipse might be sufficient to bring out the brighter planets or stars. No larger eclipse, visible at Medina, occurred about this epoch.

"12. The Eclipse of 840, May 5.—Among the causes which are said to have brought on the *maladie de langueur* that terminated the life of Louis le Débonnaire was 'the fright which a total eclipse of the sun had occasioned him.' It is related that the King was taken ill at Worms, and having been removed to Ingelheim, near Mayence, he died there on June 20. I find the northern limit of totality in this eclipse passed about 100 miles south of Worms, and on the central line in this longitude the total eclipse continued 5m. 25s., an unusually long interval for the latitude of Central Europe. The middle occurred at 1.15 P.M., with the sun at an altitude of 57°. The phenomenon under such circumstances must have been a very imposing one, and well calculated in those days to inspire alarm.

"I have already described in your columns the track of the total eclipse of 1140, March 20 (William of Malmesbury) across this country, and merely refer to it now to add, that if any one of your readers is aware of its being recorded as total in London, he might be doing an astronomical service by making the fact generally known.

"13. The Eclipse of 1133, August 2 (William of Malmesbury), a great solar eclipse, considered as foreboding evil to Henry I. of England.—The central line traversed Scotland from Ross to Forfar, and the eclipse was, of course, large in every part of the country. It would be total in Northumberland. In the centre of Forfarshire totality continued 4m 20s. Berwick-upon-Tweed was about 20 miles within the south limit.

"During the existence of the kingdom of Jerusalem there is mention of an eclipse which would appear to have been total in the city or its immediate neighbourhood, and has been variously dated from the election of Godfrey of Bouillon in 1097. I am inclined to think it must be to the eclipse of August, 1133, that the record applies, though previous or subsequent events may have been mixed up with it by the historian. Continuing the calculation of the track of total eclipse after leaving this island, I find it would enter Palestine near Jaffa, and pass over Jerusalem and Hebron, where the sun would be hidden 4¼ minutes about 3 P.M., and from Nablous on the north to Ascalon on the south the country would be in darkness for nearly the same interval. The magnitude of the eclipse of 1187, September 4, was rather more than 9-10ths at Jerusalem, the central line passing between eight and nine degrees to the north; in the eclipse of 1191, June 23, the magnitude was about 7-10ths.

"14. The Eclipse of 1433, June 7, long remembered in Scotland as "the black hour."—It was a remarkable eclipse, the moon being nearly in perigee and the sun not far from apogee. The central line traversed the country in a south-easterly direction, from Ross to Forfar, passing near Inverness and Dundee. Maclaurin mentions that in his time a manuscript account of this eclipse was preserved in the University of Edinburgh, wherein the darkness is said to have come on about 3 P.M., and to have been very profound. By direction calculation for Edinburgh I find the total eclipse commenced at 3h. 3m., and continued 3m. 41s. At Inverness totality continued 4m. 32s. The after course of this eclipse was north of Frankfort on the Main and Munich, over the Dardanelles, south of Aleppo, and thence nearly parallel to the course of the Euphrates to the north-east border of Arabia. The totality was observed in the Turkish dominions according to Calvisius.

"15. The Eclipse of 1598, February 25.—Maclaurin says the memory of this eclipse was preserved among the people of Scotland, and 'that day they termed Black Saturday.' He adds:—'There is a tradition that some persons in the north lost their way in the time of this eclipse, and perished in the snow'—a statement the probability of which our experience of recent phenomena by no means tends to support. The central eclipse may be described as having passed about five miles south of Stranraer to the Bass Rock, a little south of Edinburgh, or, more precisely, over Dalkeith. Totality came on at Edinburgh at 10h. 15m., and continued 1m. 30s. The duration was the same at Douglas, Isle of Man. From the rapid motion of the moon in declination the course of the central line was a quickly-ascending one, in latitude on the earth's surface, the total eclipse passing off within the Arctic circle. Kepler must refer to another eclipse which was observed by Jessenius at Torgau, on the Elbe, though he gives the above date.

"16. The Eclipse of 1652, April 8, to which reference is also made by Maclaurin as 'still famous among the populace of Scotland, and known among them by the appellation of Mirk Monday.'—The central line passed over the south-east of Ireland, near Wexford and Wicklow, arrived on the shores of Scotland near Burrow Head, Wigtonshire, and running within a few miles from Edinburgh, Montrose, and Aberdeen, left the island at Peterhead. Greenock and Elgin would be situate near the north limit, and the Cheviots and Berwick upon the south limit of totality. The eclipse was observed at Carrickfergus, Ireland, by Dr. Wyberd. I find by direct calculation for this place that it was only just within the north limit of totality, which would commence at 10h. 8m. 30s., and continue 44 seconds. This short duration may partly explain a curious remark of Dr. Wyberd, that when the sun was reduced to 'a very slender crescent of light, the moon all at once threw herself within the margin of the solar disc with such agility that she seemed to revolve like an upper millstone, affording a pleasant spectacle of rotatory motion.' Wyberd's further description clearly applies to the corona.

"I believe it has been generally supposed that the last total eclipse of the sun visible in England was that of 1715, May 3, so well recorded by Halley in the 'Philosophical Transactions' of the Royal Society, and I was under this impression myself until, on calculating the elements of the eclipse of 1724 (May 22), observed at Paris, and by the French King at the Trianon, I discovered that before reaching France the belt of totality must have traversed the south-west of England, and it now appears that the totality did not pass by us unrecorded.

"I am indebted to the Astronomer Royal for referring me to an account by Dr. Stukeley, who observed the eclipse from Salisbury Plain. The duration of totality in that locality would be rather less than three minutes. The eclipse of 1724 is therefore the last that has been total in England, and as I have shown in a previous communication, there will be no other till August 11, 1999, and that will be confined to the south-west corner of the country."

ON PHOTOGRAPHIC IRRADIATION IN OVER-EXPOSED PLATES[*]

THE most cursory observer of any of the recent corona photographs must have remarked the apparent eating-in of the prominences over the limb of the dark Moon. A more careful examination of the photographs shows that the whole limb of the Moon is more or less eaten into, and that the indentations under the prominences are only exaggerations of a phenomenon which is present at all parts of the limb, but which varies in intensity according as the dark limb of the Moon is projected on a brighter or less luminous background.

In all over-exposed photographs of luminous objects upon a dark background, the brighter parts of the picture are found to be surrounded by a nebulous haze or border of light, which increases the diameter of the image formed by the luminous objects at the expense of those which are less luminous.

This nebulous haze has often been spoken of as "the extension of the chemical action," but without begging the question of its cause, we propose to speak of it as photographic irradiation. It has been found to vary with the time of exposure, and the relative brightness of the object and its background.

On examining the effects of photographic irradiation in a decidedly over-exposed picture, it will be seen that the nebulous fringes round luminous objects are distinctly divided into two parts—an inner and very marked border of light, following the contour of the luminous objects, and an outer and much less definite haze, thus:—

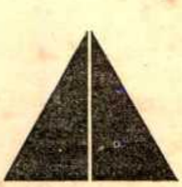

FIG. A

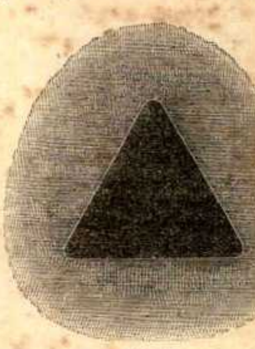

FIG. B

where Fig. A represents a normal photograph, and Fig. B a decidedly over-exposed plate from the same object.

The inner border of light fades gradually from the inside outwards, and it is very difficult, and indeed impossible, to tell where the true image of the luminous object ends, and its photographic irradiation begins. While, on the other hand, the boundary between the outer and inner fringes (or halos) of irradiation is more definitely marked, although it would be difficult to say with any absolute precision, at what point the inner fringe terminates.

Our first experiments were devised in order to test whether reflections from the back surface of the plate played any part in the production of the fringes; for this purpose plates of ebonite and the so-called non-actinic yellow glass were prepared.

In the over-exposed photographs taken on ebonite, it was found that the outer haze had entirely disappeared; while in the photographs taken on plates of yellow glass the outer haze is still distinctly to be traced, though it is much fainter than on an ordinary white glass plate with the same exposure.

By placing a piece of wetted black paper at the back of an unground plate the outer haze may be greatly reduced, while it was found that by grinding both the back and the front surfaces of a yellow glass-plate, and covering the back with a coating of black varnish, the outer haze may be rendered quite imperceptible, while, however, the inner border of irradiation still remains as before.

From these experiments we may conclude that the outer haze is produced by reflections from the back of the plate; and the action of the wetted black paper in reducing the outer irradiation may be explained by the consideration that the change of refractive index in passing from the glass to the film of water behind, is much less than in passing from glass into air. There is, consequently, less reflection at the back surface of the plate; most of the light emerges into the film of water, and is then absorbed by the black paper.

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[*] By Lord Lindsay and Mr. A. Cowper Ranyard. Reprinted from the Monthly Notices of the Royal Astronomical Society, June 14, 1872.

Fig. C represents a photograph taken upon a yellow glass plate, with a backing of wet black paper, but otherwise exposed under similar conditions to the photograph represented in Fig. B.

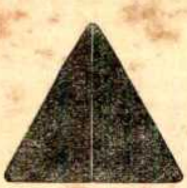

FIG. C

The outer irradiation halo may therefore be entirely avoided for the future in any corona or other necessarily over-exposed photographs by the use of the opaque plates. If, however, it is considered important that the negatives should be capable of being copied by transmitted light, the outer halo may still be to a great extent avoided by the use of transparent glass plates with a backing of wet black paper or black varnish.

Secondly, as to the inner and more definitely-marked irradiation edge which remained and seemed to be unaffected by the precautions that had served to rid us of the outer halo. Since the inner fringe was equally to be found on an opaque and on a transparent plate, we felt ourselves justified in seeking for its cause in front of the first or upper surface of the prepared plate; that is, it must be referred either to some action taking place within the thickness of the collodion, or to the optical imperfections of the instrument.

In order to determine whether the scene of action lay within the thickness of the collodion, we placed an ivory ruler with a bevelled edge in immediate contact with the collodion film. The plate with the ruler upon it was then exposed within the camera, so that the image of an incandescent platinum wire fell partly upon the collodion film, and partly upon the ivory ruler. If the scene of action lay within the collodion film, we might expect the inner irradiation fringe to extend itself under the edge of the ruler, while if it were due to the optical imperfections of the lens, the image of the wire would be cut off sharply by the edge of the ruler.

On removing the plate from the camera, and before the ruler was shifted from its place on the collodion, the whole was exposed for a few seconds to the action of the light from a gas-burner, in order that the position occupied by the edge of the ruler might be faintly printed upon the collodion film. On developing the plate, it was found that the image of the wire was sharply cut off at the place occupied by the edge of the ruler, as in Fig. D.

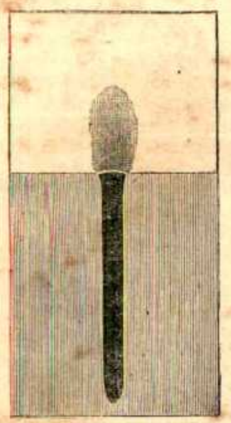

FIG. D

The very faint action extending inwards under the ruler being evidently due to the want of perfect opacity in the ivory, it seems, therefore, to be clearly proved that the inner irradiation edge is not caused by any chemical or other action taking place within the thickness of the collodion; but must be referred to the optical imperfections of the instrument which throws the image upon the collodion film.

It is instructive to remark that the photographic image of the wire is not cut off by an absolutely straight line at the edge of the ruler, but it is slightly convex, and is separated from the faint action which has apparently taken place through the ruler by a very narrow bright line, which appears to indicate the presence of a small capillary film of liquid along the edges of the ruler, forming a minute cylindrical lens. At the point where the collodion was acted upon by the light, the minute cylindrical lens appears to have been interfered with, and depressed inwards towards the ruler; we may therefore conclude that the collodion film is slightly swelled or thickened by the action of the light upon it.*

The cause of the inner irradiation-edge seems to be that every point of a luminous object is not represented by a simple point of light in the luminous image; in other words, the circle of least diffusion of any pencil is a curve of sensible area, of which the central and most intense portions imprint themselves first upon the collodion.

Some further experiments were made in order to test whether the size of the circles of least diffusion was chiefly owing to chromatic aberration (in which case the difficulty might be got rid of by the use of reflectors); for this purpose a bath of solution of sulphate of copper was placed in front of a gas-burner, and the triangular diaphragm shown in Fig. A was then placed between the sulphate of copper bath and the lens of the camera; but the blue screen thus formed seemed to have very little effect in altering the breadth of the irradiation-fringe, only slightly retarding the rate of its formation; a similar result was obtained on placing a piece of yellow glass in front of the diaphragm; in this case, however, the formation of the fringe was still further retarded.

Photographers have long known that by making use of stops they can obtain a much sharper image. By way of experiment, we cut off the edges of the lens with a circular stop, and found that the inner irradiation-fringe was thus greatly decreased. It seems, therefore, fair to argue that the aberration of oblique pencils exceeds in magnitude the other disturbing causes, and that it will be well, in making preparations for the photographic observation of the transit of Venus, to avoid as much as possible all oblique pencils.

We would, therefore, place our photographic plates in the primary focus, and thus avoid the necessarily deep curves of any arrangement of lenses which may be used for enlarging the image. Whether it would be best to make use of a reflector or a refractor, remains to be settled by further experiment, but our present experiences would lead us to vote in favour of the reflector.

We cannot conclude without returning our best thanks to Mr. H. Davis, who has rendered us able assistance in carrying out the foregoing experiments.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, February and March 1872.—This double number is unusually bulky; it contains no less than 252 pages, and abounds with much interesting matter. The effect of the new management seems to be evident, as the last paper published in this number was only received on January 14; formerly some two or three months generally elapsed from the time of the reception of a paper before its publication. The number of the papers renders it impossible in the short space at our disposal to give more than a passing glance at some of the more important. Amongst them we notice three papers in continuation of Linnemann's researches; these treat of the synthesis of normal butyric acid, of butyl alcohol, and on some of the normal butyl derivatives. The author prepared the normal butyric acid from iodide of propyl, which he obtained from the products of fermentation. This was boiled with potassic cyanide, and the product treated with alcoholic potash, yielding potassic butyrate; from this it is easy by well-known methods to prepare the alcohol and its derivatives, many of which have been carefully studied and are described in these pages.—Gorup-Besanez contributes a paper on the ozone reactions in the neighbourhood of the evaporating houses of salt springs (Gradirhausern); he finds that when large quantities of water evaporate spontaneously or in a current of air, ammonic nitrate and ozone are formed in appreciable quantities, and that the nearer to the evaporating surface the ozone paper was placed the more intense was the ozone reaction.—Schorlemmer has contributed a paper on the normal paraffines. He has examined many of them, such as pentan or amyl hydride, and hexan and heptyl hydride; he finds that by the action of chlorine on these pure bodies in each case two isomeric chlorides are obtained, and from which a primary and secondary alcohol can be produced, which yield a ketone and an acid by oxidation.—The next paper is by Patera "On the means of protecting textile fabrics, &c., from fire." The author's only objection to the use of tungstate of soda, which he considers very efficient, is on account of its price. He proposes as a substitute a mixture of four parts of borax and three parts of magnesic sulphate, which is freshly prepared and dissolved in 20 to 30 parts of warm water; the fabric to be protected is dipped in the solution, wrung out and dried. A second substitute is a mixture of ammonic sulphate and gypsum. These mixtures can be used for such things as crape, tulle, muslin, canvas, wood, and rope.—Wartha has a short note on the action of potassic hydrate on anthraquinone. He finds that at high temperatures these two react and form small quantities of alizarin.—Sintenis contributes a long and interesting paper "On our knowledge of the benzyl ethers;" and Popoff another on the oxidation of the ketones, both of which deal very thoroughly with their respective subjects.

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* We are at present unable to find any explanation of the slight apparent thickening of the end of the image of the wire where it abuts upon the ruler, but the same thickening is to be found in all the plates. It may be well to remark that it appears evident from slight indication in the negatives which it would be difficult to render in a woodcut, that the true edge of the ruler coincides with the inner side of the white line (or with the side away from the image of the wire). The convexity of the end of the image of the wire cannot, therefore, be regarded as indicating even a slight chemical encroachment. The slightly tapered appearance of the other end of the image of the wire is due to the fact that the platinum incandescent wire is cooled at its points of contact with the thick copper wires of the circuit.

THE *American Journal of Science and Arts* for June opens with a paper on the early stages of the America Lobster (*Homarus americanus* Edw.) by Mr. S. J. Smith illustrated with a plate. Dr. J. J. Woodward contributes some Remarks on the nomenclature of Achromatic Objectives for the Compound Microscope, and Prof. A. M. Mayer a description of a new form of Lantern-Galvanometer. Mr. S. W. Ford describes some new species of Primordial Fossils, and Mr. F. B. Meek some new fossils from the Cincinnati Group of Ohio; and a further important contribution to palæontological science is found in Prof. O. C. March's paper on the Structure of the Skull and Limbs in Mosasauroid Reptiles, with descriptions of new genera and species, illustrated by four plates. The new genera described are *Lestosaurus*, four species, and *Rhinosaurus*, one species. The continued articles from preceding Nos. are Prof. Verrill on Radiata from the Coast of N. Carolina, and Prof. Norton on Molecular and Cosmical Physics.

THE *Scottish Naturalist* for July consists mainly of short notes of observations and discoveries relating to Scottish Natural History, chiefly Zoology. We find also the following articles of somewhat greater length:—Description of a new Hemipter, *Anthomyia Sonchi* (the sow-thistle fly), by Mr. Jas. Hardy; on the Nest of *Formica rufa* and its inhabitants, by Dr. Buchanan White; Notes on Scottish Hemiptera, also by the Editor; On the "Yellow Fins" of the Allan-water, by Dr. W. C. M'Intosh, and the continuation of the Lists of Scottish Lepidoptera and Coleoptera by Drs. Buchanan White and Sharp.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 20.—"On the present amount of Westerly Magnetic Declination (Variation of the Compass) on the Coasts of Great Britain, and its Annual Changes." By Staff-Captain Frederick J. Evans, R.N., F.R.S.

The rapidly accelerating value within the last few years of the westerly magnetic declination over the whole area of the United Kingdom and the adjacent seas, as observed at the fixed magnetic observatories of Greenwich, Kew, Brussels, Paris, and also at Christiana in Norway, is a subject of importance in practical navigation as affecting the compass bearings derived from charts and those laid down for the guidance of pilots.

The attention of the Hydrographic Department of the Admiralty has been constantly directed to this interesting physical fact; and as the duties of Her Majesty's surveying vessels employed on our shores between the years 1866—1870 embraced nearly the whole extent of coast line, advantage was thus taken to determine, with great attention to accuracy, the magnetic declination at widely spread and favourable localities.

The observations thus made by the surveying officers of H.M. navy are given in detail, with the corrections for secular change, to Jan. 1, 1872,* for which epoch a chart of the British Islands, exhibiting the lines of magnetic declination of equal value, is also appended. By comparing these lines with the corresponding lines given in the Declination Chart for 1842·5, Phil. Trans. for 1870, art. xiv., "Contributions to Terrestrial Magnetism," No. xii., by General Sir Edward Sabine, the annual decrease of the westerly declination, in the interval 29·5 years, over various geographical districts is thus shown:—

	′
Shetland Islands and N.E. coast of Scotland, between 56th and 60th parallels	8·24
E. coast of England, between 56th and 51st parallels	7·78
S. coast of England, between 51st and 49th parallels ... . Dungeness to Scilly Islands, with the Channel Islands	7·34
(Greenwich Observatory	7·14)
Irish Channel, between 52nd and 54th parallels	7·10
Hebrides and W. coast of Scotland, between 56th and 58th parallels	6·85
Ireland, S.W., W., and N.W. coast, between 52nd and 55th parallels	6·26

It is thus seen that in the area included by the shores of the United Kingdom, the change was greater on the eastern than on the western side; as also that in the higher parallels of latitude of this area the change was greater than in the lower parallels.

By a further comparison of results as observed within the last ten to twelve years, at the same stations within the same geographical districts, the following approximate values of the present rate of annual change (westerly declination, decreasing) are obtained:—

	′
Shetland Islands and N.E. coast of Scotland..	11·2
E. coast of England (Bridlington)	10·3
S. coast of England (Plymouth)...	7·9
Scotland, W. and N.W. coasts	9·5
Ireland, S.W. coast	6·6

These values are in satisfactory accordance with those obtained in the interval (1865–71) at the following fixed magnetic observatories:—

	′	
Greenwich ...	8·33	mean annual decrease of westerly declination.
Kew... ...	8·08	" " "
Stonyhurst ...	7·85	" " "

"On the Physical Nature of the Coagulation of the Blood," by Alfred Hutchison Smee.

"On the Detection of Organic and other Nitrogenised Matter existing in the Atmosphere," by Alfred Hutchison Smee.

"Contributions to Terrestrial Magnetism, No. XIII.," by General Sir Edward Sabine, K.C.B., V.P.R.S.

The author presents this paper as the companion of No. XI. of his "Contributions to Terrestrial Magnetism," which contained the Magnetic Survey of the Southern Hemisphere from 40° S. lat. to the extreme limit towards the Southern Pole, as does the present memoir, No. XIII. of the same series, the three magnetic elements from 40° N. lat. to the furthest attained limit of the Northern Pole. In both papers the mean epoch is the same, viz., 1842·5. Where it has been possible to do so, corrections to this mean epoch have been obtained and applied to earlier and later observations.

The determinations are derived from observers of all countries, and are arranged in zones, each of 5° of lat., passing round the globe. The table thus formed contains between 3,000 and 4,000 stations at which the magnetic elements have been determined. The observers are named, and references are made to the sources from whence their observations are taken. The paper is accompanied by maps of the resulting Isogonic, Isoclinal, and Isodynamic Lines, executed at the Hydrographic Office.

"On the Law of Extraordinary Refraction in Iceland Spar," by G. G. Stokes, Sec. R.S.

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* A mean value of 19° 40′ being assumed for the westerly magnetic declination at Greenwich Observatory for this epoch.

It is now some years since I carried out, in the case of Iceland spar, the method of examination of the law of refraction which I described in my report on Double Refraction, published in the Report of the British Association for the year 1862. A prism, approximately right-angled isosceles, was cut in such a direction as to admit of scrutiny, across the two acute angles, in directions comprising respectively inclinations of 90° and 45° to the axis. The directions of the cut faces were referred by reflection to the cleavage planes, and thereby to the axis. The light observed was the bright D of a soda-flame.

The result obtained was that Huyghens's construction gives the true law of double refraction within the limits of errors of observation. The error, if any, could hardly exceed a unit in the *fourth* place of decimals of the index, or reciprocal of the wave-velocity, the velocity in air being taken as unity. This result is sufficient *absolutely to disprove* the law resulting from the theory which makes double refraction depend on a difference of inertia in different directions.

I intend to present to the Royal Society a detailed account of the observations; but, in the meantime, the publication of this preliminary notice of the result obtained may possibly be useful to those engaged in the theory of double refraction.

PARIS

Academy of Sciences, July 8.—M. Becquerel presented a memoir on the influence of pressure upon the phenomena of endosmose and exosmose —M. E. Becquerel presented a report upon the recent memoir by MM. F. Lucas and A. Cazin on the duration of the electric spark.—M. Wurtz communicated a note by M. G. Salet on the primary spectrum of iodine —M. H. Sainte-Claire Deville presented a note by M. L. Cailletet on the compressibility of liquids under high pressures, giving the coefficients of compressibility of various fluids at certain temperatures and pressures, and describing the apparatus by means of which these results were obtained.—M. Flammarion presented some remarks on a part of a recent note by M. de Fonvielle on some observations made during the ascents of the balloon Lea, relating especially to the halo observed round the shadow of the balloon, and accepting the explanation of M. Tissandier.—M. Becquerel presented a memoir on some effects of slow actions produced during a certain number of years. In this paper the author described certain products, having their analogues in nature, formed by slow action in a vessel hermetically closed for twenty years. They include crystals of arragonite and of rhombohedric carbonate of lime, crystals of arseniate of lime, glauberite—with potash instead of soda, crystals of carbonate of lead, and malachite.—M. T. Schlœsing presented a second note on the solution of carbonate of lime by carbonic acid, and M. Wurtz a note by M. C. Lauth in reply to a recent note by MM. Girard and De Laire on the manufacture of aniline colours. —M. Bardy also forwarded a note on the last-mentioned subject. —Analyses of a new variety of amblygonite from Montebras, of amblygonite from Hebron in Maine, and of wavellite from Montebras, by M. F. Pisani, were communicated by M. H. Sainte-Claire Deville.—M. Wurtz communicated a note by MM. C. Friedel and R. D. Silva on a third bichlorinated propylene.—M. E. J. Maumené presented a memoir on two new acids produced by the oxidation of sugar, in illustration and support of his general theory of chemical action.—M. Balard communicated a note by M. J. Riban on the aldanes, or aldehydes condensed with elimination of water, the agents employed by him for the removal of the water being sodium or zinc. For these bodies he proposes the name of aldanes. —A third part of MM. Berthelot and Longuemine's thermochemical researches upon bodies formed by double decomposition was read. The substances experimented on were protochloride, perchloride, oxychloride, and protobromide of phosphorus; and the results obtained by treating these bodies with water and with potash are here stated.—M. C. Bernard described the evolution of glycogene in the eggs of birds, in continuation of his previous communications on glycogenesis in animals.—An extract from a letter of the Abbé David to M. Milne-Edwards containing some zoological observations made in the province of Tché-Kiang, was read. The author notices a new species of *Ibis* (*I. sinensis*), a new Falcon (*F. sacroides*) a new *Elanus* (*E. sinensis*), and a new Salamander of the genus *Cynops* (*C. orientalis*). He also mentions the occurrence of some other birds, and of a great freshwater tortoise attaining a weight of 200 to 300 pounds, supposed to be *China indica*.—MM. Jamin and De Laurès presented a note on the alterations of weight undergone by the human body in baths, in which they confirm the results of M. Durrien, according to which the weight of the body is maintained or increased by absorption so long as the temperature is low or moderate, but diminished by immersion in warm water.—M. Bernard presented a fifth note by M. Paul Bert on the influence exerted by changes in barometric pressure upon the phenomena of life.—MM. P. van Tieghem and G. Le Monnier presented a joint note describing the zygospores of *Mucor phycomyces*; and M. Duchartre a paper by M. Duval-Jouve on a new species of the genus *Althenia* (*A. Barrandonii*) from the south of France.—M. Milne-Edwards communicated a note by M. H. Filhol on the carnivora and chiroptera, of which the fossil remains are found in the deposits of phosphate of lime at Caylux, Fregols, and Concots. The author describes the jaw of a cat, which he names *Pseudælurus Edwardsii*; a jaw serving as a link between the cats and mustelidæ, upon which he founds a new genus, and which he names *Ælurogale intermedia*; and two jaws of dogs, described as *Canis caylucensis* and *C. Gaudryi*. At Fregols there is a breccia composed entirely of the bones of bats, which the author refers to *Rhinolophus*, under the name of *R. antiquus*.

PAMPHLETS RECEIVED

ENGLISH.—Proceedings of a Joint Meeting of the Malvern, Bath, and Woolhope Field Clubs —Discussion of the Anemometrical Results furnished by the Anemometer at Sandwick Manse, Orkney, 1863-68 —On the Law which regulates the frequency of the pulse: A. H. Garrod —Report of the Winchester and Hampshire Scientific and Literary Society, 1870-71 —Journal of Mental Science, July —Memoirs of the Geological Survey of England and Wales —Man in the Crag —Scottish Naturalist, July.—Quarterly Journal of Microscopical Science, July —Naval Science, No. 2 —Quarterly Journal of Science, July —The Glacial Geology of Lancashire and Cheshire: T M. Reade —Vaccination and the Vaccination Laws: Rev. W H. Rothery —Journal of the Society of Telegraph Engineers, No 1.—On the Change of Climate during the Glacial Epoch . Jas Geikie.—Introductory Lecture delivered at the University of Glasgow A Dickson —Route for Steamers from Aden to the Straits of Sunda —On the Winds of the North Atlantic —Strictures on Darwinism H H Howorth Part I —Extracts from the Opening Address of the President of the Botanical Society of Edinburgh: Sir W. Elliot.—Annual Address to the Victoria Institute: Rev J Kirk.—Explosive Agents applied to Industrial Purposes . F A. Abel.—Remarks to accompany the Monthly Charts of Meteorological Observations —Grevillea, No. 1 . M. C. Cooke.

AMERICAN AND COLONIAL.—Die neue entdeckte Geyser-gebiete am Oberen Yellowstone u Madison Rivers . F V. Hayden —Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College. No. 6, T. Lyman—Annual Report of the Trustees of the Museum of Comparative Zoology at Harvard —The Eozoon Limestone of Easteren Massachusetts J B Perry.—Fifth Annual Report to the Trustees of the Peabody Institute, Baltimore —On the Structure of the Skull of Mosasauroid Reptiles . Prof. O. C Marsh —Preliminary description of *Hesperornis regalis*: Prof. O C. Marsh —Statement relating to the Home and Foreign Trade of Canada W. J. Patterson.—Monthly Record of Results of Observation in Meteorology and Terrestrial Magnetism at Melbourne, March 1872 —Supplement to Fifth Annual Report of the U S Geological Survey of the Territories: F. V. Hayden —Popular Science Monthly, Nos 1-3.—Mineral Statistics of Victoria, 1871.

FOREIGN.—Sulla determinazione dei pesi molecolari delle sostanze saline . Dr E. Paterno —Osservazione dell' eclisse totale del 12 Dec , 1871, a Poodacottah nell' Indostan: Prof L Respighi —Zeitschrift fur Ethnologie, 1872. No. 3 —Journal d'Anthropologie, 1872. No. 3 —Cronica Scientifica: P. Tacchini

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ERRATA.—Vol vi , p. 221, col. 2, line 34 from bottom, for "Chladin," read "Chladni;" and p. 222, line 22 from top, and also in Contents, for "Allen," read "Alexander."

THURSDAY, AUGUST 1, 1872

DR. LIVINGSTONE

THE publication of two letters in the *New York Herald* from Dr. Livingstone has thrown some new light upon the discoveries on which the famous traveller has been engaged since 1867. The letters purport to have been written by the great traveller himself, but they bear unmistakeable marks of having been manipulated to suit the tastes of the readers of that very sensational newspaper. Yet, until the traveller's own journals are before the world, we must be content to gather as much information as may be picked up from this source, doubtful though it be.

These discoveries include the great mountain range separating the drainage of the Zambesi from that to the northward; a great valley receiving numerous streams, which Livingstone believes to be the true sources of the Nile; and a beautiful lake, called Liemba, which appears to form the southern extreme of Tanganyika. But the chief interest centres in the great valley commencing south of Lake Tanganyika, from which it is completely separated by intervening hills, and then turning to the north and west. It receives a vast quantity of rain, and appears to be subject to inundations. Its river, from its source in the southern mountains to Lake Bangweolo, is called the Chambese. Thence it turns due north, and flows, under the new name of Luapula, past Cazembe's town—first visited by the Portuguese—into Lake Moero. The great river then forces its way northward through the mountains of Rua, under the name of Lualaba, and spreads out into a vast lake named Ulenga, or Kamalondo, in the Manyema country. It then takes a westerly, and for a time even a southerly course, under the name of Lufira, spreading out into a lake called Lake Lincoln by the explorer; which also receives another important feeder from the mountain range to the south, called Lomame. Finally, the now mighty river turns to the north and enters an unknown land; for this was Livingstone's farthest point. But he heard that it flows into another unvisited lake, called Chowambe, and he believes it to be the Nile.

The discovery of this valley for an extent of some 700 miles, with its great river, receiving numerous affluents and flowing through four great lakes, has occupied Livingstone for the last five years. During 1867 and 1868 he crossed the mountains from the Zambesi valley, visited Cazembe, followed the river through two lakes, and traced it until it passed into the gorge of the Rua mountains. He then turned aside to the Tanganyika lake, to pick up the supplies that he expected to find at Ujiji, on its banks. His last letter was dated from Ujiji, on May 30, 1869. From Ujiji he set out to complete his work by connecting the Lualaba, where he left it in the mountains, with Baker's lake. But this expedition seems to have been a failure. He indeed crossed Lake Tanganyika again, penetrated into the Manyema country north of the Rua mountains, and traced the great river for some distance farther, and through two lakes, until he found it to be flowing due north. But here his men became mutinous, and he was obliged to return to Ujiji last year disheartened, and sorely in want of succour and fresh supplies.

That succour was at hand. Never has traveller been so keenly watched by those at home; never has assistance been forwarded with such lavish generosity. Fortunately Livingstone's old friend and fellow-traveller, during six long years of hardship and anxiety, Dr. John Kirk, had been appointed resident medical officer at Zanzibar, and he has superintended the measures for the explorer's relief with affectionate and untiring zeal. The first supplies, however, which Livingstone found at Ujiji in 1869, were sent up by Dr. Seward, Kirk's predecessor. Kirk sent up a second supply, while Livingstone was in the Manyema country, a great part of which was stolen by the men in charge. The mistaken policy of entrusting these supplies to natives was not Dr. Kirk's, but was apparently adopted under orders from the Foreign Office. A third large instalment of supplies was sent up, and Kirk zealously superintended its despatch from the mainland at Bagamoyo. It safely reached Unyanyembe, and has been of the greatest service to Livingstone. But the Geographical Society, and the people of England, were not satisfied with these measures. The great Explorer had not been heard of since May 1869, and an expedition was resolved upon to seek him out, and relieve his necessities. Liberal subscriptions, amounting to upwards of 5,000*l*., poured in, and, as is well known, the Expedition sailed for Zanzibar under the brightest auspices last February. Thanks to the hearty and zealous co-operation of Dr. Kirk, the equipment was completed on the 27th of last April, and the members of the Expedition were on the mainland, and ready to start for the interior. Had it not been for an unforeseen intrusive element, in the shape of the Correspondent of a sensational American newspaper, all would have gone well, and the Explorer would by this time have been fully furnished forth with all necessary supplies and instruments, and with assistance which would have ensured the verification and completion of his discoveries. We cannot but feel that the members of the Expedition committed a very grave error in judgment in abandoning their work on very insufficient grounds.

It seems that the editor of the *New York Herald*, in looking about for fresh startling sensations wherewith to feed the appetites of his public, turned his attention to Livingstone and his discoveries, and despatched a correspondent to "interview" the great traveller, and so furnish new material for those large type headings and wonderful paragraphs in which that well-known paper delights to indulge. So far no harm had been done, except that a *New York Herald* "Correspondent" was the very worst messenger that could have been selected. For it was to his interest to keep all he had done, all Livingstone had told him, a close secret until a wondrous version of it could appear in New York. Carefully concealing his object while at Zanzibar, the correspondent advanced into the interior, found Dr. Kirk's ample supplies waiting at Unyanyembe, and after some difficulties caused by his own mismanagement, reached Ujiji, where he found Dr. Livingstone. There is some mention of Mr. Stanley, the correspondent, having accompanied Livingstone on an excursion to the northern end of Lake Tanganyika; but this statement has not yet been corroborated by the great traveller himself. Livingstone then proceeded to Unyanyembe, which is about a third of the way from Lake Tanganyika to the coast, where he found the supplies sent up to him by his old fellow-traveller, Kirk. Here he awaits further supplies, before setting out on a fresh expedition of discovery; while the correspondent set out for the coast, after having "interviewed" the great traveller to his perfect satisfaction, and having obtained material for a whole series of sensational articles.

Mr. Stanley certainly did useful service, which deserves acknowledgment, in passing on from Unyanyembe to Ujiji, and announcing to Livingstone that the supplies were waiting for him at the former place. But this service has been marred by his subsequent conduct. His duty to his employers obliged him to keep Livingstone's countrymen at Zanzibar in as much ignorance as possible, and to withhold all information; and it is for his employers, not for Livingstone's countrymen, to thank and reward him. But how is it that the lonely traveller had his mind poisoned against his warmest and truest friend, who had used every means to send him help, and through whose exertions Livingstone had actually been put beyond immediate want at Kazeh? How is it that the ungrateful message was imputed to Livingstone, that he wished all relief expeditions to be turned back? How is it that one of Her Majesty's Consuls, the great enemy of slavery, is stated to have sent down to Zanzibar for slave chains? How is it that Livingstone's letters to his friends are still detained by him to whom they have been entrusted? None of these acts were obligatory, as regards duty to the New York employers. Judging him even by his own lights, the "Correspondent" has exceeded his duties to his masters, and has proportionately injured, unnecessarily, the great traveller out of whom capital was to be made. Mr. Stanley's secrecy, and refusal to give any information concerning Livingstone and his wants, to his countrymen at Zanzibar, has been most injurious to the great traveller's interests; while the system he is now pursuing of withholding Livingstone's private letters to friends, and even his despatches to the Foreign Office, is most unjustifiable.

We must repeat that the abandonment of the Relief Expedition, on the ground that its work had been anticipated, was a very serious, and may become a very fatal, mistake. The correspondent's secret proceedings ought not to have influenced the open and clearly-marked course of the Expedition in any way. Their duty was to relieve and assist Livingstone, and nothing should have turned them from it. As it is, only a party of fifty men, commanded by an Arab, has been sent up to Livingstone, with stores, arms, and other equipments entirely supplied from the funds of the English Search and Relief Expedition. But, in a letter dated June 3, unfavourable reports have been received of the character of the man who commands this party, and it may never reach its destination.

Dr. Livingstone, it is stated, intends to continue his travels for two years longer; but it has not yet transpired in what direction he will turn. He will probably endeavour to complete the examination of the great river which he believes to be the head stream of the Nile; or he may turn his steps south, as we conjectured in a former number, and solve the interesting geographical question connected with the drainage of Lake Tanganyika. He still has a vast field of discovery before him, and his countrymen will continue to watch his proceedings with warm sympathy and interest.

NICHOLSON'S INTRODUCTION TO BIOLOGY

Introduction to the Study of Biology. By H. Alleyne Nicholson, M.D., D.Sc., &c. &c., Professor of Natural History and Botany in University College, Toronto. (Blackwood, 1872.)

THIS book is an attempt to give a general view of the phenomena manifested by living beings, and to form a sort of basis for a more detailed study of some special branch of biology. It commences with an account of the differences between living and non-living matters, and with a discussion of the nature and conditions of life; then the distinctive peculiarities of animals and plants are considered, and the principles of biological classification laid down. There follow short chapters on the elementary chemistry of living beings, on the chief physiological functions, and on the varieties of the developmental process; and disquisitions on spontaneous generation on the origin of species, and on distribution in space and time complete the volume.

In his preface the author states that his work is intended to be elementary, and useful at the same time both to the student and the general reader. This double object he can hardly be said to have succeeded in attaining. The book throughout is just one to interest the non-scientific general reader, but not one which can be recommended as fitted to lay a sound basis of biological knowledge in the mind of a student. Instead of describing typical instances minutely, and from them deducing the laws of life, the author, with few exceptions, deals throughout in generalities. Protoplasm, for example, is described, but no detailed account is given first of such a body as an amœba, or a white blood corpuscle, which would be much more fitted to leave on the mind of a beginner a clear and definite idea of the nature and properties of protoplasm than would any abstract account of its characters in general. So, again, no typical animal or plant is described in detail; but there is a chapter on the general differences between animals and plants, and, as scarcely any character of either can be mentioned to which there are not exceptions, the result of this method can only be to produce a very dim and confused state of mind in one new to the subject.

There are, however, worse faults than this in the book. There is a general retrograde tendency in it towards the point from which physiology has of late years been progressing—that of considering the actions manifested in living bodies as due to a source of energy essentially different from that of all other actions. A curious instance of this is found in the second page of the book, where among the differences between dead and living bodies, the author cites the fact that all the actions of living bodies are accompanied by a corresponding destruction of the matter by which these actions are manifested; of course, by destruction here he can only mean the transformation of the matter into another form, which is exactly what occurs in thousands of cases in non-living sources of force—as when a steam-engine is moved by the combustion of the coal in its furnace; and instead of being a difference between dead and living bodies, is a remarkable instance of similarity, and one which, a few pages farther on, Dr. Nicholson seems entirely to forget. Again, "Dead matter is completely passive, unable to originate motion; living matter is the seat of energy, and can overcome the primary law of the inertia of matter." This point of difference is, to say the least, by no means proven. In a certain sense dead matter cannot originate motion, it can only convert some other form of force into it; but there are very good grounds for supposing that when an animal moves its limb, and so originates motion, it does exactly the same thing: at any rate, no one is justified in making the express statement that it does not; if so, indeed, where is the need for "that destruction of matter" which accompanies vital actions, or why should an animal or plant need food at all?

Protoplasm, the author states, may be regarded as a general term for all forms of albuminoid matter—an extension of the meaning of the word which is certainly not justifiable. Few would be inclined to call the boiled white of an egg, or coagulated fibrin, protoplasm. Yet upon this definition of the word Dr. Nicholson bases an argument against the theory of those who maintain that life is one of the properties of protoplasm. He represents them as asserting that life is the "result of the combined properties of the elements which form albuminous matter," and then brings forward the fact that dead albuminous matter exists, as an argument against the truth of this statement. What is really asserted is that life is a property of protoplasm, and that protoplasm is nitrogenous compound related in chemical composition to albuminous bodies; but it would be as reasonable to call starch and dextrine by the same name because one is readily converted into the other, and because they have a similar chemical composition, as to call all albuminous matters protoplasm. But even were it admitted that protoplasm, as such, can exist in a dead state, to deny that life, under other conditions, can be one of its properties, is to deny altogether the possible existence of allotropic states of any kind of matter whatsoever; and this is what Dr. Nicholson practically does. He says, moreover, that as water is a definite chemical compound, with universally the same properties, there is no need for ascribing its properties to any assumed principle of aquosity; but that, as living protoplasm has certain properties different from those of dead albuminous matter, it is best to regard *vitality* "as something superadded and foreign to the matter by which vital phenomena are manifested." But, admitting for the moment that albuminous matter and protoplasm are convertible terms, would the author assume a principle of *phosphorosity* to account for the different properties of yellow and red phosphorus.

The author falls into the very general error of stating that plants can only build up protoplasm in the light; but certain fungi will flourish in Pasteur's solution, although kept in total darkness. The difference as regards food between animals and plants is stated in a manner calculated to mislead a beginner. It is hardly correct to say that the food of plants consists of inorganic compounds; plants have the power of building up food out of these compounds, and it is this which distinguishes them from animals; the essential nutritive processes of each are the same, and consist in the breaking up of the unstable compounds thus formed; and the nutrition of plants cannot, as stated, be narrowed to the question of the modes and laws by which these unstable organic compounds are built up.

The chapters on Classification and Homology are good; in fact, throughout the book morphological questions are much more ably treated than physiological. The account given of the chemistry of living beings is very bare. Fats are the only non-nitrogenous compounds mentioned as occurring in animals. Starch is rendered conspicuous by having the only chemical formula in the book attached to it, and that on the old system of notation.

The next chapter is one on "The Elementary Structure of Living Beings," and here the author closely follows Dr. Beale, accepting his views as to "germinal matter" and "formed material." In speaking again here of protoplasm (or as he, following Beale, prefers to call it, bioplasm), the word "contractility," as applied to the cause of amœboid movements, is objected to on the curious ground that it implies an identity in kind with the contractions of a muscle, an identity which most physiologists, we think, would readily admit. The accounts of development and reproduction are much better than the earlier parts of the book. In his account of the origin of species, the author simply states the opposing views on the subject, and the chief objections to them, but expresses no opinion of his own; in fact, for some reason, he seems desirous to be regarded as having none, for in a foot-note he asks it to be remembered that a statement of each side of the case commits him to neither. "Distribution in Space" is the heading of an interesting chapter, but that on "Distribution in Time" is very imperfect, at least in a biological sense; it contains an epitome of geology, but such questions of great interest as the succession of life on the globe, or the extraordinary persistence of certain species, as Nautilus and Lingula, are entirely ignored.

The book contains a considerable number of woodcuts, some of them very good; but on the whole it is not one to be recommended as a safe guide to the acquirement of a firm foundation of biological knowledge.

OUR BOOK SHELF

Natural History of the Year. By the late B. B. Woodward, B.A. (London: S. W. Partridge.)

It does not often fall to our lot to notice a book of this description which we can so heartily commend. The design is to interest young people in the varied productions of Nature, by taking them into the fields and woods from month to month, and pointing out the numberless objects of interest that will meet the observant eye at every turn. This is all told in a pleasant manner, and withal with a religious spirit. The author was himself a keen and accurate observer of Nature; and we do not meet with those atrocious blunders with which books intended for the instruction of children too often abound in describing the most familiar things. The book is illustrated with some extremely tasteful illustrations, one for each month, and forms altogether a most attractive gift-book for an intelligent child.

Annual Record of Science and Industry for 1871. Edited by Spencer F. Baird, with the assistance of eminent men of science. (New York: Harper and Brothers, 1872.)

THE Americans are more fortunate than ourselves in possessing a Year-Book of Science edited by a scientific man whose name is a guarantee for the accuracy and value of its contents. The various items of information are arranged under thirteen heads, viz. (1) Mathematics and Astronomy; (2) Terrestrial Physics and Meteorology; (3) Electricity, Light, Heat, and Sound; (4) Chemistry and Metallurgy; (5) Geology and Mineralogy; (6) Geography; (7) General Natural History and Zoology; (8) Botany and Horticulture; (9) Agriculture and Rural Economy; (10) Mechanics and Engineering; (11) Technology; (12) Materia Medica, Therapeutics, and Hygiene; and (13)—a very small one—Miscellaneous, with a very brief Necrology appended. To each paragraph is added the indispensable reference to the authority. Prof. Baird's position as Secretary of the Smithsonian Institution at Washington has given him unusual facility for consulting all the leading magazines, and other scientific publications of Great Britain, France, Germany, Holland, and America, of which he has availed himself to the full. An account of the discoveries made in Italy, Sweden, Russia, and other countries of Europe, some of which are daily rising into more and more importance, has been obtained only second-hand. The whole is prefixed by a general Summary of Scientific and Industrial Progress for the year 1871, occupying sixteen pages, wherein the more noteworthy incidents in each department of science are briefly chronicled. The work is the result of great labour; and, as far as we have been able to test it, presents a very fair and accurate record of the progress of science during the year. To those who desire to possess such a record for handy reference, we can confidently recommend Prof. Baird's Year-Book as the best and most complete work in the language, and decidedly preferable to anything published in this country.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Atmospheric Effect

AN atmospheric effect, which is sometimes observed in England, displayed itself here in great beauty yesterday. The western sun had been cut off from us by an intervening ridge, while the upper atmosphere was still filled with his light. There was a good deal of opalescent haze in the atmosphere, which, had the sun shone upon it uniformly, would have presented a tolerably uniform hue. But besides the haze, small detached clouds floated in the air, and behind each of them was a sheaf of shadow, drawn through the haze. The density of these shadows varied with that of the clouds which produced them, nor was the density uniform for all parts of the transverse section of the same shadow. The parallel bars of graduated shade thus produced converged, through an effect of perspective, to a point *in the east*, exactly as if the sun were going to rise there. The display of the convergent glory was strikingly beautiful.

The same effect showed itself at Oran during one of the evenings spent there by the Eclipse Expedition. I have seen it two or three times in England (always, I think, near the coast), the last occasion being in company with Mr. Hirst at Margate.

Faraday, if I remember aright, has described this effect. There was also, I believe, a question asked regarding it some months ago in NATURE. This brief account may interest the questioner.

Bel Alp, July 22 JOHN TYNDALL

Spectrum of Aurora

IN the article on the Aurora Borealis of Feb. 4, a translation of which appears in your issue of April 25, Prof. Respighi mentions having noticed the green line of the aurora when observing the zodiacal light; also, that this line was visible—more or less defined—from horizon to zenith, in every part of the heavens.

In 1867, when Angstrom observed the green line in the spectrum of the zodiacal light, he also detected its presence in all parts of the sky. From his own and Angstrom's observations, the Professor demonstrates the identity of the zodiacal light and the aurora. This appears to me, at the least, premature. Had the spectrum appeared only where the zodiacal light was perceptible to the naked eye, there would have been reason for believing it due to that light; but, be it noticed, the green line is everywhere seen as bright as in the zodiacal light itself. We have only to suppose that in both cases auroral phenomena, imperceptible to unaided vision, were present, and the spectra seen by Angstrom and Respighi are at once accounted for.

With all due deference to the great authorities just named, I may state that at Mr. Lockyer's request I have been observing the zodiacal light with a spectroscope since last December, and brilliant as the phenomenon has frequeently been, I have hitherto failed to detect the slightest appearance of bright lines or bands. A faint diffuse—to use Capt. Herschel's nomenclature—spectrum, about as intense as that of a bright portion of the "milky way" is all I have yet obtained.

The spectroscope used is one of Browning's 5 prism ones.

G. H. PRINGLE

Camp Charmadi, South Canara, June 23

Kinetic Energy

IF the loss of kinetic energy in water which has flowed from lower to higher latitudes is due to friction, and represents work consumed in overcoming friction, as Mr. Croll maintains, how is the gain of kinetic energy in water which has flowed from higher to lower latitudes to be accounted for?

Mr. Croll's answer to this question will be awaited with interest. J. D. EVERETT

Belfast, July 27

Bree on Darwinism

I AM very much obliged to Mr. Alfred R. Wallace for pointing out some errors of the press, and some of hasty writing which were not corrected in the proofs, of my work upon the "Fallacies of Darwinism."

I do not think, however, he has adduced anything which justifies his virulent condemnation of a work which he has not ventured to criticise, and I do not, however, write for the purpose of making any complaint. I hope to have the opportunity of answering his remarks upon another occasion.

My object in writing is, however, to request you will give me the opportunity of pointing out that no blunder which I have made is so great as that committed by Mr. Wallace himself, when he states that Darwin's imaginary human ancestor with cocked ears and a tail should have been evolved after the incoming of catarrhine monkeys, which creatures, by what I presume some might call anticipatory retrogression of development, have actually been placed by Mr. Darwin in the human pedigree, and separated from their congeners the platyrrhine or new-world monkeys, because they had so far reached the human goal to which they were tending as to lose their tails.

Also according to Mr. Wallace the cocked-eared creature must have not only re-evolved a tail, but have gone so far backward as to lose the human-shaped, and gain the canine-like "cocked ear." C. M. BREE

Colchester, July 27

Volcanoes and Earthquakes

IT is generally admitted that an earthquake is due to the passing of a sensible wave through the earth's crust. It has also been observed that the occurrence of earthquakes is generally associated with eruptions from volcanic vents, usually in the neighbourhood, but not unfrequently at considerable distances. Now it is evident—and it has struck all observers—that there must be some connection between the two classes of phenomena. The nature of this connection has been differently explained by different writers. But the purpose of this note is not to criticise existing theories, but to propose one, which I believe to be new, and to be capable of explaining why a sudden volcanic eruption must ordinarily be accompanied by earthquake shocks of greater or less violence (not necessarily always sensible), and why earthquakes may occur without any contemporaneous outburst.

In the preface to his "Physikalische Geologie," Bischof suggests that the phenomena observed in the laboratory should be taken as our guides to explain what happens in nature. Let us see, then, if in the laboratory we meet with any phenomena analogous to volcanoes and earthquakes.

When a reaction has to be performed in a sealed tube, and it is expected that much gas will be evolved, and consequently the pressure in the tube be much increased, it is one of the commonest precautions to draw out the tube to a capillary orifice before closing it. When this precaution has been neglected, and even although the point be allowed to blow itself out in the flame of a lamp, an explosion not unfrequently attends the attempt to open it. Let us consider the circumstances.

We have a tube whose walls are being pushed out by a very high pressure from within, which, however, it resists; but at the moment that this pressure is suddenly relieved at one point, the tube bursts. What is the cause of the explosion? It clearly cannot be the mere reduction of pressure. As long as the pressure was equally distributed over the walls of the vessel, we have seen that it was successfully resisted; as soon, however, as it was suddenly relieved at one point, a great inequality in the tension of the gas in the immediate vicinity of this point would be the result, the gas immediately at the opening assuming at once the atmospheric pressure, while that at, say, the eighth of an inch from it is at the tension of the gas in the tube. The practical effect of this sudden inequality of pressure would be to produce a tug on the mass of elastic fluid, which would cause the walls momentarily to tend to collapse, and this tendency to collapse would be transmitted through the glass as a wave. This wave would to a certain extent distort, and therefore weaken, the walls; and consequently, if the pressure inside were great enough, it would burst them; if not, the only effect would be that a shock would traverse the walls of the vessel, and the pressure would relieve itself by the orifice.

Now, suppose the vessel to be a subterraneous cavity containing an atmosphere of very great tension, and that suddenly the envelope gives way at one point, what will be the result? Just as in the case of the glass tube, the sudden relief of pressure will, in the way indicated above, cause the walls to experience a momentary collapsing impulse, which will be propagated as a wave until extinguished by the imperfect elasticity of the crust. The sudden outburst will be a volcanic eruption, and the consequent collapsing shock will be an earthquake, which either will or will not be accompanied by rending of the crust, according to the strength of the walls and the greatness of the pressure.

It is, however, not necessary that there should be a visible volcanic eruption. For, suppose two such subterraneous cavities at different pressures, separated from one another by a wall weaker than that which separates either of them from the outside of the earth; then, if the pressure in the one becomes so great as to burst the barrier between the two, the result will be an earthquake. And similarly, the pressure in the two thus united cavities may go on increasing until they burst into a third, and so on until they come to a vent, which is either open or weak enough to yield to the pressure. In this way an earthquake and an eruption may be in intimate connection with one another, although a considerable interval of time intervene between the occurrences, and the localities affected be at great distances from each other. And it is possible that some connection of this kind may have existed between the earthquake of Antioch and the eruption of Vesuvius, both having been extreme in their violence. Indeed, the whole series of disturbances, commencing with the earthquake in California and terminating with the eruption of Vesuvius, noticed by Mr. Corfield in NATURE of May 23, may possibly find an explanation under this theory.

The effect of sudden relief of pressure in weakening the walls of vessels explains many cases of explosion which otherwise appear anomalous. Thus, high-pressure boilers have been frequently observed to burst at the moment when the engineer turns on the steam.

In conclusion, the above sketched theory assumes nothing but what we know to be fact. We know that, at least in the neighbourhood of volcanoes, there must be subterraneous cavities whose atmosphere is at an exceedingly high-pressure, for we not only see it emitted from the vent, but it projects enormous masses of rock high into the air, thus testifying to the energy with which it was endowed. Further, given this high-pressure atmosphere, it is certain that, on its being suddenly relieved, it would communicate a shock to the crust, and this, on being felt outside, would be described as an earthquake. I think it is therefore clear that some earthquakes must be produced in this way. Of course this does not include the possibility of there being other causes of concussion which might produce similar effects.

Edinburgh, June 10 J. Y. BUCHANAN

On the Cohesion of Figures of Creosote, Carbolic and Cresylic Acids

WITH reference to the note by Mr. J. H. Spalding contained in NATURE of June 13, I am reminded by my friend Mr. Rodwell that some five or six years ago I showed him the cohesion figure of carbolic acid. A crystal of this acid was taken up on the end of a platinum spatula, and gently delivered to the surface of clean water contained in a clean glass; the crystal gave a few jerks, then suddenly liquified, and displayed its highly characteristic figure so well described by your correspondent. I may further remark that I showed this figure in the chemical section of the British Association, at Manchester, in September 1861, and a drawing of it is given in the plate which accompanies my paper in the *Philosophical Magazine* for October of that year. In this paper it is described as "an exaggerated form of the figure of creosote; the water seems to tear it to pieces; the crispations are amazingly active, and the disc quickly breaks up and disappears. Indeed, while a drop of creosote will endure five minutes on the surface of an ounce of distilled water in a small glass, a drop of carbolic acid will last only a few seconds on the same quantity of water. The cohesion figure is however quite characteristic of the substance, and cannot be for a moment mistaken for any other substance that I have examined "

Creosote, carbolic and cresylic acids, and newly-distilled oil of cloves, give remarkable figures of the same type, each of which is characteristic of the substance.

Mr. Spalding remarks that warm water destroys all action, by lessening, as he supposes, the adhesion of the liquids. I am sorry to have to object to this remark, but I have no doubt that Mr. Spalding was led to make it by employing unclean water or an unclean recipient. If distilled or even ordinary tap water be heated over a spirit lamp in a clean flask, and be poured into a clean vessel, the surface of the water is active at all degrees between the temperature of the air and just below boiling. On the surface of cold water a drop of creosote passes through the following changes:—(1) As soon as the drop is placed on the surface of the water the figure is formed for an instant; (2) it splits open and forms a kind of brittle arc, which (3) is shivered into a number of separate discs, each of which is a perfect cohesion figure of creosote. These figures perform their evolutions independently of each other, sailing about with rapidity, but never clashing with or disturbing each other. In the *Philosophical Magazine* for June 1867, figures are given of these different phases of the figure.

Now, if the water be heated to 100° Fah., or from that to 150°, a drop of creosote deposited on its surface produces a good active figure, but it does not split open or form the brittle arc above referred to; it sails slowly over the surface, firing off volleys of small globules in radial lines, and only when much wasted does it split into smaller systems. All this is what might be expected from the diminished surface-tension occasioned by the heat. Indeed, it is a beautiful illustration of the slight diminution of surface-tension in hot water as compared with cold. In consequence of not splitting open, the duration of the figure is greater on the surface of hot water as compared with cold.

Carbolic and cresylic acids are also very active on hot water, at all temperatures up to 210° Fah. C. TOMLINSON

Highgate, N.

Hereditary Instinct

WILL you allow me to recount to your readers what appears to me to be a striking instance of the transmission of impression in animals?

A few years ago I bought in Skye a perfectly uneducated Skye terrier. The first accomplishment which I taught him was that of "sitting up"—an accomplishment which he had great difficulty in acquiring. This was not owing to any stupidity on his part, for when he had once passed over this *pons asinorum* of dog-performances, he proved to be a very clever animal, and learnt many other tricks with great ease. He appears, however, never to have forgotten the pains which were taken to teach him his first trick, and to have judged therefrom that there is great merit in sitting up. Not only does he rely upon this as a last resource to move me to take him out, or not to whip him, but he judges that it must soften even the heart of an india-rubber ball. Sometimes when annoyed at his playing with this, his favourite toy, I have placed it on a chimney-piece, and turned my attention elsewhere. On looking round again I have seen my dog sitting up to the india-rubber ball, evidently hoping that it would jump down and play with him again. Perhaps he looks upon this ball as "animated by a living essence" (*vide* Chap. ii. of Darwin's "Descent of Man").

My dog is now the father of a family, and one of his daughters, who has never seen her father, is in the constant habit of sitting up, although she has never been taught to do so, and has not seen others sit up. She is especially given to this performance when any other dog is being scolded. Whether this is an instance of helping a fellow animal, of which Mr. Darwin gives such curious examples, or whether the dog simply hopes to avert the passing storm from her own head, the fact appears to me patent, that this dog has inherited the impression that sitting up has some special virtue for turning away wrath.

L. HURT

Alexandra Hotel, Harrogate, July 27

RECENT OBSERVATIONS IN THE BERMUDAS

AS my late visit to these islands has placed me in possession of facts relating to their original aspect of a somewhat conclusive nature, I deem it advisable to communicate such in a brief form, instead of awaiting the time requisite for the preparation of a more elaborate paper on the subject.

On previous occasions I have always regretted my inability, from lack of time, to look more closely into their geological character in the hope of discovering some satisfactory clue to their primitive condition. I was aware that in different parts of the islands road cuttings and well borings had revealed layers of red earth at certain depths below the surface, the consistence of which was similar to that now forming the present surface soil, and it did not require much force of imagination, after personal inspection, to conceive that such layers of red earth were first formed by the decomposition of vegetable matter which grew upon former surfaces, and became covered to their respective depths by accumulated masses of drift sand, which from natural causes hardened into more or less compact sandstone. But these different layers were but a few feet beneath the surface, and so, although interesting as throwing light upon the gradual elevation of the land by drift material forming over them, yet they afforded no evidence of a contrary nature—viz., the *submergence* of the Bermuda group. Indeed, I have always been led to suppose from appearances that the whole group was the result of an upheaval of the ocean bed slightly above the water level, and a gradual elevation afterwards by means of drift matter aided by the consolidating agency of reef-building zoophytes encircling the whole with a barrier reef, and by isolated patches gradually filling up the space within. The investigations, however, which I have recently been able to make, tend I think to prove that the barrier reef encircling the islands which has hitherto been considered an atoll is merely the remnant of the more compact calcareous rock which formed the shore of a much more extensive island group than that now existing.

My views in this respect are borne out by the following facts:—The barrier reef, as far as I have inspected it, is merely ordinary calcareous rock coated with serpulæ, nullipores, &c., the reef builders only working in the sheltered waters between the reef and the shore in three to eight fathoms. About two years ago submarine blastings were carried on at the entrance of Hamilton Harbour, and at a depth of over six fathoms a cavern was broken into which contained stalactites and red earth. Again, within the last few months, I have, through the kindness of his Excellency Major-General Lefroy, C.B., F.R.S., the present Governor, been placed in possession of still more satisfactory information. During the past two years extensive submarine blastings have taken place inside an artificial harbour, situate at the western extremity of the islands, for the purpose of forming a bed of sufficient depth for the reception of the "Great Bermuda Dock," which attracted so much attention off Woolwich when launched some three or four years ago. The excavations extended to a depth of fifty-two feet below low water mark. At forty-six feet occurred a layer of red earth two feet in thickness, containing remains of cedar trees, which layer rested upon a bed of compact calcareous sandstone. Here we have the first satisfactory evidence of the submergence of an extensive deposit of soil once upon the surface, and that to the depth of forty-eight feet below the present low water level, which consequently grants an equal elevation above it in former times. Now, on carefully surveying the Bermuda chart, we find that an elevation of forty-eight feet will bring the whole space which intervenes between the present land and the barrier reef, now covered with water, above the water level. This attained, what more is required to prove the former extent of the island group before the present submergence to the present barrier reef? But having clearly ascertained beyond doubt that the Bermudas were once forty-eight feet higher than at present, will any one be bold enough to deny them a greater elevation? I have reason to believe that they once extended in a south-westerly direction—not only out to the reef, but to a greater distance. There are some rocky ledges about twenty to twenty-five miles from land in that direction, known as "The Flatts," lying in about thirty-five to forty fathoms water; and, singularly enough, in the very oldest maps of the Atlantic, copies of which I have consulted in the British Museum, "The False Bermudas" are put down about this position. Is it unreasonable to suppose that a low lying group of islets did actually exist here in former times? Again, in Smith's "History of Virginia," which gives an excellent account of the islands in the early part of the seventeenth century, it is stated among other notes upon their natural history that flocks of crows, no doubt the same species (*Corvus Americanus*) which now inhabits them, were in the habit every evening of winging their flight from the main island towards the north. This observation, which from its simplicity I should the more readily believe to be a true statement, would clearly prove the existence of land in that direction at no great distance; for the habit of this bird to leave its roosting place for distant feeding grounds during the day, to return at random, is one of its well-known characteristics.

Taking these matters into consideration, I see everything to support the supposition that the Bermudas once presented a much more extensive aspect than they do at present, and certain additional evidences which I hope to bring forward shortly in a collected form will, I conceive, tend to confirm my impression that the restricted terraqueous area lying within the limits of the outer barrier reef is merely the summit of one of a range of islands which extended in somewhat semicircular form for a distance of seventy or eighty miles, and which have suffered submergence to a depth only to be correctly ascertained by borings, which might be successfully accomplished under the auspices of the Imperial Government at a trifling expense.

J. MATTHEW JONES

SPONTANEOUS APPEARANCE OF EXOTIC FORAGE PLANTS IN FRANCE AFTER THE LATE WAR

IN a communication lately made to the Paris Academy, M. Vibraye states the fact of certain exotic forage plants having appeared in considerable numbers in the central parts of France, after the stay of the army in these localities. He gives the following account of these plants :

"Their appearance, no doubt, results from forage supplied from abroad, the seeds of which had fallen into the ground. At the present time several Mediterranean plants, (chiefly Algerian), having braved the cold of an exceptionally severe winter, are being largely propagated, forming extensive meadows, and changing soil that was formerly arid, and produced no vegetation of importance, to veritable oases. This may probably lead to the definitive introduction of a large number of plants into a more northern region than that which they appear naturally to occupy and to prefer.

"The first notion of the possibility of such plants being brought into French soil, through the fodder consumption of the army, arose in 1870. A botanist of Strasburg, M. Buchinger, wrote to M. Franchet (conservator of my collections) some days before the investment of the place that, on examining the fodder which was being given to the horses, he had found in it forty-four species of plants belonging to the Mediterranean region, and most of which were Algerian. He suggested to M. Franchet to make observations, and see whether some of these plants would not appear in the soil. The prediction was verified; and in April 1871, M. Franchet found two exotic centauries, and communicated the fact to M. Nouel, the director of the Orleans Museum. Since then these two botanists have made minute researches on the subject, and have discovered many more such plants. This is, briefly, the history of the discovery. The places where the phenomenon has been best observed are in the Department of the Loire-et-Cher: (1) on the right bank of the Loire, near the railway; (2) on the left bank, the country about Blois. Then, too, in the Communes of Cour, and more especially of Cheverny, which districts were successively occupied. Observations have also been made at Orleans, on the Boulevard Saint Jean, and the Isle of Arrault.

"I shall best recount these observations by giving them as they were written down in the order of date.

"On the 18th March, 1872, certain new plants, observed the previous year at Blois and Orleans, had increased considerably on arid soil, which formerly bore only some poor and stunted herbs. Each of the districts furnished about 90 to 100 species. But the species were not the same in each district, and the total number of species met with, was 157, belonging to twenty-one families, as follows :—

Ranunculaceæ .	3	Compositæ .	28
Resedaceæ . .	1	Convolvulaceæ	1
Cruciferæ . . .	8	Borragineæ .	1
Sileneæ . . .	5	Verbasceæ .	1
Alsineæ . . .	1	Plantagineæ .	1
Lineæ	1	Labiatæ . .	2
Malvaceæ . . .	8	Amaranthaceæ	1
Geraniaceæ . .	7	Salsolaceæ .	2
Leguminosæ . .	52	Polygoneæ .	1
Umbelliferæ . .	4	Gramineæ .	28
Dipsaceæ . . .	1		

Entire total . . . 157

"The first observations could not be considered quite definitive, and there were some species which it was, perhaps, a little premature to determine; but after deductions made on this account there remained 140 species; a number much larger than that of the plants on the best meadow lands in France, which, according to botanists, do not furnish more than 90 to 100 species.

"It will be seen that the Leguminosæ (52 species) form about one-third of the whole, while the Gramineæ and the Compositæ form each about one-fifth (28 species). Among the Leguminosæ trefoil and lucerne predominated; 12 kinds of trifolium were counted, and 10 or 11 of medicago; the 28 Compositæ belonged to 21 genera, and the 28 Gramineæ to 16 different genera. Two thirds of these forage plants are annuals or biennials.

"On the 12th April a fresh examination of the Blois district showed that many of the species, and especially the medicagos, had not only persisted but been propagated from seeds. Unfortunately for the full development of this, the land was used as a pasturage for sheep, which devoured the plants in proportion to their growth, and uprooted many with their feet, the arid soil having little cohesion. At Cheverny, where I had enclosed certain spaces, several Algerian plants attained a superb growth; among these, trefoil, melilot, phalaris, and alopecurus.

"By the 19th April it was evident that not only a large number of species had persisted, but that from being rare and poor the previous year, they had developed to a wonderful extent, notably the *Alopecurus utriculatus*, *Vulpia ligustica*, *Avena barbata*, *Trifolium nigrescens*, *Trifolium isthocarpum*, all southern and forage plants. The *Medicagos sphærocarpa* and *pentacycla* reappeared in great abundance where they had escaped the cattle, or the sickles of women in search of herbs. It was difficult to persuade these herb-gatherers to a temporary self-sacrifice with the view of ultimate advantage in respect of forage.

"On the 27th April my attention was called to the large number of annuals and biennials disseminated. The annual plants, which De Candolle called monocarpian, because they terminated their existence by a perfect and unique fructification, are invariably reproduced by the dissemination of their seeds when they have come to maturity; the individual dies, while the species becomes perennial; which duration, on the other hand, is temporarily acquired by the individual, when a scythe, or the grazing of cattle, retards the last and indispensable phase of the plant's existence.

"At this date, April 27, trefoil and lucerne promised to be very abundant in the Blois district. By the 1st of May an incessant appearance of new species was noticed; and it seemed beyond a doubt that before the end of the season the four localities of Blois, Cheverny, Vendôme, and Orleans, would furnish not fewer than 200 species, of which 170 at least were peculiar to meadows and pasture lands.

"On the 3rd May a new fact presented itself. During the war a forage depôt had stood at the side of the market place of Cour Cheverny, but last year no appearance of any new plant had been discovered there, notwithstanding careful search. On the 3rd some twelve species of foreign forage plants made their appearance, and others were expected. This shows that the seeds had remained in the soil for sixteen months without alteration of their germinating properties. This is an interesting fact, and a corollary of a law of rotation observed in all natural meadows, viz., the temporary disappearance of dominant species, yielding for an indeterminate period to new types, which they by-and-by come to displace, when they meet with certain favouring atmospheric influences, not very fully understood.

"The Algerian plants withstood successfully the frost of the 12th May, which did much damage to our vines and the shoots of certain trees, especially indigenous conifers."

M. Vibraye, in closing his note, speaks of the above phenomena as being quite an event. It is not the ephemeral appearance of a few plants sparsely disseminated, raising the problematical hope of fixing in the soil a useful vegetable—it is an exodus; the migration, not of a modest *florule*, but of a *flora*, independent and complete. It is, in a word, a treasure which the central districts of France are called on to study and to appropriate. He argues that the propagation of these plants should not be left to chance, but that human skill should be brought to bear on it.

A. B. M.

THE HURON RACE AND ITS HEAD FORM[*]

THE tribe of North American Indians known by the name of Hurons appears, when first brought to the knowledge of the intruding French, to have been settled in palisaded villages around Lake Simcoe, in Western Canada. They called themselves Ouandots or Wyandots. They consisted of four septs or nations; the Attignaouentans, or Nation of the Bear, the chief member of the league, the Attignenonghaes, the Ahrendarrhonons, and the Tohotaenrats. They occupied thirty-two villages when visited by the Jesuit missionaries in 1639. Brébeuf reckoned them in all in 1635 at 30,000, and they were stated in the *Relation* of 1660 at 35,000. The Hurons with other tribes dwelling at this time in Canada, were fully acquainted with agriculture, as Dr. Wilson shows, wholly independent of any European influences. The Hurons became known to the civilised world only in their decline, and immediately before their extirpation. They were then in alliance with the Adirondacks and other Algonquins, against their common Iroquois enemy. This latter is the name of a league of tribes often designated the Indians of the Five or of the Six Nations. This confederation of tribes during the seventeenth century was the great aggressive nationality of the American Continent, which subdued, exterminated, or incorporated the other tribes with which they came into contact. Cartier discovered Canada in 1535. Champlain explored and settled it subsequently. He visited the Huron country in 1615, and appears to have found the whole district between the river Ottawa and Lake Simcoe to have been almost depopulated, which is to be attributed to the implacable enmity of the Iroquois. This region, "in the latter part of the seventeenth century became the scene of the indefatigable operations of a succession of missionary fathers, some of whom divided their self-denying labours between them and their Iroquois foes, and several suffered martyrdom at the hands of the savage nations whose conversion they aimed at. Minutely detailed maps and narratives of exploration and missionary labours, record the progress of discovery in the region around the Georgian Bay, and illustrate the topography of the Huron villages so accurately, that most of their sites have been identified in recent years. Dr. J. C. Taché devoted such leisure as he could command during a period of five years, prior to 1865, to a minute exploration of the Huron country. Following in the steps of early writers whose accounts are preserved in the *Relations* of the Jesuit Fathers, communicated to the Provincial of the Order at Paris, from 1611 to 1672, he was able to determine the sites of their principal villages, and to explore their cemeteries, abounding with implements, weapons, and numerous other archæological records of native arts and habits.

The sepulchral rites of the Hurons were of a peculiar character. Their dead were primarily exposed on raised biers, as is still done by the Cowlitz, Columbia River, and other tribes; and around them were hung implements and personal ornaments of the deceased, with the tributes of affection of the survivors. In the case of death on a journey, or on the war-path, the body was temporarily interred. But the place of sepulture was carefully noted for future transfer of the bones to the general cemetery of the tribe. At intervals of ten or twelve years the great "Feast of the Dead" was celebrated by each nation of the Huron confederacy. One of these grand ceremonies, performed at Ossosane, the chief town of the Bear nation, on Nottawasaga Bay, was witnessed by the Jesuit missionaries in 1636. Skeletons were gathered from old scaffoldings, or disinterred from distant graves, by the relations of the deceased. The bones of those more recently dead were cleansed of the remaining flesh, and then wrapped carefully in skins, and adorned with prized decorations. The old wampum-belts, pipes, kettles, bows, arrows, axes, beads, and shells, which had been hung around the bier, or deposited in the grave, were anew gathered together, and the whole were brought to the appointed cemetery. There a great trench was dug, and carefully lined with beaver skins and other furs, and after a funeral-feast, with lamentations by the women, and orations by some of the chiefs in praise of the dead, the relics of mortality were cast into the trench, along with the funeral offerings. Only in cases of recent death were the corpses wrapped in furs and deposited entire; and then, amid the shrieks and wails of the mourners, the earth was thrown in, logs and stones piled over the cemetery, and with a closing funeral chant, the great "Feast of the Dead" was brought to a close.

Owing to the systematic practice of thus gathering together the remains of the Huron dead, one or more ossuaries were to be looked for in the vicinity of each village. Dr. Taché explored sixteen of them in all, containing from 600 to 1,200 skeletons in each. From the same depositories he also recovered numerous specimens of native art, and illustrations of the various customs of that people. From the same cemeteries, Dr. Taché selected upwards of eighty skulls, most of which with the accompanying relics he deposited in the museum of the Laval University, at Quebec. Another inquirer, Mr. John Langton, a writer "On the Early Discoveries of the French in North America," conceived the same idea of tracing the villages of the Hurons. He succeeded in tracing out the sites of fourteen villages, in many of which the remains of houses and stockades could still be recognised. He even succeeded in identifying St. Ignace, a village at which the principal chief and nearly 100 of the Iroquois warriors fell before the Hurons were overpowered, and the miserable remnant bound to stakes to perish in the relics of their blazing settlement. St. Ignace was finally destroyed in 1649. Some few Hurons found refuge among the Petuns, Neutres, and Eries, and shared in the subsequent fate of these tribes. The fortunes of another body of the fugitives illustrate the Indian practice of adoption into another tribe. The survivors of two Huron towns opened negotiations with their Seneca foes, and were adopted into the Seneca nation. And another band, under the conduct of the Jesuit missionaries, made their way to Quebec, then subsequently settled at Lorette, on St. Charles's river, where their lineal descendants remain, their blood mingled with that of European colonists, and speaking a French *patois*, and where they still share in certain Indian funds distributed to them by the Canadian Government.

The admixture of blood has nearly effaced the genuine characteristics of the Hurons of Lorette, although this tribe originally does not appear to have been exposed in the same degree to the adoption of prisoners of other tribes as that of the Iroquois Confederacy. Hence the remains recovered from the ancient cemeteries of the Huron country have a special value for ethnological purposes; they are the most authentic relics of the pure Hurons. As an instance of the readiness with which the aggressive Iroquois received those of other races into their tribe, Dr. Wilson speaks of an old squaw of pure white blood, reputed to be nearly a century old, who survived till recently, if she be not still living, as a member of the Mohawk tribe, of the Bay of Quinte. Her Indian name is Ste-nah, supposed to be a contraction of Christiana, and she is described as a full-blood Sko-ha-ra, or Dutchwoman. When the author last heard of her, in 1868, she was living with her granddaughter, the wife of a Mohawk chief.

The learned author is probably the solitary instance of a well-instructed British Craniologist being transferred to American soil, who has continued his favourite pursuit in the new field with perseverance. Dr. Daniel Wilson has given numerous and valuable proofs of his abiding taste for, and persistent investigation in, this fundamental branch of anthropological inquiry. In his comprehensive "Pre-Historic Man," which has reached a second edition, he has already paid a good deal of attention to the Huron tribe, as well as to other tribes of the North American Indians, which, in fact, constitutes one great feature of value in the work.

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[*] The Huron Race and its Head Form. By Daniel Wilson, LL.D. 3 plates.

In this volume Dr. Wilson gave a table of the measurements of thirty-seven Huron skulls "obtained from Indian graves to the north of the water-shed between Georgian Bay and Lakes Erie and Ontario." The great purpose of the construction of this table, as of so many other investigations made by the author, was to test the truth of a doctrine which had been generally received and was in great measure endorsed by Dr. S. G. Morton, who produced the classical "Crania Americana." This doctrine, in few words, is that the American race is almost uniform in its head characters from one end of the continent to the other, and that the American typical cranium is distinguished for its shortness. To Dr. D. Wilson's persevering researches we must allow the merit of having shown that there is much variety in native American races, and that in their skulls there is considerable diversity of length. This may be very confidently asserted, without conceding to him an agreement with his assertions respecting the long skulls among the ancient Peruvians; for he evidently mistook the deformed crania of these people for the representatives of the natural form of their heads.

In the table of the measurements of thirty-seven human skulls referred to, it was seen that their length varied much. If we exclude the "Barrie skull," No. 23, which appears to be quite aberrantly short, and it is thought has been distorted by art, we find their longitudinal diameter to vary from 7·9 inches to 6·8 inches, or above an inch. The cephalic indices of these two examples, unfortunately, cannot be accurately calculated from Dr. Wilson's measurements. One of his items of measurement is "F. D." frontal diameter, which he says in this memoir "is taken from the point of junction of the frontal, parietal, and malar bones." This latter is probably an inadvertence, and should be sphenoidal bone, as the malar bone nowhere joins the frontal.

Since this table was constructed, thanks to the efforts of Dr. Taché and others, Dr. Wilson has had the opportunity of measuring many more Huron skulls. He now says: "The sight of upwards of seventy skulls, all derived from the cemeteries of a single tribe or nation, is a peculiarly interesting study to the ethnologist. But to one at all impressed with the uniform persistency of a specific ethnical type, the result is far from satisfactory." They are seen to vary materially, and especially in length. The skulls of women present a decided projection of the occiput, and here we may be permitted to allude to Dr. Wilson's plates. The first, which gives shaded profile views of the calvaria of a Huron man and woman, is excellent and very characteristic. Plate II. is lettered, "Long Huron skull, male." It is often very difficult to determine positively the sex in crania, but in this example it seems doubtful whether the plate does not exhibit the calvarium of a woman. We incline to think that it does. The long oval, vertical view is quite apparent and unquestionable. Plate III. is an undoubted instance of a "Long Huron skull, male."

Dr. Wilson, after attesting the great range of diversities in the Huron skulls he has seen, concludes in these noticeable terms: "But the specialties of the whole, in their front aspect, suggest a greater uniformity in their physiognomy than in cranial conformation. The nose is in most cases large and prominent; the superciliary ridges in the males are strongly developed; and a common ethnical character may be traced in the full-face as a whole, including the massive broad cheek-bones and superior maxilla; as well as in the indications in the greater number of a tendency towards a pointed apex, or meeting of the parietal bones at an angle at the sagittal suture." Perhaps this is as much as anyone can reasonably expect, even when divergences are to be acknowledged in the proper calvarial form. And it is difficult to conceive that these divergences are so utter and so puzzling as to prevent our seeing any constancy among them.

Dr. Wilson, in showing that some tribes of American Indians are characterised by long or dolichocephalic heads, still admits that other tribes have short or brachycephalic heads. This must be conceded, as well as that Morton's generalisation was too comprehensive and too literal. Former sweeping conclusions as to dolichocephaly and brachycephaly cannot now be sustained. It was long since seen that among the crania of any extended race of people, as the ancient Britons, there is much variation as to length, indeed that a scale might be exhibited from the shortest to the longest, in which the numerous intermediate lengths intervene to fill up the ascending degrees of the range. This truth is now more than ever apparent, since the elaborate researches among the skulls of Italian races by the distinguished anatomist, Prof. Luigi Calori, of Bologna. He has conclusively shown that there is much more dolichocephaly among the races of Italy than was previously known.

Much of the difficulty that craniologists have encountered in the study of the head-forms of different races, has had its foundation in the too rigid rules which they have assumed these forms to observe. Dr. Wilson's labours have served well to illustrate this point in reference to the American races. Were this the proper place, it would be easy to point to examples of the futile labours which have resulted from these self-imposed rules. How many learned controversies have been entertained to determine the race of a man whose only existing relic was his lower jaw? On finding that such rules cannot be fixed and defined in so absolute a manner, it has often been the case that other inquirers have lost confidence in craniology itself. This is a transition from one extreme to another. More moderate expectations from the doctrine of skull-forms would have prevented confidence in their value from being so often shaken. Larger views must be taken, but these are quite compatible with our knowledge, without any necessary leaning to the meanderings of the evolutionary hypotheses alluded to by our author.

CONDUCTIVITY OF MERCURY

It was shown in a previous article* that solar intensity cannot be accurately ascertained by the thermoheliometer employed by Père Secchi, owing, among other causes, to the imperfect conductivity of the mercury in the bulb exposed to the sun. Meteorologists, however, do not generally accept the assumption that the conducting power of mercury is so imperfect as to affect materially the correctness of the indication of mercurial thermometers, Deschanel being quoted in support of the opinion that mercury is not an imperfect conductor. We are reminded that Prof. Everett, in a recent translation of the works of the author mentioned, assumed that the conductivity of quicksilver in the bulb of a thermometer is the same as a vessel "with thin metallic sides containing water which is stirred" (see Prof. Everett's translation of "Deschanel's Natural Philosophy," Part II., pp. 245-387). The subject is so intimately connected with the determination of solar temperature and solar energy, that it has become indispensable to settle the question by some thorough practical test. Accordingly an apparatus, represented by the following illustration (Fig. 1, p. 266) has been constructed by the writer, to ascertain the conductivity of mercury. Before entering on a description, it will be instructive to point out that the heat communicated to the bulb of a thermometer by solar radiation is transmitted to its contents chiefly by convection, hence that the altitude of the sun during the observation influences the accuracy of the indication. This will be readily comprehended. Fig. 2 (p. 267) represents the bulb of a thermometer exposed to the rays when the sun's zenith distance is 65°; Fig. 3 representing the bulb when the zenith distance is 18° 23′, the latter being the minimum at the Observatory of the Roman College, where the thermoheliometer has been long employed for the purpose of ascertaining the intensity of solar radiation. Referring to Fig. 2, it will seen that the blank crescent *c*, whose varying thickness indicates very nearly the amount of heat imparted at each point of the spherical surface presented towards the sun, occupies a nearly vertical position. The mercury contained within the space indicated by the said crescent, having its specific gravity reduced by the radiant heat, will ascend; while the mercury on the opposite side, which retains its specific gravity, will descend; thus a circulation will be established by means of which the heat received from the sun will be gradually communicated to the entire mass of mercury in the bulb. But, when the latter is exposed to the sun's rays under a zenith distance of about 18°, as shown in Fig. 3, the heated mass of mercury contained within the crescent *a* has so slight an inclination that scarcely any circulation takes place. Consequently, if it can be shown practically that mercury is incapable of transmitting heat from particle to particle with sufficient velocity, it will be evident that thermometers and thermoheliometers, with spherical bulbs are worthless as means of measuring the intensity of solar radiation. It will be perceived that if the bulb in Fig. 3 be surrounded by an enclosure, as in the thermoheliometer, the mercury contained within the space indicated by the crescent *b* will radiate far less heat towards such enclosure than the mercury within the opposite heated crescent *a*. It will also be perceived that by increasing the size of the bulk the transmission of heat from *a* to *b* will be retarded unless the conductivity of mercury be perfect. Hence the *size* of the bulb is an element affecting the accuracy of the indication—a circumstance fatal to the employment of a spherical bulb in the thermoheliometer.

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* NATURE, vol. v. pp. 344-347.

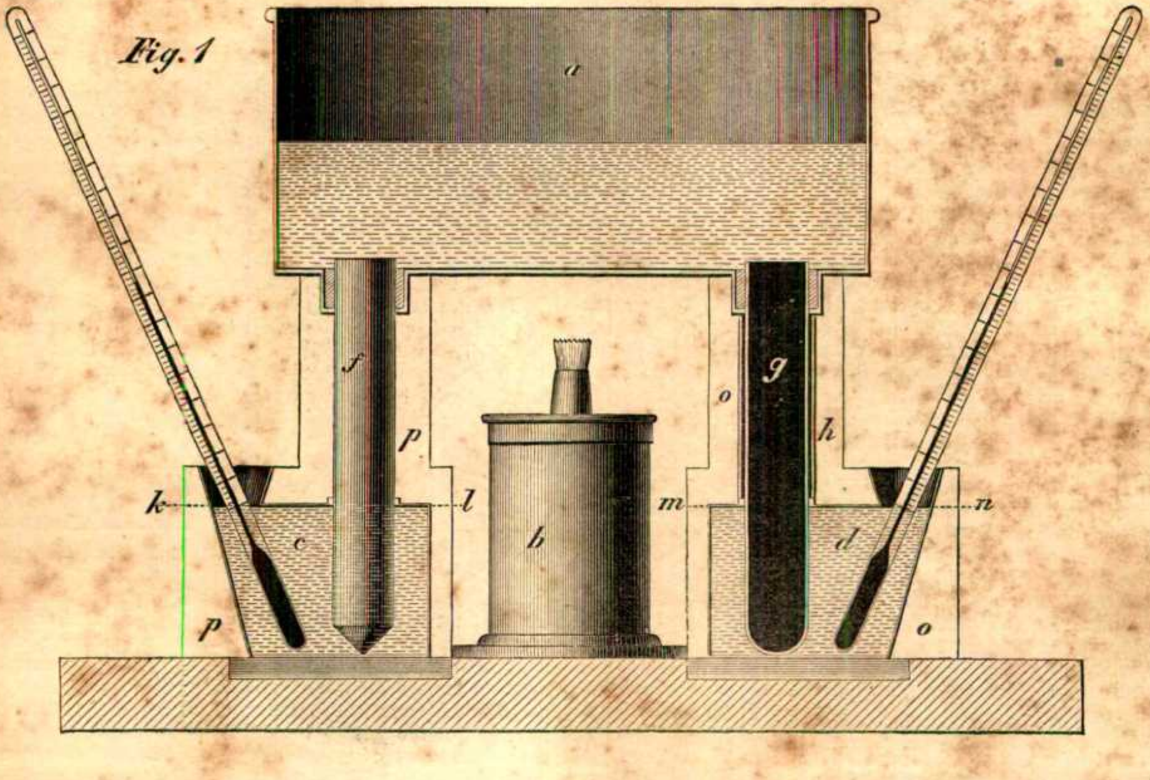


Fig. 1

The nature of the illustrated apparatus constructed for the determination of the conductivity of mercury will be readily understood from the following description:—Fig. 1 represents a longitudinal section through the vertical plane. *a* is a boiler, with a flat bottom and semicircular ends, supported on two columns, *f* and *g*, resting on the bottom of the cisterns *c* and *d*. The column *f* is composed of wrought copper plated with silver, highly polished. The column *g* consists of a cylindrical vessel of glass open at the top, filled with mercury, and surrounded with a socket, *h*, composed of polished silver. The cisterns *c* and *d*, supported on nonconducting substances, are plated with polished silver, and provided with funnel-shaped openings at the top, through which thermometers are inserted. These cisterns, as well as the columns *f* and *g*, are surrounded with nonconducting coverings, *p*, *p*, and *o*, *o*. A lamp, *b*, is applied behind the cisterns for heating the water in the boiler. It is scarcely necessary to observe that the polished silver plating of the copper column, and the polished silver socket round the mercurial column, are intended to prevent loss of heat by radiation, while the coverings before mentioned are intended to prevent loss of heat by convection attending atmospheric currents. The inside diameter of the cylindrical vessel *g*, it should be noticed, is 0·5 in., corresponding exactly with the diameter of the copper column *f*, the top of which is on a level with that of the mercurial column. The lines *k l* and *m n* are in the same horizontal plane, their distance below the upper ends of the columns *f* and *g* being precisely two inches.

The object of the apparatus being that of comparing the conductivity of mercury to that of some other metal *copper* has been selected, as its conducting property is better known than that of any other. The leading feature of the arrangement will be comprehended by a mere glance at the illustration. An equal amount of heat being applied to each column, it is intended to show by the elevation of the temperature of the water in the cisterns *c* and *d*, what relation exists between the conductivity of mercury and copper. Regarding the application of the heat, it will be evident that an equal amount must infallibly be imparted to each column if the lamp be sufficiently powerful to keep the water in a state of continuous ebullition. Obviously the heat from the lamp, if urged, will cause a rapid upward motion of the water in the middle of the boiler, and a correspondingly rapid descending current at each end. Accordingly lateral currents varying in velocity with the strength of the flame applied under the boiler, will flow inwards over the upper ends of the columns *f* and *g*.

Several experiments have been made under varying barometric pressures and different atmospheric pressures; but the results as regards the comparative conductivity of mercury and copper have proved to be very nearly alike in all. The accompanying tables record the result of the last trial, conducted as carefully as practicable. The headings of the several columns explain so clearly the object of the tables that it will only be necessary to state that the energy inserted in the fourth column is the energy developed from the beginning of the experiment.

Referring to Table I., it will be seen that at the termination of four minutes from the commencement of the experiment, the temperature of the water in the cistern *c* had increased 29·06°, the differential temperature being then 212° − 102·56° = 109·44°. During the same period a dynamic energy represented by 2·525 thermal units had been transmitted past the line *k l*, communicated to (1) the water in the cistern; (2) the part of the copper column immersed; (3) the metal composing the cistern; (4) the immersed part of the thermometer. But, while the entire energy transmitted past the line *k l*, during the four minutes thus amounted to only 2·525 units, the rate of transmission was actually 0·850 unit per minute at the termination of the fourth minute. This apparent discrepancy was caused by the heat absorbed by that part of the column which extends above the line *k l*, the temperature at the commencement of the experiment being the same as that of the surrounding air, 73·50°. Referring to Table II., it will be seen that the energy transmitted through the mercurial column, past the line *m n*, during four minutes, was only 0·087 unit against 2·525 units for the copper column, although the differential temperature of the water in the cistern *d* was 137·50° − 109·44° = 28·06° higher than in cistern *c*. Accordingly, the conductivity of the copper composing the column *f* has proved to be $\frac{2{\cdot}525}{0{\cdot}087} = 29{\cdot}06$ times greater than the conductivity of the mercury of the column *g*, notwithstanding the higher differential temperature to which the latter was exposed. It will be observed that the glass, 0·02 in. thick, composing the cylindrical vessel which contains the mercury, will conduct some heat downward, tending to increase the temperature in the cistern *d*. This tendency, however, will be balanced by the loss of heat occasioned by the radiation of the glass cylinder, since the application of the polished silver socket and the non-conducting covering cannot wholly prevent the refrigerating action of the surrounding air. It is important to observe, regarding the loss of heat from the latter cause, that the cisterns, previous to trial, are charged with water of the same temperature as the atmosphere. Now, considering that the increment of temperature in the cistern *d* does not average more than 0·40° above that of the atmosphere during the trial, it will be evident that the amount of error caused by radiation will be quite inappreciable. We are therefore warranted in concluding that the conductivity of mercury, determined by the increment of temperature in cistern *d*, and by the dynamic energy transmitted past the line *m n*, cannot be far from correct. It will be asked why columns of such small diameter have been employed. The principal object has been that of presenting a sectional area in the mercurial column *g*, corresponding as nearly as possible to the size of the bulb of an ordinary thermometer. Regarding the dimensions, it will be readily admitted that the conductivity of mercury might have been ascertained with greater exactness, if columns of very large sectional area had been employed; but the trial has conclusively established the fact that mercury transmits heat from particle to particle too slowly to effect a sufficiently rapid indication of mercurial thermometers provided with *spherical* bulbs; and that, when the heat is applied from above, the indication of such thermometers is wholly unreliable.

Fig. 3

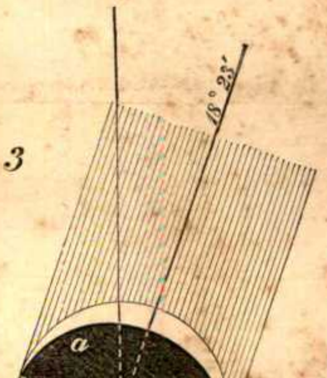
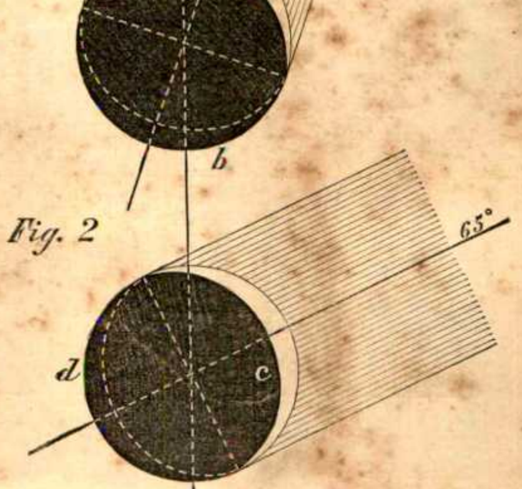


Fig. 2



A subject of profound interest presents itself in connection with the rate of transmission of energy exhibited in the sixth column of Table I. It will be seen that although the copper column *f* is only 0·5 in. in diameter = 0·19635 sq. in. section, the rate of transmission at the termination of the fourth minute is 0·850 unit per minute. Reducing this amount to the usual standard of one square foot, it will be found that the energy developed is $\frac{144}{0{\cdot}19635} \times 0{\cdot}850 = 623$ thermal units per minute for a sectional area of 1 sq. foot. It will be observed that this extraordinary amount of energy (theoretically capable of exerting $\frac{623}{427} = 14{\cdot}5$ horse-power) is called forth by the moderate differential temperature of 212° − 102·56° = 109·44° F. Now, let us compare the energy of 623 thermal units per minute to that produced by the radiation of a metallic surface coated with lamp black, and maintained at a temperature of 212°, within an enclosure of 102°. Actual trial shows that under these conditions, the radiant energy of a blackened plate composed of copper, containing 144 sq. inches, scarcely reaches 6 thermal units per minute. Our experiment has therefore established the fact, incidentally, that under the stated conditions, a plate of wrought copper two inches in thickness is capable of transmitting by conduction from one side to the other, in a given time, an amount of mechanical energy more than 100 times greater than the mechanical energy developed by the radiation of the same plate during an equal interval of time.

TABLE I.—COPPER COLUMN

TIME.	Temperature of Water in Cistern.	Increment of Temperature in Cistern.	Energy Transmitted past *k l*.	Differential Temperature Between Boiler and Cistern.	Energy Transmitted past *k l* per Half-minute.	Energy Transmitted per Square Foot per Half-minute.
Min.	Fah.	Fah.	Therm. Units.	Fah.	Therm. Units.	Therm. Units.
	73°50	°		138°50		
0·5	75·15	1·65	0·143	136·85	0·143	104·873
1·0	77·25	3·75	0·326	134·75	0·183	134·208
1·5	80·14	6·64	0·577	131·80	0·251	184·078
2·0	83·84	10·34	0·898	128·10	0·321	235·415
2·5	88·14	14·64	1·272	123·80	0·374	274·284
3·0	92·81	19·31	1·678	119·00	0·406	297·752
3·5	97·67	24·17	2·100	114·25	0·422	309·486
4·0	102·56	29·06	2·525	109·44	0·425	311·686

TABLE II.—MERCURIAL COLUMN

TIME.	Temperature of Water in Cistern.	Increment of Temperature in Cistern.	Energy Transmitted past *m n*.	Differential Temperature Between Boiler and Cistern.	Energy Transmitted past *m n* per Half-minute.	Energy Transmitted per Square Foot per Half-minute.
Min.	Fah.	Fah.	Therm. Units.	Fah.	Therm. Units.	Therm. Units.
	73°50	°		138°50		
0·5	73·52	0·02	0·002	138·48	0·002	1·466
1·0	73·56	0·06	0·005	138·44	0·003	2·200
1·5	73·64	0·14	0·012	138·36	0·007	5·133
2·0	73·75	0·25	0·022	138·25	0·010	7·334
2·5	73·90	0·40	0·035	138·10	0·013	9·534
3·0	74·08	0·58	0·051	137·92	0·016	11·734
3·5	74·28	0·78	0·068	137·72	0·017	12·467
4·0	74·50	1·00	0·087	137·50	0·019	13·934

J. ERICSSON

NOTES

THE voluminous correspondence connected with the management of Kew Gardens, printed by order of Parliament, was issued at the close of last week. In the debate in the House of Lords on Monday, introduced by Lord Derby, the essential points of the controversy were hardly touched; and pending the debate in the House of Commons on Sir John Lubbock's motion, which was not reached on Tuesday evening, it is obviously premature to enter into the various discussions which are prompted by the contents of the Parliamentary papers.

THE choice of a foreign correspondent of the French *Académie des Sciences* has resulted in the defeat of Mr. Darwin and the election of M. Loewen, of Stockholm, who received 32 votes, against 15 given to the English naturalist. The discussion had extended over three long sittings in secret committee, the leader of the advocates of Mr. Darwin's claim being again his opponent in controversy, M. de Quatrefages, while M. Emile Blanchard led the opposition. A correspondent of *Les Mondes*, an eminent member of the Academy, in commenting on the result, states that not one of those who voted for Mr. Darwin shared his philosophical doctrines, and not one of those who opposed his candidature alleged as their motive the error or danger of his doctrines. "What has closed the door of the Academy to Mr. Darwin is that the science of those of his books which have made his chief title to fame—the 'Origin of Species,' and still more the 'Descent of Man'—is not science, but a mass of assertions and absolutely gratuitous hypotheses, often evidently fallacious. This kind of publication and these theories are a bad example, which a body that respects itself cannot encourage."

THE subject of Prof. W. K. Clifford's discourse at the ensuing meeting of the British Association will be, "The Aims and Instruments of Scientific Thought." Mr. W. Spottiswoode will deliver a lecture to working men, on "Some Properties of Light, Sunshine, Sea, and Sky."

THE Iron and Steel Institute will hold its next meeting in Glasgow, under the presidency of Mr. Henry Bessemer. The reception room will be in the Corporation Galleries, Sauchiehall Street. The general meetings will be held in the Lecture Hall, Corporation Galleries, Sauchiehall Street, as under:—Tuesday, Aug. 6, 1.30 P.M., Election of Members, Report of Council to be presented, Papers; Wednesday, Aug. 7, 10.30 A.M., Reading and Discussion of Papers; Thursday, Aug. 8, 10.30 A.M., Reading and Discussion of Papers. A room will be provided in the Corporation Galleries for the exhibition of objects of interest to the trade, either from the neighbourhood of Glasgow or elsewhere. The business proceedings will terminate about 3 o'clock on Tuesday, and it is proposed that members should devote the remainder of the day to visiting the iron and other works in the neighbourhood of Glasgow. A list of these works will be given in the detailed programme to be distributed at the meeting; and a sketch map of the district will also be ready at the same time. On Wednesday, immediately after the conclusion of the business meeting, a special train will be provided to convey the members to Coatbridge and Monkland, where the Gartsherrie, Summerlee, Langloan, Coatbridge, Malleable, and Monkland Iron Works will be inspected. On Thursday, after the conclusion of the general meeting in Glasgow, a special train will be arranged for the Motherwell district, and for the inspection of the iron works in that locality. On Thursday evening, at 7 o'clock, the members will be entertained at dinner, in the Corporation Galleries, Glasgow, by the Local Reception Committee. On Friday an excursion will be made down the Clyde, for the purpose of seeing the industrial features connected with the river. The excursion will also be prolonged to the head of Loch Long, the return being by Loch Lomond to Balloch, and thence by train to Glasgow. Luncheon will be provided at Tarbet. The following papers have been already promised:—"On the Geological Features of the Iron and Coal Districts of Scotland," by Mr. James Geikie, F.G.S., of the Geological Survey of Scotland; "On the Rise and Progress of the Iron Trade in Scotland," by Mr. Jno. Mayer, F.C.S., Glasgow; "On the Differential Clutch for Reversing Mills," by Mr. R. D. Napier, Glasgow; "On Reversing of Rolling Mills," by Mr. Graham Stevenson Airdrie; "On an improved form of Squeezer for Blooms produced in

Rotary Puddling Furnaces," by Mr. James Robertson, Glasgow; "On the Rise of Iron Steamship Building on the Clyde;" "On further Improvements in Spencer's Revolving Puddling Machine," by Mr. Adam Spencer, West Hartlepool; "On the Westward Development of the Iron Manufacture in the United States," by Mr. T. Guilford Smith, Philadelphia.

THE Annual Meeting of the Association of German Naturalists and Physicians will be held at Leipzig, from Aug. 12 to Aug. 18.

A MATHEMATICAL society of Paris has been formed, on the plan of the similar societies of London, Moscow, and Berlin, having for its object to encourage mathematical studies and increase mathematical knowledge, and to form a bond of union of those interested in the mathematical sciences. Among its original members are MM. Chasles, Serret, Ossian Bonnet, Resal, Bourget, Mannheim, Laurent, Halphen, de Polignac, Ribeaucourt, Lemoine, Laguerre, Gros, Brisse, and André. The Society proposes publishing a *Bulletin* of its Proceedings.

WE regret to learn that Prof. H. E. Roscoe is laid by from a serious accident—a kick from a horse. The injuries, however, we understand, are only such as complete rest will repair.

JUDGMENT has been given by Lord Gifford, in the Court of Session at Edinburgh, in favour of Miss Jex Blake and other lady students in their action against the Senatus Academicus, asserting their right to complete at the University their full medical curriculum, and to graduate on the same footing as male medical students. The judgment finds the ladies entitled to all the privileges of medical students, and to graduate in medicine at the University. The laws of Scotland, continues the judge, hardly as they bear against women in some respects, have never gone so far as to forbid their entering the medical profession.

WE are glad to observe that a movement which we noted in April last has succeeded in giving a well-earned reward to Mr. James Lindsay, Experimental Assistant to the Professors of Natural Philosophy in Edinburgh University for the last fifty-four years. A meeting was held in Edinburgh on the 24th, presided over by Prof. Sir R. Christison, Bart., M.D., who, in a few words, pointed out the great merit which had induced Mr. Lindsay's friends to present him with this manifestation of their respect, and gave a sketch of his long connection with the Natural Philosophy chair at the University. The subscriptions amounted in all to 235*l.*; the list of subscribers including the names of H.R.H. the Duke of Edinburgh, the Marquis of Tweeddale, the Duke of Argyll, Dr. Lyon Playfair, M.P., Mr. Carlyle, Prof. Tyndall, Prof. Sir R. Christison, Sir W. Thomson, W. M. Rankine, Tait, &c.

MR. J. LOWTHIAN BELL, of Newcastle-on-Tyne, has been elected an honorary member of the United States Mining and Engineering Institute, in recognition of his labours.

IT has been determined to erect a statue in Berlin to the memory of Albert von Graefe, the eminent oculist. The names of some of the most distinguished surgeons of Europe are on the committee.

WE understand that the Royal College of Chemistry will resume its operations at the commencement of the Autumn Term, in the new laboratories at South Kensington.

THE Museum at Thornhill, Dumfriesshire, is now open to the public on Saturdays from one o'clock. Admission will be by free tickets, which may be obtained by application at the museum, and a description will be given of some special class of objects. On other days of the week, Sunday excepted, admission will be by tickets for which the charge of sixpence will be made. To the museum will be added a small library of selected books, which may be had on loan; also a library of books for consultation at the museum. This library consists of about two thousand volumes. A few periodicals will also be added. A descriptive catalogue of the museum and a catalogue of the library are in preparation.

THE most recent publication of the Geological Survey consists of the explanation of quarter-sheet 98 S.E., illustrating the geology of the neighbourhood of Kirkby Lonsdale and Kendal, by Messrs. Aveline, Hughes, and Tiddeman. It contains the usual sketch of the physical geography of the district, and lists of the fossils of the various formations, and is illustrated by several sections.

MR. W. R. HAYWARD has in preparation a new work, which will be called "The Botanist's Pocket-Book." It is intended as a handy pocket companion for the botanist in the field, to enable him to identify on the spot the plants he may meet with in his researches. It will contain the characteristics of species and varieties, the botanical name, common name, soil, and situation, colour, growth, and time of flowering, of every plant, arranged under its own order. The volume will be published by Bell and Daldy, York Street, Covent Garden.

AN excellent compendium of our present knowledge of the sun and the phenomena of its atmosphere, from the pen of Prof. Young, has just been published by Chatfield and Co., of New Haven. This author, it is well known, has himself occupied a very prominent part in the history of more recent discoveries in regard to the sun, and the article referred to is the substance of a lecture delivered at New Haven during the past winter. This has, however, been materially modified, so as to bring the subject up to the present state of our knowledge, as rendered necessary by the rapid progress made in the science of solar physics.

IN the form of a 4to pamphlet is printed "Discussion of the anemometrical Results furnished by the Anemometer at Sandwick Manse, Orkney, 1863-1868, from the Appendix to the Quarterly Weather Report for 1871.

WE have received three papers by Prof. O. C. Marsh, on "The Discovery of Additional Remains of Pterosauria, and of the Dermal Scutes of Mosasauroid Reptiles;" "The Structure of the Skull and Limbs in Mosasauroid Reptiles," and a "Preliminary Description of Hesperornis Regalis." These have already been alluded to in our *résumé* of the contents of *Silliman's Journal.*

WE have a reprint from the *Geological Magazine* of Mr. James Geikie's valuable and interesting papers "On Changes of Climate during the Glacial Epoch." The author has made several additions and alterations, one of the most valuable additions being a summary of the general results arrived at in the memoir in a series of thirty-six short paragraphs.

THE *Notizblatt des Vereins fur Erdkunde und verwandte Wissenschaften zu Darmstadt und des Mittelrheinischen Geologischen Vereins* for 1871 contains a host of valuable papers and statistical tables connected with all branches of geography—physical, political, and social, and the sciences connected therewith, especially meteorology and geology, relating to the Grand Duchy of Hesse-Darmstadt. We notice especially two very elaborately-constructed charts of the meteorological observations taken during the years 1870 and 1871.

THE *Chemical News* contains a paper by Mr. Hugo Tamm on "A Ferro-Tungstine, a New and Interesting Mineral," which Mr. Tamm desires to designate "Crookesite." This name, however, having been already appropriated, Mr. Crookes pro poses that, when the missing constituent is discovered, it should be called "Tammite" Mr. Tamm's analysis of the new mineral gives:—Metallic Tungsten 88·05, Metallic Iron 5·60, Metallic Manganese 0·15, Undetermined Substance 6·20. Mr. Tamm ventures upon various conjectures as to this last substance, and thinks it just possible that, after all, he may have made an analytical error, and that there may be 6·2 per cent. more of tungsten than appears in the analysis. He has, however, strong doubts on this point.

Regarding "sea-serpents," the following note may be interesting:—The South African Museum, Cape Town, recently received a specimen of the Ribbon fish (*Gymnoterus*) fifteen feet long without the tail. It appears that this fish is known to distant inland fishermen as being forty feet long, and from its slender shape and snake-like movement is probably the "sea serpent" of late years so minutely described by navigators From its head there is erected a plume of flexible rose-coloured spines, and from head to tail along its back there is a conspicuous mane-like fin. Its general colour is like burnished silver. The eye is large and silvery, and the profile of the head comports well with that of the horse. The specimen could not be preserved, but there are two smaller specimens in the Museum.

The *Garden* calls attention to the great value of the Island of Jamaica as a tropical garden. Its oranges, pine-apples, bananas, limes, lime-juice, cocoa-nuts, and other such products, could not be surpassed in quality, and might be cultivated to any imaginable extent. Besides all this, the soil and climate are eminently suitable to the growth of precious drugs and plants. Bark is raised easily, the cinchona plantation being in a most satisfactory state. Then there are hemp and China grass of excellent quality, nor would any arrowroot be superior to that of Jamaica if it were but more carefully prepared for market. Here, it will be said, is a noble prospect for the colony. True, but it is a prospect only. Not until the very last returns is there shown any "tendency to the development of new industries requiring little capital and no extraordinary skill." It is the old story, "minor articles" are neglected, though they are the very articles which are wanted, and which the colonists could send However, Jamaica is fortunate in having a Governor in Sir J. P. Grant, who can discern the true capabilities of the island, and the true place for its industry in the markets of the world.

A correspondent of the *Madras Mail*, quoted by the *Times of India*, says that on the night of June 15 last the plain to the east, north, and north-east of Nandidroog was covered with "many thousands" of lights, which have been observed occasionally in former years. The correspondent compares the appearance to that of a large city brilliantly illuminated, and in one direction the scene, through a binocular glass, "looked like a view of part of the starry heavens, each flame being like a star." As many of the lights were from ten to fifteen miles distant from the reporter's point of view, he conjectures that each flame must have been five to six feet in length. An attempt is being made to find out the cause of the curious phenomenon, the most likely hypothesis being that the lights are "caused by the ignition of some inflammable gas escaping in jets from the surface of the earth."

We learn from the *Field* of July 20, that Mr. Parnaby has succeeded in bringing sixty black bass fry home from America, and that they are safely deposited in the tanks at Troutdale, Keswick, and are feeding heartily, so that they may now be considered safe. He found great difficulty in collecting the fry and bringing them safely across the Atlantic on account of the intense heat. Mr. Francis considers this the second greatest feat in pisciculture, the first being the conveyance of salmon to Australia.

PROFESSOR AGASSIZ'S SOUTH AMERICA EXPEDITION*

III.

In Mayne Harbour, on the western side of Owen's Islands, I had an opportunity of investigating two very interesting new genera of naked-eyed Acalephs. The locality naturally suggested appropriate names, and I called one after Captain Mayne, *Crossotoca Maynei*, from the festooned disposition of the ovaries, and the other after my old friend Owen, *Staurotoca Owenii*, from the cross-like figure of the ovaries. While I was collecting on board, Pourtales and Steindachner ascended the adjoining hills in search of glacial marks and land animals. The result of their excursion was most satisfactory. Pourtales found very well preserved glacial scratches and furrows upon wide greenstone dykes, which here intersected the rocks in every direction. Upon one such ledge the marks were divided into two distinct series, one running S.W. by W., the other crossing these S.S.W. Higher up on other dykes of the same character, the first series of marks occurred again, being perfectly rectilinear, in the same direction, and though in one instance interrupted, were renewed on the other side of the break on the same level with the same bearing. Still higher up on the same hillside he found also very distinct glacial furrows upon granite ridges, the furrows bearing S.W. by S., and finer lines again on greenstone dykes. The highest marks in the locality were some 500 or 600 feet above the sea level. Steindachner collected frogs and their tadpoles, and some insects and earthworms.

On approaching the Guia Narrows, the hills on Chatham Island are plainly furrowed in a north-westerly direction, and large boulders are seen all along upon the ridge of the range, while Esperanza Island appears in the south like a large rounded dome between two channels running N. and S. In wide channels we saw many whales and also small icebergs. The hills to the height of about 2,000 feet were everywhere distinctly *moutonnées*. Saumarez Island, opposite the mouth of Eyre Sound, and the island to the west of it, were particularly instructive. We followed the western channel, which is also the narrowest, and it soon became plain that wherever opposite shores with high walls approached near one another the glacial scratches and furrows, alike distinct, assumed an ascending direction, as is the case whenever a moving glacier meets an obstacle. That the south side was here also everywhere the strike side, was equally apparent from the facts that all these marks were either wanting or less distinct on the north side of the islands. Had any abrading agent advanced from the North, all appearances must have been reversed in these narrow channels; or they must have crossed them at right angles had the action come from East or West. Floating ice is out of the question where the furrows are not horizontal, and here in the narrowest part of the channel, west of Saumarez Island, there is a track where the scratches and furrows are distinctly ascending on the west side of the channel, and horizontally on the eastern side opposite, showing that the pressure of the ice-sheet must have been from S.E. to N.W.

Looking south, after passing Saumarez Island, the scenery appears totally different, from the fact that this is the lee side of the glacier action; and yet the channels have about the same width and bear the same relations to one another. In the narrowest channels the polished surfaces, with their scratches and furrows, are as well preserved and as distinct as in those of the Helle Platten at the Handeck in the valley of the Hassli in Switzerland. About Iceberg Sound all the mountains are beautifully rounded and *moutonnées*. That local glaciers, however, existed, and extended much beyond their present range, may be plainly seen in many of the inlets crossing the main channels in an east-westerly and west-easterly direction. It is true that general and local glacial phenomena are so interwoven throughout this region that it is at times difficult to appreciate their true connection; but there are also many localities where the difference is quite obvious. The most interesting of the places here have been well photographed by Dr. White, and may serve at some future time as illustrations of the fact described in this report.

In some places the various kinds of glacial marks were as plain as in the valleys of Switzerland, and I am surprised that travellers who have visited this region since the glacial phenomena have been so much discussed, have failed to notice them here. As in Switzerland, there seems to be a level above which the ice-sheet has never risen; at least there is a line above which the mountain ridges remain jagged and abrupt, while below their crusts the whole land is *moutonnée*. The abrasion by ice is so uniform and so general that I found it difficult to trace the direction of the abrading motion. There seemed to be nowhere a distinct lee and strike-side among the hills. But, as I grew more familiar with the appearance of the country, I became satisfied, and succeeded in convincing others also, that the abrading movement had taken place from the south northward—or, in other words, had been connected with the climatic condition of the Southern Hemisphere In Smyth's Channel there is no possibility of mistaking the evidence. I know no more interesting locality for the study of glacial phenomena than the vicinity of Saumarez Island. It shows in the most palpable manner that glaciers only—that is, terrestrial masses of ice moving upon solid ground—can have produced these abrasions, that floating icebergs cannot have been the cause. Their direction is such, also, that no one could suppose the adjoining cordillera to have been in any way connected with the abrasion or planing of the rock, or with its grooving and furrowing. The country has everywhere a glacier-worn aspect as far as the Gulf of Pennas. On reaching Chiloe I noticed that the rounded knolls became somewhat less marked, but yet the prominent trend of the hills continued to be in a north-southerly direction. An observer not familiar with the character of glacial denudation may sometimes be perplexed by finding the seeming lee and strike-side of the rocks in a position exactly the reverse of the general one. A critical scrutiny shows that these appearances are due to a superficial disintegration, often producing a rough side of a hill or rocky ledge where the observer of glacial phenomena would expect a smooth and polished surface. This is especially the case here, where, from the character of the stone as well as from the climate, the rock peels off and splits up very readily. One must be careful not to blinded by local appearances to the more general phenomena. At the entrance of Corner's Cove, for instance, a beautiful inlet trending east-west in Messier Channel, the rocks forming the southern and northern entrance might seem at first sight to have been ground or smoothed by a local glacier, moving out of the cove in an east-westerly direction. Seen however from a certain distance, where the local disintegration is merged in the general aspect of the exposed surfaces, the direction of the main abrasion from south-northward becomes as plain as daylight. You can trace rectilinear furrows upon the knolls both south and north of the entrance of the cove, following not only the same direction, but occupying the same identical level on both sides. There can be no doubt that they were continuous. Darwin has stated that the erratics, the only part of the ancient glacial phenomena observed by him in these regions, follow everywhere the course of the main channels, and he believes this to be an evidence of iceberg action. Valuable as are his results, being, indeed, almost the only connected geological observations ever made in this region, he is mistaken in his facts upon this point. Whenever we entered an inlet opening at right angles into the main channel and intersecting several parallel ridges of hills, the *roches moutonnées* and all the accompanying glacial phenomena trended in a south-northerly direction; as they did also in the main channel. Before entering the Gulf of Pennas, in Messier Channel, we passed an opening through which seven parallel ridges could be seen on the eastern side and five toward the west, all trending mainly northward, and plainly exhibiting glacier-worn surfaces.

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* Reprinted from the *New York Tribune*, concluded from p. 231.

Moreover, the Strait of Magellan itself has a main trend from east to west, and yet there is no sign throughout the whole length of any transportation of erratics from east to west, or from west to east. Dawson has made a similar mistake with reference to Switzerland. He supposes that the erratics of the Jura were deposited by icebergs sweeping up and down the great valley of Switzerland, from east to west and from west to east. He seems not to know that the older Escher von der Linth and Leopold von Bach had already clearly demonstrated the line of their transportation across the valley of Switzerland from south to northward; and that Guyot, more than twenty-five years ago, traced the different tracks of those boulders separately through the chief valleys of Switzerland northward across the very road which Dawson would have them follow.

The erratic pebbles and boulders from the eastern to the western coast of Patagonia, judging from my observations at Montevideo, in the Bay of San Mathias, in Possession Bay, at Sandy Point, in all the ports of the Straits of Magellan which we have visited, at Shell Bay, on entering Smyth's Channel, throughout Smyth's Channel itself, and upon the shores of Chiloe, have the same character. They consist of a mixture of plutonic and metamorphic rocks, among which the hardest siliceous rocks prevail. Their geological identity is further shown by the unfailing presence of a very hard, compact, epidotic rock, never absent from these erratic materials, yet never found in place, as far as I know, over the whole extent of country through which I have traced them. You will remember that I mentioned it among the loose pebbles of San Mathias Bay; nor did I lose sight of it until we left San Carlos, at the northern end of Chiloe Island, where I found it again, and as you will soon see, in still more interesting juxtaposition. This fact is of great significance, inasmuch as it shows that the drift phenomena in this region cannot have been due to the enlargement of the present glaciers, otherwise the drift would consist mainly of the rocks in place, and differ from one locality to the other. And yet their glacial origin is unmistakable, since a considerable proportion of these pebbles and boulders are polished, scratched, grooved, and furrowed, like the erratics of the United States and of Northern Europe. It is this uniformity in the character of the drift which has led me from the first to discriminate between the glaciers as they exist, and even as they once existed in their greater extension, in short, between all the phenomena connected with local glaciers, and those belonging to what I have called the glacial period, during which the two hemispheres must have been capped with a sheet of ice of enormous thickness and extent. The equatorial limit of this ice-sheet, both in the northern and southern hemisphere, is part of the problem upon which we have thus far fewest facts in our possession. In South America I have now traced the facts from the southernmost point of the continent uninterruptedly to 37° S. latitude, on the Atlantic as well as the Pacific coast. Even here at Talcahuano, large erratic boulders and *roches moutonnées* exist at the mouth of the Biobio on the hills of Hualpen.

In San Carlos de Anend, at the northern end of Chiloe Island, I have observed a fact which introduces a new element in the study of the glacial period. The ground upon which San Carlos is built is volcanic: the promontory of San Carlos consists of a volcanic breccia, the precise age of which I had no means of determining. From its mineralogical character, it must belong to the age of volcanoes proper. Now, erratic materials, small pebbles, and large boulders, among which some exhibit unmistakable glacial polish, rest in considerable quantity upon this volcanic ground. It is therefore plain that the glacial period in this part of the world, at least, has followed the older volcanic eruptions. Among these erratic materials the green epidote which I had followed so far was still to be found. The facts observed by me at San Carlos, taken in connection with Pourtales's discovery of a great many extinct craters near Possession Bay, point to the possibility of climatic changes in this region, which, should similar facts be found elsewhere, may account for the glacial period. At all events, it shows a direct connection between the glacial period and volcanic phenomena. Since finding drift upon volcanic ground at Anend, I have been watching for erratic pebbles and boulders of volcanic rocks along the coast of Chili. Their presence near the shore would prove that the glaciers of the Andes formerly reached the sea-level, after crossing the coast ranges in the temperate, and perhaps also in the tropical zone. Thus far I have failed to find anything of the kind. Darwin assumes that the erratics of western Patagonia have descended from the Andes, and he compares the outlying islands, such as Chiloe, in their relation to the Cordeillas, with the chain of the Jura in its relations to the Alps. But the erratics of Chiloe have the same character as those of the Strait of Magellan and of San Mathias Bay and the two latter, and those of the two latter can hardly be referred to this source. Neither did I see any indication of very large glaciers coming down from the Andes in a westerly direction, though I have no doubt that I shall find them farther north. Evidently we are not yet sufficiently advanced in our journey from the southern extremity of the continent northward that the influences of altitude should outweigh those of latitude in the increase and decrease of those climatic conditions upon which the extension of glaciers has depended in former ages. During the waning of the glacial period, the glaciers of the Cordillera have unquestionably been much more extensive than now, and I shall not be surprised to find, upon a more careful survey, that the glacier of Snowy Bay in Smyth's Channel and those of Eyre Sound, and perhaps some of the other parts of the Cordilleras, once crossed the main channel and reached the opposite island. But I doubt that they ever reached the shores of the Pacific Ocean. It is at all events certain that the local glaciers of the present time have never had the power in their greatest extension, or lasted long enough to obliterate or even obscure the phenomena of the glacial period. To refer the latter to an enlargement of the present glacier is simply absurd.

This leads me naturally to some remarks about the present glaciers of South America, of which we have seen great numbers during our journey. On the whole the glaciers of these southern regions recall those of Switzerland, with which I am so familiar. And yet there are marked differences also. The form of the mountains in the Straits is not favourable to the accumulation of large masses of snow, in extensive depressions and troughs like those from which the river-like glaciers of my native country descend. There are some of that character, it is true, on the highest ranges bordering Magdalena Bay and Gabriel Channel, such as Mount Sarmiento, Mount Buckland, and no doubt also Mount Darwin, though the latter were too far out of our track to be examined. Of course, as we have approached the range of the Andes with its deeper valleys, I have seen more glaciers with an Alpine character. But most of the glaciers of the Straits are dome-like, with an indented edge marking the limits where the glacial ice moved down beyond the névé. It is already known to all students of glacial phenomena that these southern ice-fields have the same general aspect, produce the same effects, and are bordered by the same loose materials, as those of other countries. But it is interesting to find that, like the glaciers of Switzerland, those of the Straits of Magellan have had a much greater extension in past times, and have gradually shrunk to their present size and relations. I have studied these facts in one of them very carefully, choosing for that purpose a glacier occupying a gorge on the northern side of the Straits. I preferred the northern side, because a glacier moving from the north southward must necessarily have encroached upon the area covered, at a still earlier time, by the Antarctic ice-sheet moving from the south northward. By the way, our party agreed, at my suggestion, to call this glacier the "Hassler Glacier," in remembrance of the Coast Survey and of the vessel in which our trip was made. It lies in what is known as Glacier Bay, so marked on the Admiralty maps, made from the combined observations of Capts. King, Fitzroy, and Mayne.

I expected to find here all the "facts" now accepted by geologists as evidence of the former greater extension of glaciers. I looked, in other words, for polished ground and furrowed surfaces, for dykes and strata on edge abraded to one level with the surrounding rocks; for moraines on a higher level and at greater distance from the ice than those at its present terminus; for erratic glacial materials of all kinds in the trough formerly occupied by the ice, and even for the peculiar scooped surfaces, called *coups de gouge*, on otherwise level slopes of rocks. I was not disappointed. All these signs are as legible about the Hassler glacier as they are in the neighbourhood of the glacier of the Aar, or that of the Rhone, and I found, besides, what is quite as characteristic, namely, a small lake shut into its basin, and kept there by an old moraine, 500 feet above the trough of the valley. There can be no doubt that this glacier once filled the whole bay down to its entrance into the main channel of the Straits, that is, three miles beyond its present termination.

Although I made a more careful examination of this glacier than of any other, we saw many local glaciers descending from the south northward, or from the north southward through similar gorges toward the main channel of the straits, and in Smyth's Channel also we passed many glaciers moving down from the W. and E. through valleys on either side of the Channel. Along our whole course we met with like evidence that all these ice fields have had a greater extension in former times. From a general survey of these appearances, it is plain that all phenomena connected with local glaciers and their former extension are independent of those produced by the more universal accumulation of ice during the glacial period proper. They form, of course, a consecutive phase—the last phase, indeed, of the waning glacial period during its passage into the present condition of things. By what combination of circumstances the glacial period was ushered in cannot be determined as yet; but after seeing the dispersion of the drift in a south-northerly direction over this part of the South American continent, and observing the relation of the local to the general glacial phenomena, I protest anew against the confusion introduced into the subject by those who imagine that what I have called the glacial period was produced by the gradual enlargement and subsequent shrinking of the glaciers now in existence.

You see that my anticipation of finding drift phenomena here independent of any local glacial action, has been realised on a greater scale than I had dared to hope. I most earnestly wish the European geologists would make a special investigation of glacial tracks upon the summit of high table lands and of mountain ranges, where, from their position, these characteristic marks cannot be traced to other ranges in the neighbourhood rising to greater heights. The true way to study general glacial phenomena is indeed to trace them over disconnected mountain surfaces, which were once entirely covered by the great ice mantle of the glacial period. Such localities I have already pointed out in New England and in Great Britain. Several appear to exist in Scandinavia also. It is most important to discriminate between the local and the general phenomena. Until this is done, we shall never understand the true relations of the facts.

Let me state that I have not noticed anything to confirm the idea that the glaciers of the northern hemisphere have alternated with those of the southern hemisphere in their greatest extension, as is assumed by those who connect with the precession of the equinoxes the difference of temperature required for the change. The abrasions of the rocks seemed to me neither more nor less fresh in one hemisphere than in the other; nor do the veins of molten rocks rising above the surrounding disintegrating rocks stand out in a more or less bold relief in either case. However astronomical causes may have been connected with the climatic conditions of the world, I see no reason for believing, from any facts I have observed, that alternations of temperature in the northern and southern hemispheres have ever been the primary and efficient clause of glacial phenomena. The more I consider these phenomena, the more am I satisfied that ice has been the great paring machine by which rocky surfaces have been fashioned. The great geological agents are not alone fire and water, as is universally admitted. Ice has had a great share in the work, and I believe this also will sooner or later be recognised with equal unanimity. After having traced what seems to me palpable evidence of an ice mantle over-spreading once the southern part of this Continent, the effect of which I have seen from Monte Video on the Atlantic to Talcahuano on the Pacific coast, the question naturally arises how far the southern extremity of Africa, as well as New Zealand and Australia, were involved in the extension. I hope I may live to see younger naturalists investigate these regions with the same object. I believe that whenever such an investigation is undertaken by a competent observer it will be found that over and above all local glacial phenomena, and still by side with them, there is also evidence of a southern circumpolar glacial agency.

You may think that I have given you too many details. I have done so purposely that no one may accuse me of basing theories on imperfect observations. I am well aware that my results will be questioned, and I shall be thought fanciful by geologists of all schools, as I have been at every step of my glacial researches. But an old hunter does not take the track of a fox for that of a wolf. I am an old hunter of glacial tracks, and I know the footprint whenever I find it.

While I was transcribing this report, Pourtales came in with the statement that he had noticed the first indications of an Andean glacier in this vicinity. I have visited the locality twice since. It is a magnificent polished surface, as well preserved as any I have ever seen upon cold glaciated ground, or under glaciers of the present day, with well marked furrows and scratches. Think of it! A characteristic surface indicating glacier action in lat. 37° S., at the level of the sea! The place is only a few feet above tide level upon the slope of a hill on which stand the ruins of a Spanish fort, near the fishermen's huts of San Vicente, in the Bay of San Vicente, which lies between Concepcion Bay and the Bay of Aranco. Whether this polished surface is the work of a glacier descending from the Andes to the sea shore or not I have not yet been able to determine. I find no volcanic pebbles or boulders in this vicinity, which, after my experience in San Carlos, I should expect all along the shore if the glaciers of the Andes had descended to the level of the ocean in this part of the country. The erratics here have the character of those observed farther south. It is true the furrows and scratches of this polished surface run mainly from east to west; but there are some crossing the main trend at angles varying from 20° to 35°, and running south-east and north-west. Moreover, the magnetic variation is 18°.3′ at Talcahuano, April 23, the true meridian bearing to the right of the magnetic. I shall soon know what to make of this, as I start to-morrow for the interior to go to Santiago and join the ship again at Valparaiso. The trend of the glacial scratches in San Vicente reminds me of a fact I have often observed in New England near the sea shore, where the glacial furrows dip to a considerable extent eastward toward the deep ocean, while farther inland their trend is more regular and due north and south.

While in Talcahuano we have made very extensive collections of littoral marine animals, so that we now have an excellent basis for comparison with the results of the deep-sea dredgings, which Pourtales is going to make between this and Juan Fernandez. I shall make similar collections in Valparaiso, and in order to do so in the short time allowed me I take Dr. Steindachner with me.

I had almost forgotten to say that I have obtained unquestionable evidence of the cretaceous age of the coal deposits of Lota and the adjoining localities north and south, which are generally supposed to be tertiary lignites. They are overlain by sandstone containing baculites. I need not adduce other evidence to satisfy geologists of the correctness of my assertion. I have collected myself a great many specimens of these fossils in beds resting upon coal seams. L. AGASSIZ

To Prof. Benj. Peirce, Superintendent U.S. Coast Survey

ANATOMY

The Placenta of the "Tamandua" Ant-eater

To the last number of the *Annales des Sciences Naturelles* M. Alphonse Milne-Edwards contributes an important paper upon the structure of the placenta of the "Tamandua" ant-eater (*Tamandua tetradactyla*)—important, at all events, to those who, with us, reckon the *Bruta* as one of the most interesting, but, at the same time, spite of Dr. Gray's most elaborate but somewhat intricate arrangement,[*] least satisfactorily classified groups of the mammalian class.

M. Milne-Edwards, after mentioning his countryman Lenou's division of the ant-eaters proper into three genera—viz., the terrestrial *Myrmecophaga*, and the *Tamandua* and *Cyclothurus* with arboreal habit and correlated prehensile tail, draws attention to the fact that as yet no opportunity has been had of examining the fœtal envelopes of the great ant-eater (*M. jubata*), that the placenta of the two-toed genus *Cyclothurus* is described in the *Leçons* as a kind of concave disc, but it has not been determined to what extent the walls of the ovum are occupied by the specialised vascular tuft.

The fœtal specimen of the Tamandua examined by M. Milne-Edwards was derived from a female which had come from New Grenada. The placenta is described as situated at the end of a pretty long and cylindrical umbilical cord, in which the vessels did not take any special course. It occupies a considerable extent of the ovum, and though circular and made up of but a single lobe, is of a form too convex (*trop bombée*) to come under the category of the so-called "discoidal" placenta. It would be, in fact, more correct to term it a "placenta discoidale envahissante." It is not made up of single villosities, such as the placenta of Pachyderms, of Camels, and of Tragulines, for the vascular tufts are much crowded together, especially at the central portion, so as to give the organ at this point a spongy appearance. The edges are sharply defined, leaving that portion of the chorion smooth which corresponds to the neck of the uterus. The vascular vegetations do not, in their disposition, remind one in any degree of the reticulated fold and the honeycombed aspect described by Sharpey as occurring in the placenta of the Pangolin. Towards the centre there appeared to be *débris* of the uterine tissue, suggesting the existence of a "decidua," but on this point there is, unfortunately, no certainty. No trace of an allantois was discovered, from which it is concluded that this fœtal appendage must be at least greatly reduced in size. Owing to the long immersion of the animal in alcohol, it was impossible to dissect out the laminæ of the envelopes of the ovum, or the factors of the umbilical cord. The internal surface of the chorion is stated to have been perfectly smooth, and not to have presented any of the protuberances which have been observed on that of the Unau.

If the placenta of the Tamandua, remarks M. Milne-Edwards in conclusion, be compared with that of some other members of the groups into which the *Edentata* have been subdivided, we shall not fail to be struck with the considerable differences which seem to exist in the structure of this organ in the different members of a group considered by zoologists as constituting but a single order.

The figures given by Carus (*Tabulæ Anatomiam comparativam illustrantes*, Pars. iii., Pl. ix., fig. 15), of the placenta of the Unau, do not, in the eyes of M. Milne-Edwards, resemble that of an ant-eater, of any other kind of Edentata, or even of any Mammal.[*] According to Prof. Owen's description of the placenta of the "Tatou,"—a general term for the Armadilloes—this organ resembles, at least in general form, the discoid placenta of an Insectivore, while that of the Pangolin, described by Huxley "Introduction to the Classification of Animals," p 98), after Sharpey, presents a third mode of organisation not less distinct from the preceding. The Tamandua, in fine, thinks M. Milne-Edwards, offers an arrangement which, though differing in some particulars from that existing in *Cyclothurus*, seems to be only an exaggeration.

M Milne-Edwards concludes by putting the pertinent question —"are we to regard this diversity in the order *Edentata* as of less importance than that accorded by naturalists to like variations in the fœtal envelopes in other groups of the class Mammalia? or are we to conclude that the different zoological types included by zoologists under the name *Edentata* have less affinity between them than is generally believed, and might be represented in our system of classification by division of a higher character." M. Milne-Edwards inclines—and in this we feel also disposed to follow him—to the latter proposition, and proposes at some future time to discuss and enlarge upon the same.

J. C. G.

METEOROLOGY

On a Meteoric Iron lately found in El Dorado County, California[†]

For my knowledge of the meteoric iron of El Dorado Co., I am indebted to Mr. Alfred Stebbins, librarian of the Mercantile Library Association of San Francisco. A letter from him, dated April 26, inclosed a few grams of turnings obtained during the separation of a slice of the mass destined for the collection of the geological survey now in progress under the direction of Prof. Whitney.

The mass is described by Mr. Stebbins as having the size and shape of a man's head. It was found in a field, and, as usual, was first taken to a blacksmith's shop, where it was soon found to be an unmanageable subject for working, and hence, fortunately, found its way into scientific hands. Its surface possesses the indentations common to these bodies—the crust or coating being partially oxidised. It weighs eighty-five pounds.

I find the turnings to have a specific gravity of 7·80, which may perhaps be a trifle above what the mass possesses, as it is presumable that the turnings have suffered a slight condensation in the process of separation.

The fragments sent are free from all traces of sulphur. A single analysis upon one gram has afforded me,

Iron	88·02 per cent.
Nickel	8·88 ,,
Insoluble, consisting of a mixture of Fe_2O_3 and FeO, with minute silvery particles of supposed phosphor-metals (Schreibersite)	3·50 ,,

The amount of material at command was too small to search for the other metals commonly found in meteoric irons.

SCIENTIFIC SERIALS

Le Moniteur Scientifique, April, 1872. This number commences with a translation of a paper by M. Mayer, on alcoholic fermentation, and on the nutrition of the yeast plant, and is followed by a long dissertation on scents, according to recent discoveries in chemistry and physiology, by M. Papillon. The next is a translation of a lecture by Dr. Hofmann on organic chemistry and therapeutics. The author points out the numerous discoveries which have advanced the science of medicine, showing also clearly that to the remarkable and rapid development of this branch of chemistry during the past few years, is due the more scientific and complete system of therapeutics now obtained. He also points out in how many instances medicine has derived immense benefit from the discoveries of new compounds, which at first seemed only to possess a theoretical interest to chemists.—A paper on Anthracene and its derivatives, follows, by M. E. Kopp. It treats, in the first instance, of the formation from anthracene of dichloranthracene, and on the action of sulphuric acid on this body. This yields an acid called disulphodichloranthracenic, and which possesses in dilute aqueous solutions a most intense and beautiful fluorescence in the blue end of the spectrum. The salts of this acid, the soluble ones of which exhibit the same property, are here described. This acid, by simply heating or by the aid of oxidising agents, is converted into disulphanthraquinonic acid. Dibromanthracene may be made to yield an analogous series of compounds, which in some instances are also beautifully fluorescent. Disulphanthraquinonic acid, heated strongly with a caustic alkali, is converted into alizarin, which is identical with the colouring matter obtained from madder root. In this paper a description is given of the methods proposed by the various patentees in the matter, thus forming a tolerably complete history of this new branch of industry. Some space is also occupied with a discussion as to the state in which alizarin occurs in garancin; the balance of evidence seems to show that it is glucoside, which may be called ruberythric acid, and which by the assimilation of two equivalents of water, forms alizarin and two equivalents of glucose.—M. Blossum contributes another part of his memoir on Caoutchouc and Gutta-percha considered from a chemical standpoint. In this article he discusses the vulcanisation of caoutchouc, and the manufacture of the softened variety, giving Parkes' process for vulcanising, which consists in exposing the articles to the action of a mixture of carbonic disulphide and chloride of sulphur, after which treatment they are boiled in dilute soda solution; the same communication also deals with vulcanite and the vulcanite employed in dentistry.—Dr. Benrath has a paper on the Chemical Theory of Devitrification. The author has made numerous analyses which show reason to think that a part of devitrification is due to a separation of silica, which was previously held in solution by a silicate. The number finishes with a critical analysis by M. F. Papillon on the recent work of M. Ritter, "On the Relation between the Modifications of the Blood Corpuscles and the Modification of the Excretions," which appears to be a very valuable work.

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[*] "Revision of the Genera and Species of Entomophagous Edentata." Proc. Zool. Soc., April 11, 1865.

[*] Rapp seems to have made more out of Carus's plates than did M. Milne-Edwards, for he states (*Anatomische Untersuchungen uber die Edentaten*, 2te Aufl, p. 103. Tubingen, 1852), that according to the said anatomist, the placenta in this animal is made up of several *cotyledons*, which are from half-an-inch to an inch in transverse measurement.

[†] By Charles Upham Shepard, Sen., Massachusetts, Professor of Natural History in Amherst College. Reprinted from the Amer. Jour. Science and Art.

THE *Journal of the Franklin Institute* for June contains, besides papers to which we have specially alluded, continuations of papers previously commenced, and the usual Editorial items and novelties, the Report of the Committee of Judges upon the Trial of Steam Boilers, American Institute, 1871; experiments on various coals of the Carboniferous and Cretaceous periods, an article on the gunpowder pile driver, by F. C. Prindle, C.E., with a plate; one on the great fires of 1871 in the North-west, by Prof. J. A. Lapham; and one on the utilisation of the light petroleum oils, by W. H. Wahl.

THE *Archives des Sciences physiques et naturelles* of Geneva, No. 174 for June 15, commences with an interesting article by Alph. De Candolle on the question whether modifications in vegetable species are caused by prolonged influence of climate. For this purpose he obtained from remote localities in Europe, Moscow, Edinburgh, Montpellier, and Palermo, seeds of widely-distributed plants, and sowed them in the same soil and at the same time in Geneva. Although the series of experiments was not sufficiently extensive for the conclusions to have any decisive value, the general result was that the seeds obtained from the more northern localities germinated somewhat earlier than those grown in more southern latitudes, and the plants resulting from them also came to maturity somewhat more rapidly, a difference which was more decidedly manifested in the second generation. If these results are confirmed by a more complete investigation, they will be of considerable importance in the question of acclimatisation.—The only other original articles in this number are by M. Ador on phtalyl, the radical of phtalic acid, and on the increase of intensity of voltaic induction currents, by Prof. Lemstrom.

IN the *Journal of Botany* for July, Dr. Braithwaite continues his series of papers, "Recent Additions to our Moss Flora," this instalment being illustrated by two plates, of *Splachnobryum Wrightii*, and several species of *Grimmia*; and the Rev. Eugene O'Meara contributes further researches on the Diatomaceæ. Two useful local lists of flowering plants are also given, by Dr. M. M. Bull, of the Island of Sark, and by Mr. J. F. Duthie, of the Islands of Malta and Gozo.

THE *Quarterly Journal of Science* for July contains four original articles. The first is a short one, entitled "The Music of Speech," by the Rev. R. W. Higgs, consisting chiefly of an epitome of "The Philosophy of the Human Voice," by Dr. James Rush, of Philadelphia, who claims to have shown that "the sentiment and the logic of our speech have a distinct mode of expression apart from the subject matter." The article is interesting; but we must protest against the introduction into our language of such barbarisms as an "orotund," compounded from *ore rotundo*, and a "vocality," to express a vocal sound.—The second article is an able advocacy of the advantages of a uniform decimal system of weights and measures, compared with our present multifarious scales.—Mr. R. A. Proctor on "The Construction of the Heavens" gives an outline of the different theories which have been started as to the constitution of the sidereal system, especially those of the two Herschels and the elder Struve, and argues in support of his view that all the nebulæ hitherto discovered, whether gaseous or stellar, exist within the limits of the sidereal system.—The last article is by Captain Oliver on "Mediæval and Modern Ordnance and Projectiles compared."—The remainder of the number is taken up with notices of scientific works and reports of progress in physical and mechanical science.

Révue Scientifique, Nos. 51-53, and 2nd series, 2nd year, Nos. 1-4.—The report of M. Claude Bernard's course of lectures at the Collège de France on experimental medicine is brought to a conclusion with the close of the volume. Further reports are given of papers read at the Rostock meeting of the Association of German naturalists and physicians. M. L. Dumont has an article on civilisation considered as accummulated force. Report of M. Milne-Edwards' lecture on the Classification of Mammalia, being the introduction to his course at the Museum of Natural History on Zoology (Mammalia and Aves).—The new volume commences with a translation of Sir John Lubbock on the Origin of the Family. Sir W. Stokes's lecture before the University of Dublin on Public Medicine in Germany is translated. Of courses of lectures in France, we have M. de Quatrefage on the origin of the Prussian race at the Museum of Natural History; M. A. Chauveau, before the Society of Medical Sciences at Lyons, on the general physiology of virus; and M. Georges Ville, at Vincennes, on Chemical Manures. No. 4 contains an interesting sketch of the history of the Observatory at Greenwich, a sequel to the history of the Paris Observatory in an earlier number. We have also the usual amount of reports of the proceedings of foreign scientific societies.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, July 1.—Prof. Westwood, president, in the chair.—Mr. Jenner Weir exhibited two examples of a rare British lepidopterous insect, *Agrotera nemoralis*, captured by him in Abbot's Wood, Sussex.—Mr. Meldola exhibited varieties of several species of British Lepidoptera, and an example of *Leucania vitellina*, taken at Brighton in 1869.—Prof. Westwood exhibited several remarkable coleopterous insects sent from Ceylon by Mr Thwaites; also, from the same locality, banded cocoons of a species of *Ichneumonidæ* attached to threads nearly three inches long; and an illustration of the habits of some species of moth which cuts out large oval pieces from the leaves of *Citrus* and forms therewith a moveable flattened tent, beneath which it lives and undergoes its transformations.—Mr. Muller exhibited portions of the leaves of *Pteris aquilina* from Weybridge, attacked by three species of dipterous larvæ.—Mr. Dunning called attention to a letter in NATURE from Dr. Leconte concerning the parasite of the beaver on which Prof. Westwood had founded the order *Achreioptera*. Dr. Leconte considered the insect pertained to the *Coleoptera*. Prof. Westwood dissented therefrom.—Mr. Dunning also read extracts from an article in the same journal by Mr. Moseley, concerning the sound produced by the Death's-head Moth, in which the writer maintained that the noise proceeded from the proboscis, and was caused by the expiration of air.—Mr. Lewis brought to the notice of the meeting a circular addressed to entomologists (with a list of signatures appended thereto), urging them to ignore the re-instatement of forgotten names until such time as the method of dealing with them shall be settled by common agreement.

Meteorological Society, June 19.—Mr. John Tripe, president, in the chair. At the ordinary meeting, which preceded the Anniversary Meeting, Captain Toynbee exhibited charts showing the results already obtained in the meteorological office by the discussion of the observations for a portion of the North Atlantic, comprising ten degrees square, for the first four months of the year. The district extends from the Equator to 10° N., and is bounded by the meridians of 20° and 30° W. He explained the variations in the several elements from month to month which had been rendered visible by the minute discussion to which the materials had been subjected, and pointed out the importance to navigators of the precise information as to winds, &c., now presented to them. He stated that the meteorological committee intended to distribute copies of the chart for January, in order to elicit opinions as to the proposed method of publication. At the Anniversary Meeting which followed, the Report of the Council was read. In the Report the Council stated that as the number of Fellows showed a diminution from 340 to 314, it had been deemed advisable to introduce a change into the management of the society. Accordingly a room had been taken at No. 30, Great George Street, Westminster, and an assistant secretary appointed to attend there daily. The gentleman selected is Mr. W. Marriott, formerly engaged at Greenwich Observatory, and he entered upon his duties on May 1st. It is hoped that by these means the business of the Society will be conducted in a more satisfactory manner.—The fifth volume of the Proceedings being now complete, the Council have increased the size of the publication to royal 8vo, so as to allow of the binding up of the Registrar-General's Quarterly Returns with the Proceedings of the Society. The new series will be entitled the "Quarterly Journal of the Meteorological Society," and will be edited by a committee of the council, Mr. Glaisher having resigned the editorship. The Report concluded with the usual obituary notices of deceased Fellows.—The president then delivered an address, in which he said that he would allude briefly to some facts connected with meteorology and its correlations with sickness and death. The careful daily record of meteorological observations made with standard instruments was commenced at many stations some time before the compilation of mortality returns, in the office of the Registrar-General of Births and Deaths, so that the mortality tables of the metropolis can safely be compared with the Greenwich returns, and for extra-metropolitan localities with those supplied by any of our observers. He had carefully compared the Greenwich observations for some years with those made by himself at Hackney, by Mr. Burge at Fulham, Mr. Symons at Camden Town, and Mr. Heywood in the City, and ascertained that the mean daily temperature did not vary on an average more than half a degree, although the maximum and minimum observations differed very considerably. He had therefore used the Greenwich tables in all his comparisons between the rate of death from different diseases and varying states of the weather. A number of valuable results have already been obtained as regards the course of epidemics, the influence of high and low temperatures on the public health, and, to a less extent, of different hygrometric conditions of the air. Dr. Hoskins long since (about 1855) wrote a valuable paper on the "Correlation between Meteorological, Medical, and Agricultural Science;" and he (the president) commenced a series of essays in 1848 on the influence of variations in the temperature, moisture, weight, and electricity of the atmosphere on the death-rates of scarlet fever and other epidemic diseases. The Manchester Medical Association, Dr. Ballard, and others, have written on the effects of variations of temperature on the health of the people. The whole of the writers have arrived at tolerably uniform conclusions, viz., that very cold and very hot weather induce an increase in the number of cases of disease and of deaths, and that a temperature between 55° and 65° is most beneficial to health in this country. He stated many years since, in one of his reports, that a cold wet summer always coincides with a less amount of sickness and fewer deaths than a hot dry summer. It is somewhat singular that, whilst very cold weather causes a great increase in the sickness and mortality of any given population, and especially amongst the very young and very old, the increase should extend to almost all diseases. It is true that the chief sickness and mortality are caused by affections of the lungs; but there is also a greater number of cases, although not of deaths, even from diarrhœa. Thus the rate of death, in weeks having a mean temperature of less than 35°, was nearly 45 per cent. greater than in weeks having a temperature of 60° to 65°; and in weeks having a mean temperature above 65° the average rate of death was about 30 per cent. more than in weeks having a mean ranging between 60° and 65°. The range of temperature in this country which is the best for health is so small that every one should use reasonable care when the mean is above or below the standard; at the same time we must not forget that extremes are always injurious, whatever the average may be. This is especially the case as regards diarrhœa, for the mortality from this cause, with a continuance of the mean above 65°, is at least twenty times as great as at 40° to 45°. The comparison between temperature and epidemic diseases has led to the important facts that, as regards small-pox, it produces the smallest number of deaths as soon as the daily mean reaches 62°, and has continued a short time at that degree of heat, which is usually about the end of July or early in August, and does not become so fatal again until the mean temperature has sunk for a short time below 54°, which is generally about the end of September. This is not quite invariable, as it varies somewhat in epidemic and non-epidemic years. The fatality from small-pox increases as the temperature sinks below 54°, until the middle of January, when the lowest average temperature is ordinarily reached, viz., about 35·5°. Scarlet fever, on the other hand, is at its lowest point from the middle of March to the end of the third week in April, when the daily mean varies between 41·5° and 47·5°, from which it gradually increases in fatality as the weather gets warmer, but not quite at an equal ratio, until the end of October or early in November, when the impetus apparently given to it by the warm weather has ceased, and the mortality declines. He had very carefully examined the influence of other meteorological elements on the disease, and find all of them to be almost inert as compared with that of temperature. How far the temperature, moisture, and electricity of the atmosphere are concerned in exciting diseases to become epidemic, we are unable at present to state; but the periodicity which epidemics exhibit is opposed to these being the chief causes. Thus small-pox, scarlet fever, and measles, have a very decided tendency to become epidemic in the metropolis every fourth year, whilst there is no single meteorological element or combination of elements which has so decided a periodical excess or minus of its average amount. A record of correct observations extending over many more years than we now possess, and a close comparison of these with correct returns of sickness and death in any sufficiently large area will, he does not doubt, enable statisticians to determine the precise relations which exist between the state of the public health and meteorology. There is at present, however, so little known of the varying electrical conditions of the air (at least so as to measure the changes) that it is perhaps somewhat premature to express this opinion.

BOSTON

Lyceum of Natural History, November 13, 1871.—Dr. H. Endemann read a paper "On Meat and the Methods of Preserving it," in which he described the extract of meat made according to Liebig's process, and stated that its value is overestimated, as experiments have shown that the ashes of the extract are as nutritious as the extract itself. No organic substance has been found that will produce the effects of extract. He then described the process of salting meat, and showed that the salts used, as well as any water employed subsequently to freshen the meat, remove a large part of the extractive salts, leaving it difficult of digestion. Smoking depends on the carbolic or cresylic acid contained in the smoke, by which the albumen and fibrin are coagulated, hence the meat is not readily digested. One of the best processes for preserving meat is enclosing it in air-tight cans, but this often fails on account of mechanical difficulties. He proposed to preserve meat by cutting it into slices and drying it in a hot-air chamber, at a temperature below 140° F., which may be done within two hours. This dried meat is then ground in a mill. The fibrin and albumen are not coagulated, and will take up water. The apparatus used in the preparation of the dried meat, and its applications for soup, solid dishes, and for invalids, was also described.

November 20.—Prof. B. N. Martin, vice-president, in the chair. Prof. T. Egleston exhibited five crystals of Diamond and one of red spinel, from South Africa. Two of the diamond crystals showed the cleavage parallel to the octahedron, two were curved hexoctahedra. The fifth was a cube one quarter of an inch square, weighing 0·906 gms. The cube is a twin by interpenetration, and shows the faces of the rhombic dodecahedron on both crystals. The cube faces are all striated in the direction of the diagonals of the faces of the cube, and show, consequently, the tendency toward the octahedral form. The spinel was perfectly transparent, of a beautiful ruby colour. Its form was that of a hemitrope octahedron.—Prof. D. S. Martin exhibited specimens of a clay containing recent shells, from a deposit which had been the bed of a lagoon within quite a modern period, near the town of Lewes, Delaware. The shells are in very perfect preservation, though the epidermis is nearly gone, and the texture is becoming fragile and chalky. The principal species are *Sanguinolaria fusca*, *Nassa obsoleta*, and *Modiola plicatula*, of which the first two are now living on the beaches outside, and probably the last also. These specimens give an excellent illustration of the mode of formation of many of our fossiliferous clays and marls. The deposit may, perhaps, have value as a fertiliser. He also gave some description of the very remarkable sand-dunes or moving hills at Cape Henlopen, a mile or two east of Lewes. The sand brought down by the Delaware River accumulates at this point, and when thrown up on the beach, is taken in charge by the heavy east winds, and carried inland in a great line of drifting hills, which rises in a very long and gentle slope on the windward side, and falls off abruptly from the crest on the leeward, as is usual in wind drifts. The whole surface of the windward side is studded with the tops of dead tree trunks, the remnants of a pine forest, overwhelmed by the advance of the hill. The crest seems steadily approaching the lighthouse keeper's dwelling, and will, probably, necessitate its removal in the course of some few years. Prof. A. M. Edwards said the specimens just exhibited are of considerable interest, as they show very nicely the mode in which certain stratified rocks containing fossils are evidently formed. Under certain circumstances, say when formed in a locality like the tropics, where animal life abounded, and the mollusca especially occurred in large quantities, so that calcareous matter would accumulate, such a deposit might become, in time, converted into a limestone in which the forms of the enclosed shells and other organic remains would be preserved in a more or less perfect manner. If, on the other hand, calcium compounds were not present in abundance, but the particles of the deposit thrown down should consist of coarse and for the most part siliceous sand, sandstone, also enclosing fossils, would eventuate. But to me, the material of which the deposit exhibited consists, and which encloses the well-preserved remains of mollusca, is of more special interest, as this is the third time that such a formation has come under my observation, and I have studied one of these deposits with some care, as it proved to be, for the most part, made up of the siliceous skeletons of Diatomaceæ, to the consideration of which, both recent and fossil, I have devoted many years. All cases of marsh inversion are of interest to geologists, and the opportunities they present the microscopists of obtaining specimens for study makes them doubly attractive. The Hoboken and the Cape Henlopen specimens will be examined and reported upon hereafter.—Mr. Jas. Hyatt made some remarks on the occurrence of some plants in the vicinity of New York city. The cotton thistle, *Onopordon*, may be found at Fishkill Landing, on the Hudson River, a short distance from the railroad station, at the office of the iron works. He was able to secure flowers there for Dr. Torrey's collection. The plant has maintained itself there for several years. *Gentiana quinqueflora* abounds in South-Eastern Duchess County, and from thence he was able to furnish for Dr. Torrey's collection the only specimens from this State. *Viola rotundifolia* abounds at Weehawken, N.J., at the foot of the Palisades, west of the Ferry dock.

PARIS

Academy of Sciences, July 15.—M. de Pambour presented a further note on the reaction water wheel.—An important memoir by MM. Jamin and Richard on the cooling of gases was read.—M. A. Thenard described an apparatus for subjecting gases and vapours to the action of electricity.—M. Gaiffe described a new and cheap form of battery, consisting of a vessel in which are immersed a rod of lead and a rod of zinc, the former reaching the bottom, which is covered with a layer of aluminium; the exciting fluid in water containing 10 per cent of hydrochlorate of ammonia.—M. H. Sainte-Claire Deville communicated a note by M. J. M. Gaugain on the induction currents developed in M. Gramme's machine.—M. Faye presented a note by M. Respighi in reply to some criticisms of Father Secchi, upon his observations on the constitution of the sun.—M. J. A. Broun read a second note on the simultaneity of barometric variations between the tropics.—M. H. Tarry presented a note on the magnetic currents and solar explosions, which accompanied an aurora borealis observed on July 7. Upon this paper MM. C. Sainte-Claire Deville and Tissot made some remarks.—M. C. Sainte-Claire Deville also presented a note by M. J. Gay, describing cloud-shadows observed by him at the Grande Chartreuse, with reference to a recent balloon observation by M. Tissandier.—M. H. Sainte-Claire Deville communicated a note by M. A. Houzeau on the instantaneous oxidation of alcohol, in which the author described the conversion of alcohol into acetic acid and aldehyde by the direct action of ozonised oxygen.—M. C. Bernard presented a note by M. N. Gréhaut on the quantitative determination of urea by means of Millon's test and the mercurial pump.—M. C. Dareste communicated his discovery of the presence of starch in the young of the European fresh-water tortoise (*T. europæa*).—M. Des Cloizeaux read a further note upon amblygonite and montebrasite.—M. Daubrée reported upon a collection of minerals from Chili, offered by M. Domeyko to the School of Mines at Paris.—M. Sainte-Claire Deville read a paper on the absence of Combustible Gases in the emanations from the Caldeira of Furnas in St. Michael's. The same gentleman communicated an extract from a letter by M. H. de Saussure, giving an account of his observations upon the late eruption of Vesuvius in April of the present year, and made some remarks upon its contents. He also presented a note by M. Gorceix on the state of Vesuvius, and of the gaseous emanations of the Phlegræan fields in the month of June, 1869.—M. Milne Edwards presented a note by M. Wetelet upon the genus *Ovulites*, which the author regards as belonging to the Polyzoa. He describes a new form under the name of *Ovulluba margaritula*. M. Milne Edwards also communicated a note by MM. A. Grandidier and L. Vaillant on the fossil crocodile of Amboulintsatre in Madagascar, which they regard as a new species, and name *Crocodilus robustus*.

BOOKS RECEIVED

English.—The Thanatophidia of India: J. Fayrer (J. and A. Churchill).—Qualitative Analysis by Dr. C. R. Fresenius, translated by A. Vacher, 8th edition (Churchill).—The Battle of the Gauges renewed, 1872. R. T. Fairlie (E. Wilson).—Perspective, or the Art of Drawing what one sees: Lieut. W. H. Collins (Longmans).

American.—Description of the *Balænoptera musculus* in the possession of the Boston Soc. Nat. Hist.: T. Dwight (Boston Soc. Nat. Hist.).—Embryological Studies on Hexapodous Insects: A. S. Packard, jun. (Peabody Academy of Science).

Foreign.—Die Pflanzen Galiziens u. der Bukowina: J. A. Knapp.—(Through Williams and Norgate).—Révue d'Anthropologie, 1872, No. 1.—Zeitschrift der Biologie, 8 Band 2 Heft.—Ueber algenartige Einschlusse in Diamanten u. uber Bildung derselben: Dr. Goppert.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, AUGUST 8, 1872

THE GOVERNMENT AND THE SOCIETY OF ANTIQUARIES

THE Government has outdone itself. Mr. Lowe and Mr. Ayrton have added another to their many claims on the esteem of their admiring fellow-countrymen, another to their many efforts to place England at the front in all matters relating to culture, and let us add another to the many indications that if science and culture are to go on in any large sense here at all, there must be some very considerable change in our present arrangements.

This time it is not a question of refusing a monument to Faraday or a search after Livingstone, of insulting a distinguished man of science at Kew, or any point relating to the investigation of any phenomena, such as the tides, which it will be recollected "My Lords," after having given their "anxious attention" to, were good enough to characterise as of an "interesting nature." This time the Government has changed its tactics altogether. Not content with hurling refusals at those societies whose duty it is to remind the Government of the claims of the sciences to which they devote themselves, the Government has singled out a society, begged it to do certain work, of course at the expense of the members, and after this work has been well and promptly done, it has turned round, and practically told the society that it is a fool for its pains. This, of course, is a *coup de maitre*, one admirably adapted to keep the societies, as well as architects, sculptors, and gardeners down, and we can well imagine that Messrs. Lowe and Ayrton have enjoyed their Bank holiday with a greater relish with this on their mind. But there is another point of view in which the transaction is less satisfactory, and to point this out it is necessary to mention some details.

We gather from the *Times* that prior to Mr. Layard's appointment as Chief Commissioner for Works and Buildings in 1868, "the office had usually been bestowed on some member of the party in power for whom there was no convenient place in the Cabinet, yet whose claims could not safely be disregarded. Sir William Molesworth had occupied the position as a distinguished champion of Radical opinions. So had Lord Morpeth as a scion of the Whigs, and Lord John Manners as a representative of the Tories. Sir Benjamin Hall succeeded to the place not so much because he represented, like his predecessors, a great political party, as that he secured for the Ministry of which he was a member the confidence of the metropolitan vestries. After Lord John Manners' second term of office the place ceased to be one of Cabinet rank; but when, after a third trial of Lord John Manners, Mr. Layard came into office with Mr. Gladstone, a new theory as to the duties of the place appears to have been initiated. Mr. Layard, setting aside his political claims, was well known to the public as a distinguished archæologist and man of letters." One of Mr. Layard's first acts was to courteously request the Society of Antiquaries to furnish him with "a list of regal and other historical tombs or monuments existing in cathedrals, churches, and other public places and buildings," such as it might be desirable to "place under the protection and supervision of the Government, with a view to their proper custody and preservation."

Thereupon the Society of Antiquaries, whose aid Mr. Layard's predecessors—whose only thoughts had been of place and party—had never required before, set to work in a most vigorous manner. They passed resolutions which were forwarded to the Government; they corresponded with their local secretaries, they appointed a numerous "Sepulchral Monuments Committee;" they divided England and Wales into districts, and made most minute inquiries; prepared a list of 531 monuments, which they considered to be included in the terms of Mr. Layard's letter; and communicated them to the Government with a report pointing out the desirability of the proposed Government action as evidenced by the demolitions which had already occurred, and stating that the work had been one of enormous labour.

By the time the report was sent in, however, Mr. Layard had left the Office of Works, and Mr. Gladstone having apparently come to the conclusion that no culture was requisite for the head of that office, Mr. Ayrton had been appointed. When the Prime Minister rewarded the important services rendered by Mr. Ayrton to his country by placing him in a position of considerable emolument, it can readily be understood that the lucre formed but a small part of the reward; and on the receipt of a letter from the Society of Antiquaries, in March, this gentleman entered upon the sweets of office with a vengeance. The Society was very quickly informed (1) that Mr. Layard's letter, on which they had acted with such alacrity and diligence, had been written "without the authority of the Treasury having been at any time obtained;" (2) "that the First Commissioner has now been informed by their Lordships that they must decline to authorise him to undertake any duties in respect to the regal and historical tombs or monuments referred to;" (3) "that the object contemplated could not apparently be accomplished without legislation;" and (4) that there was "no intention either of introducing a Bill or of laying before Parliament the report which has been made by the Sepulchral Monuments Committee."

The *Times*, in commenting upon this strange conduct, distinguishes, in reviewing Mr. Ayrton's conduct, between "the responsibilities which weighed upon him and those which encumbered his predecessor;" because "Mr. Layard, plainly owing to the enervating influence of his artistic training and literary associations, felt that in the Office of Works it was his business to encourage the fine arts, to protect the great historic monuments of the country, and to preserve from the ravages of time or ignorance those priceless memorials of the past which may be neglected by their casual owners;" while "Mr. Ayrton came into office inspired with a faith the very opposite of this, and flushed with the success which his convictions, not wholly to the satisfaction of those with whom he came into contact, had obtained at the Treasury."

But we think that the *Times* is hard upon Mr. Ayrton, inasmuch as the Lords of the Treasury are let off scot-free. Any one acquainted with the ordinary working of our political system will have a shrewd suspicion that, if it had been a question of giving a place to some little living personage who had helped his party instead of looking after the monuments of those great men who have made England what she is, "My Lords, under the exceptional circumstances of the case," would have "been pleased to sanction the action of the First Commissioner." Moreover, it must be remembered that Mr. Layard was only First Commissioner, and that Mr. Gladstone as a Commissioner is responsible for Mr. Layard's action.

After all, however, perhaps it is well that, considering what we know of Mr. Ayrton's treatment of the living, he should have as little to do with our great dead as possible. Let their records vanish, let their sepulchral monuments disappear. What is this to the English Government? But there is a moral in all this which concerns the present. This treatment of a scientific society is the *ne plus ultra* of official Philistinism. It shows that any assistance rendered to the Government by scientific men or scientific bodies is rendered, as matters stand, at present, at their peril; and until some alteration is made, any expenditure of time and energy for Government purposes should be respectfully declined.

NEW RESEARCHES IN ENTOZOA

Beiträge zur Anatomie der Plattwürmer. (Leipsic: Engelmann. 1872.)

IN the first part of this serial work, just issued, the authors—Dr. F. Sommer and Dr. L. Landois, Professors in the University of Greifswald—confine their attention to the structure of the sexually mature joints or segments of *Bothriocephalus latus.* With excellent judgment they record the results of their own investigations in the first twenty-six pages, the remainder of the *brochure* being devoted to a critical comparison of the writings of other helminthologists from the time of Eschricht down to the latest period. This method, as they remark (s. 27), not only preserves the continuity of the record of a great number of frequently repeated observations and statements, but it also has the advantage of enabling their readers to discriminate between the results obtained by themselves and those acquired by earlier and equally independent observers.

So considerable a portion of our knowledge of the structure and economy of the tapeworms is due to the researches of their own countrymen, that no surprise need be expressed at the completeness of the analysis which they afford of the writings of Siebold, Leuckart, Böttcher, Stieda, and Knoch, of St. Petersburg. Nevertheless, we may remark that, although their analysis is for the most designedly confined to the facts observed in a single species, there would have been no impropriety in noticing some of the anatomical facts given in Van Beneden's account of *Bothriocephalus punctatus;* and also, more particularly, certain facts of a similar order given in Krabbe's description of the general structure of several species of parasites belonging to the same genus. Dr. Olssen, of Lund, and other helminthologists, have likewise recorded detached observations on the structure of the Bothriocephali and their allies, some reference to which might very well have been introduced in Drs. Sommer and Landois' admirable summary.

On account of the complex character of the organisation of the proglottides of Bothriocephalus, we have hitherto been in doubt respecting many particulars connected with the intimate structure of the adult parasite. Now, happily, these are well-nigh all removed, owing principally to the investigations of Leuckart, supplemented by the present "contributions." If, in matters of biological investigation, any proof were wanting of the necessity of extending the principle of division of toil, it would be sufficient to point to Drs. Sommer and Landois' labours as affording ample proof of the value of patient research within a given limited area.

The authors commence with a description of the exterior of the proglottis, conveniently recognising at the ventral surface a clear central space which corresponds with the region occupied by the mass of the reproductive organs, and on either side of this a marginal space whose comparatively dark colour is due to the presence of numerous large corpuscles lying immediately beneath the integument. These are the yelk chambers.

Their account of the mode of termination of the ducts of the reproductive organs at the ventral surface is in harmony with the descriptions of Eschricht and Leuckart; but it is in reference to the precise nature of the connection subsisting between the *vas deferens* and the various ducts proceeding from the female reproductive organs that these contributions lend such important aid.

The male generative apparatus consists, in the first place, of a number of testicular chambers, or minute testes, individually measuring about $\frac{1}{102}''$ in diameter. Each of these is furnished with an excretory duct; all the outgoing passages uniting to form a central cistern-like reservoir; the latter emptying itself into a single tortuous *vas deferens*, or common seminal duct. Near the final outlet it expands into the well-known globular or bottle-shaped muscular organ, as previously described by Leuckart and Böttcher. Our authors ascertained that a single joint was supplied with from ten to twelve hundred of these little testes. Truly the provision made for ensuring the propagation of these intestinal worms is astonishing; for if we reckon a full-grown Bothriocephalus to consist of three thousand proglottides (an estimate decidedly below the mark), that would give us over three millions as the number of testes supplied to a (so-called) single parasite. Shakespeare was not far wrong in the remark that "evil things do fastest propagate"—a conclusion which becomes all the more striking when we make ourselves acquainted with the exceeding complexity of the female reproductive organs of the *Tæniada* and their allies.

The sexual apparatus comprises not only the vagina and uterus (which in this class of creatures form totally distinct passages, with separate outlets), but also three special sets of organs severally concerned in the production of the germ, the yelk, and the egg-shell. Moreover, each organ is itself made up of numerous parts, being, at the same time, supplied with its own proper excretory channels. All this, of course, we knew before; but in tracing out the relations subsisting between these various channels and the organs whence they proceed, and also in establishing the mode in which their final connection with the vagina and uterus is brought about, Drs. Sommer and Landois have displayed consummate ability, and have thus materially added to our knowledge.

T. SPENCER COBBOLD

OUR BOOK SHELF

Description of a Specimen of Balænoptera musculus, in the possession of the Boston Society of Natural History. By Thomas Dwight, jun., M.D. (Boston Society of Natural History.)

THE eleventh volume of the "Memoirs of the Boston Society of Natural History" contains a descriptive account, by Dr. Thomas Dwight, of the external characters and skeleton of a young razor-back whale, the skeleton of which is preserved in the Society's Museum. This animal was captured alive in October 1870, off Gloucester, Massachusetts, and its skeleton is the best preserved specimen of a large whale in any of the American museums. The animal was 48 ft. long, the flipper was 5 ft. 4 in., and the height of the dorsal fin, measured along the anterior edge, was 1 ft. 2 in. The baleen was of a very light straw colour anteriorly, whilst further back dark stripes appeared on it, until the hindmost blades were of a uniform dark slate colour. From the very careful description which Dr. Dwight has written of the skeleton, and from the figures given in illustration, there can be no question that the animal is a young example of the fin-whale, which Dr. Gray has named *Physalus antiquorum*, but which is more appropriately named *Balænoptera musculus*. In some remarks on the classification of the specimen, he refers to the tendency to variation in the forms of the bones exhibited in the skeletons of cetacea, undoubtedly belonging to the same species, and he agrees with those cetologists who have shown the danger of accepting mere individual variations in the forms of the bones of particular specimens as affording data for establishing specific or generic differences. W. T.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Bree on Darwinism

PERMIT me to state—though the statement is almost superfluous—that Mr. Wallace, in his review of Dr. Bree's work, gives with perfect correctness what I intended to express, and what I believe was expressed clearly, with respect to the probable position of man in the early part of his pedigree. As I have not seen Dr. Bree's recent work, and as his letter is unintelligible to me, I cannot even conjecture how he has so completely mistaken my meaning: but, perhaps, no one who has read Mr. Wallace's article, or who has read a work formerly published by Dr. Bree on the same subject as his recent one, will be surprised at any amount of misunderstanding on his part.

August 3 CHARLES DARWIN

Ants and Aphides

AMONG other misstatements in Dr. Bree's "Fallacies of Darwinism," so ably criticised by Mr. Wallace in NATURE of July 25, occurs the following:—"All the stories about aphides being treated as milch-cows are myths, the result of inaccurate observation" (p. 166). I can personally refute this statement, having on many occasions watched the process. Speaking of the attraction of male emperor moths by a captive female, Dr. Bree observes:—"All this was clearly, and without doubt, done by the sense of smell" (p. 209). I, in common with most other entomologists, should much value the evidence on which this very positive assertion rests; for the explanation of the attractive power of female insects has hitherto remained a mystery. R. MELDOLA

Atmospheric Effect

THE phenomenon mentioned by Prof. Tyndall as recently occurring at the Bel Alp is not infrequent at the coast. At Folkestone in the month of June last, we saw several more or less striking instances. Some years since I witnessed, while driving, on a summer's evening, between Guildford and Godalming, an equally beautiful though different effect. The evening was stormy, and the sun, still some distance above the western horizon, threw its sheaf of rays *downwards* from behind a light cloud. In the eastern horizon was a dense, dark thunder-cloud, and upon this was seen a reflection of the opposite horizon, the shadows being absorbed by the dark background, while the intervening spaces or rays shone out with a brilliancy considerably exceeding that of those in the west. The whole of the circumstances were different from those described by Prof. Tyndall, there being, as far as I can recollect, no *upward* rays from the sun, and the rays seen on the cloud being neither convergent nor divergent, but merely parallel, and apparently a complete reflection of those which shot from the sun to the horizon. Their wonderful brightness, as contrasted with the rays of which they were the image, was, no doubt, the effect of contrast upon the almost blank screen on which they were seen. This latter, however, was lighted up to a certain extent by a sort of golden haze, in which the rays shone. The whole phenomenon was one of great beauty, and was witnessed by some friends of mine at Guildford at about the same time as I saw it from a point near to Godalming. J. RAND CAPRON

Guildford, Aug. 30

The Carbonic Acid in Sea-water

IN the Deep-sea explorations undertaken of late years in England, the gases obtained from sea-water at various depths, and under different conditions, have been the subject of investigation. As coadjutor in the German expedition to the Baltic, I have been engaged in the analysis of the sea-water gases. There have occurred circumstances which I have thought it desirable to communicate to you with reference to your forthcoming future Deep-sea explorations.

I must premise that the expulsion of the sea-water gases was undertaken in a similar manner to that of the English expedition, the pans of water being boiled for a long time in vacuum, the expelled gases being collected and afterwards analysed. The result of these analyses pointed unmistakeably to a hitherto unrecognised source of error, for the prevention of which a series of supplementary experiments was necessary. The principal results of these latter can be comprised under the following heads:—

1. The complete expulsion of the oxygen and nitrogen from sea-water presents no difficulty; it is accomplished as easily as with fresh water. The proportion of oxygen to nitrogen is not sensibly different in the first and last portions of the expelled gas.

2. The carbonic acid is only partially expelled by boiling the sea-water for hours in vacuum; the proportion of carbonic acid found in the expelled gas justifies no conclusion as to the amount in the water. It is, in the first place, dependent on the length of time during which the ebullition has been continued; the portions of the sea-water gas first driven off is almost entirely free from carbonic acid, the later portions are richer in it.

3. The complete expulsion of the carbonic acid from the sea-water is attained by its distillation in a current of air free from carbonic acid. Even under this operation, the carbonic acid is detached so slowly, that only after the evaporation of a considerable amount of water carbonate of lime begins to separate; the distillation must then be continued till, at the most, a fourth of the original quantity of water remains. The carbonic acid which is passed into baryta water can be conveniently estimated by volumetric analysis.

The fact that carbonic acid is present in large proportion in sea water, not as a dissolved gas in the same sense as oxygen or nitrogen, but in a peculiar condition of closer combination, must be of great importance, not only as respects the animal and vegetable life, but also the geological relations of the sea.

I am now proposing to myself the problem to ascertain to which constituent of sea-water is due its power of close combination with carbonic acid; and to what extent the amount of carbonic acid is proportional to its saltness. Full details will be given in the Report of the German Baltic expedition. In the expedition to be sent from here to the North Sea, application of the experience hitherto obtained will be made to the estimation of carbonic acid.

Kiel, July 5 OSCAR JACOBSEN

MR. AYRTON AND DR. HOOKER

IT has been determined that this question shall not be brought on in the House of Commons at this late period of the session, as so many members are already absent that it is possible the debate might fail somewhat to represent the actual feelings of the House. At the same time it is known that had the discussion come on some weeks ago, the Government, if they had ventured to support Mr. Ayrton, would have been beaten.

Of the return moved for by Sir John Lubbock, and prepared by Mr. Ayrton, it is difficult to speak or write without expressing strong indignation. We have not what we want, and we have what no one wants. The whole object of the compilation is to leave the reader in a fog, and from this point of view the compiler deserves credit. We regret also to see the respected name of Prof. Owen dragged into the discussion on a point which has nothing whatever to do with the question under discussion, which is simply Mr. Ayrton's monstrous behaviour to a man of science. It is gratifying to see from the Treasury letter which we reprint that "My Lords" have not hesitated to hint with sufficient pointedness for an official document their opinion on the matter. It must not be forgotten that for a time Mr. Ayrton was at the Treasury, *and that he is there no longer.*

COPY OF TREASURY MINUTE, dated 24th of July, 1872.

My Lords have under their consideration the Memorandum of the First Commissioner on the rearrangement of Kew Gardens by the Office of Works, and of the changes therein.

This Memorandum embraces three subjects :—

1. The manner in which matters connected with the management of Kew Gardens have been conducted, and in doing so, refers to instances in which complaints have been made by the Director of Kew Gardens.
2. The arrangements under which this management ought to be conducted.
3. Suggestions and questions of the First Commissioner as to changes therein, and as to connecting the Kew Gardens with the Kensington Museum, which, however, the First Commissioner does not propose should be taken into consideration at present.

To the last part of the Memorandum, therefore, my Lords do not propose to refer in the present Minute.

In discussing any arrangement for the management of the establishment and gardens at Kew, it must be remembered that there is a considerable space of ground beyond the hot-houses, buildings, and ground appropriated to the cultivation of shrubs and plants for the promotion of botanical science.

Part of this ground has been used as a nursery for young trees, and the remainder has been laid out as ornamental pleasure grounds.

The establishments at Kew have always been under the superintendence of the Office of Works, subject, of course, to the superior control of the Treasury, to which department the annual estimates of expenditure are submitted for their sanction, and this control it is indispensable to maintain.

With regard to the local management at Kew, the First Commissioner's Memorandum divides it, for the purposes of administration, into four branches, Botany, Horticulture, Police, and Works.

It is unnecessary, in the present Minute, to refer to Police.

The Department of Botany the First Commissioner states to be "under the immediate direction and control of the Director of Kew Gardens;" the Department of Horticulture to be "under the immediate cultivation of the Curator, subject to the orders and control of the Director, as the responsible head."

The works are carried out by an officer of the Office of Works.

My Lords consider this statement to represent with sufficient accuracy the proper arrangement for the establishment, and that if fairly carried into execution, in the friendly and conciliatory spirit which ought to prevail amongst the different members of all public departments, no difficulty will occur.

It is essential to maintain the superior authority in all respects of the First Commissioner, *but the nature of the case makes it evident that this authority should of course be exercised with due regard to the feelings and position of the officers under him.*

The Botanical Department has been formed by the exertions of Sir W. Hooker, and of his son Dr. Hooker. It stands high in the estimation of men of science both here and abroad, and both these eminent men are entitled to the gratitude of the country for their services in this department of science.

In all matters connected with this department of the establishment, whether as regards the hot-houses, buildings, or the cultivation of shrubs and plants for botanical purposes, the opinion of Dr. Hooker should be followed, subject only to the consideration of expense. It is for him to represent to the First Commissioner what he considers necessary for the advancement of botanical science, and it is then for the First Commissioner and the Treasury to determine whether the expense necessary for the purpose shall be incurred.

No alterations in existing arrangements in the scientific branch of the department should be made without the Director's concurrence.

The actual execution of the works to be undertaken must be under the direction of the proper officer of Works, but the opinion of the Director of the Gardens should be taken as to the efficiency of what it is proposed to do, and any requisition of his for work or repairs necessary for the preservation of the valuable plants in the houses should on all occasions receive prompt attention.

With regard to those parts of the grounds which are not used for the purpose of botanical science, but as nursery grounds or pleasure grounds, it will be the office of the First Commissioner to give such directions as he may think advisable.

My Lords, however, think it desirable that even on these points he should communicate with the Director of the Gardens, through whom, as head of the establishment, all orders to the curator and to other subordinate officers should, in regular course, be conveyed.

My Lords gather from the Memorandum of the First Commissioner that, speaking generally, the business connected with Kew Gardens has been conducted in accordance with the views thus entertained by their Lordships.

My Lords do not consider that it would be conducive either to the public advantage or to the maintenance of that good and friendly feeling which they are anxious to see prevailing in every public department, if in closing this correspondence they were to go in detail into the cases where any disagreement has taken place between the First Commissioner and the Director.

But adverting to the facts contained in the Memorandum of the First Commissioner, they are not surprised that in various cases Dr. Hooker should have thought that he had just cause of complaint, though this may have grown in some instances out of arrangements for which the First Commissioner was not responsible, and in others they learn from the Memorandum of the First Commissioner that the cause of complaint has been removed.

My Lords see no reason why under these conditions there should be any serious difficulty in discharging the respective duties of the First Commissioner and of the

Director of the Gardens in a manner satisfactory to both, whoever may be the occupants of those offices, maintaining the proper authority of the First Commissioner, with due regard to the position and character of the Director of the Gardens.

Let a copy of this Minute be sent to the First Commissioner, with a request that it may be communicated to the Director of Kew Gardens.

THE ROYAL ARCHÆOLOGICAL INSTITUTE MEETING AT SOUTHAMPTON

THE annual meeting of the Royal Archæological Institute was opened in the Hartley Institution, Southampton, on Thursday last. The members of the Institute were welcomed by the Mayor and Corporation of the borough, the chair being taken by Lord Talbot de Malahide, the permanent president.

Archdeacon Jacob furnished some interesting information respecting the tomb of William Rufus. He was, he said, accessory to the removal of the tomb of William Rufus, and he had not heard the last of it. He thought, however, he was sufficiently pachydermatous not to mind this. There was William Rufus's tomb, an eyesore and a footsore, for persons dashed their feet against it to their injury, and the place was particularly wanted, by reason of the enlargement of Winchester Cathedral, for the boys to sit. The question raised was—Is that the grave of William Rufus? Is there anything in it? If there is, whose bones are they? If there are bones, are they William Rufus's? Might it not be perfectly empty, and, if so, why should it stand there an obstruction? He, then, having skilled persons about him, ventured to take up the slab, and found that the tomb had been rifled and dishonoured in every way. There were bones there thrown about and trodden. It was suggested by some that the Parliamentarians had done this, whereupon he sent for and consulted the chief science men in Winchester. They had the bones taken out and placed on the pavement, so as to see the height of the man, and, gauged by the height which Thackeray says does the work of the country, he was found to be 5ft. 8in. Having examined them microscopically, they came to the conclusion (it was not stated how) that they were the bones of William Rufus, and were quite certain the tomb had been moved again and again before. When they visited Winchester they would find where he had had it placed, and if they had not been told otherwise they would probably have believed it had always been there. Believing in the *dictum* of Bishop Andrews, who said the church was for the living and not for the dead, and knowing that King Rufus had not been a benefactor to his country, he thought it not unbecoming to move him a little further, but he was still within the sacred walls. Therefore, anticipating their judgment, he trusted they would not pitch him into the river Itchen for the so-called disturbing of the bones of William Rufus.

At three o'clock the members of the society and friends, including the Marquis of Bristol, Lord Talbot, Colonel Pinney, and many others, visited the town under the guidance of Mr. Parker, who described most of its ancient features.

The Mayor, and Mayoress, gave a *soirée* to the members of the Institute at the Hartley Institution in the evening. It proved to be a very successful affair. Over 800 invitations were issued, and a large number of these were accepted, several officers from the United States fleet being present.

The sections met on Friday. The Bishop of Winchester presided for a short time, and introduced Lord Henry Scott, who read an introductory address in the Historical Section.

Lord H. Scott confined his remarks to a history of the County of Hampshire. As to the origin of Southampton, he said there was an ancient British town called Hampton, which was probably situate higher up the Itchen, at Bitterne. The town was even now often called Hampton by the country people. In "Domesday Book" the county was called Hampton-shire and the town Hampton. It suffered severely from the incursions of the Danes. Henry I. made it a borough, and King John gave it its first charter and had a palace there. Thence sailed the expedition for Palestine, and Henry embarked for Cressy and Agincourt. Philip of Spain also landed there to meet his Queen at Winchester. It was also from thence that the great apostle of Germany, afterwards better known as St. Boniface, departed on his mission. After referring to Leyland's "History of Southampton," he came to the general history of the county. One of the most valuable historical monuments that had been left to them was the New Forest; and however much they might condemn the severe laws which were made for the protection of what were called "the King's beastes," yet they felt some gratitude that it had been preserved to their use as it was now. He then gave a sketch of the historical associations connected with the Forest. He disbelieved the old supposition that houses and churches had been destroyed by William Rufus to make the Forest, though some small holdings might have been enclosed. The connection of Tyrrel with the death of Rufus was preserved by a ford which at this day was called Tyrrel's Ford. In the Forest was Beaulieu Abbey, which was described in the charter of King John as the *bella loca regis*. It was founded by King John in 1234. It was forty years in building, and Henry III. and all his Court attended the dedication. It was the sanctuary of Perkin Warbeck for many years. His lordship observed that this part of our coast had always in our early history been exposed to invasion. Hampshire also suffered much during the civil wars, and after the Restoration Charles II. used to come down from London to enjoy holiday in it. William III. was the first after Charles II. to attempt to repair the damages which the Civil War had created in the Forest; and later, in the era of the Georges, the county had been connected with our naval victories under Nelson. Hampshire also contained the Strathfieldsaye so closely connected with the later days of the Iron Duke; and in Hampshire the poet Keble found a quiet and honourable grave.

The Rev. F. W. Baker then read a memoir of Beaulieu Abbey.—Lord Henry Scott, in moving a vote of thanks to Mr. Baker, gave the Institute an invitation to Beaulieu for the following day, and said that Mr. Baker would be in attendance to explain every point worthy of their consideration in this most interesting abbey.—The proposition having been heartily accorded, the sitting was then suspended. In the afternoon there was an excursion of the members of the Institute to Romsey and Porchester, which occupied until eight P.M. At Romsey the vicar (the Rev. E. L. Berthon) gave a discourse on the Abbey of Romsey, and showed the result of recent excavations; and at Porchester Mr. G. T. Clark lectured upon the Castle there. Saturday was devoted to the excursion to Beaulieu Abbey, and a visit to Christchurch and Rufus's Stone.

On Tuesday, the members of the institute visited Silchester (Saxon "Sil," great or best, and "castrum"), which has the largest area of any of the Roman fortifications in England. The walls at present are about 13 ft. high and 8 ft. thick. The city had four gates—north, south, east, and west—and beyond the wall was a deep ditch, and beyond the ditch a vallum 15 ft. high. The amphitheatre is situated outside the city, 150 yards from the north-east corner of the wall. The members also visited the remains of the Chapel at Basingstoke, dismantled by the Parliamentarians, and Basing House, formerly the abode of the Marquis of Winchester, by whom it was defended against the Parliamentarians. In the evening the members held an evening session for the reading of papers.

Exeter has been fixed upon as the place of meeting next year.

THE ELECTRIC TELEGRAPH—ITS IMPROVEMENT AND CAPABILITIES

IN the beginning of the present year a Society of Telegraph Engineers was established for the general advancement of electrical and telegraphic science, intended to include not only those persons who are professionally connected with telegraphy, but those also who from their position and pursuits are enabled to render assistance in telegraphic enterprise. The institution has made a successful and promising commencement, the members at the opening meeting in February last numbering 110, the list including the historical names of Wheatstone and Cooke, the distinguished names of Thompson, Tyndall, and others scarcely less renowned for their important contributions to electrical science. The President, Mr. C. W. Siemens, D.C.L., in the course of his inaugural address, said :—

History teaches us how to read the events of the present day, and what we may reasonably look forward to even in the future; let us, therefore, review shortly in our minds the remarkable history of the Electric Telegraph, in order that we may be better prepared to deal with questions of immediate interest.

A generation has hardly passed away since the remarkable discoveries of Oersted, Ampêre, Faraday, and Weber, which laid the foundation of the electro-magnetic telegraph. The names of Steinheil, Schilling, Ronalds, Wheatstone, Cooke, and Morse furnish us with striking illustrations of the readiness with which the thinking men of different nations turn scientific discovery to practical use. While these pioneers in the field of telegraphic progress were still contending against practical difficulties, other earnest labourers entered the same field, amongst whom Werner Siemens, Bain, and Brequet should not pass unmentioned here. But so rapid has been the progress of our branch of science, that, while I am obliged to speak of these men as belonging to our early history, they are still, almost without exception, living amongst us in full enjoyment of their faculties, and, I am happy to add, members of our new society. They have the rare satisfaction to see their early day-dreams carried out upon so vast a scale that there is to-day hardly a country, however remote, that is not within a few minutes', or at all events a few hours' call, from every central point of the civilised world, that diplomatic conferences have to be held to regulate international telegraphy, and that a proposal is seriously entertained by the leading powers of the earth to place telegraphic property upon the highest, I may also say a sacred basis, by declaring it inviolable in case of war. The electric telegraph has, indeed, attained to the dignity of a commercial, a social, and an international institution of the highest importance; it is a civiliser of the first magnitude, and we may well be proud to meet here together in furtherance of such a cause.

You will pardon me if I abstain from making special reference to the numerous claims to recognition of the fellow-labourers of the present day whom I am now addressing; they are well known within our own circle and to the public at large, but neither my ability nor the time at my command would suffice for such a task. I will only endeavour, before concluding this address, to summarise the subject-matters which, judging from my experience, should engage our principal attention.

Problems of pure electrical science meet the telegraph engineer at every turn, the methods of testing insulated wire, of determining the position of a fault in a submarine cable under various circumstances, or of combining instruments so as to produce recorded messages by the mere fluctuation of electrical tension in a long submarine conductor, are problems worthy of the most profound physicist and mathematician. On the other hand, there is hardly a problem in electrical science that is not of practical interest to the Telegraph Engineer; and, considering that electricity is not represented at present by a separate learned society, ranking with the Chemical or Astronomical Societies, I am of opinion that we should not exclude from our subjects questions of purely electrical science. The phenomena of electrification and polarisation, of specific induction and conduction, the laws regulating the electrical wave, the influences of rise of temperature on conduction or the potential force residing in a coil of wire of a given form, when traversed by a current, involves questions belonging just as much to pure physical science as to the daily practice of the Telegraph Engineer, and would at any rate be inseparable from our proceedings. Next in order come questions of selection of materials for conduction or insulation, of apparatus for the best utilisation of feeble currents, of apparatus for producing, alternating, and directing electrical currents, which, although still intimately connected with physical science, call into play considerations of mechanical combinations. This brings us to questions of purely mechanical import, such as the mechanical construction of instruments for recording or printing messages, of protecting and supporting insulated conductors by sea or land, or of constructing machinery for the manufacture, the laying, and the repairing of submarine cables.

These questions again lead up to the more general ones of transport of materials through difficult and inhospitable countries, of navigation, of investigations into the depth and the nature of the bottom of the seas, into the nature and effect of sea currents, and so forth, all of which belong, under certain aspects at least, to the province of the Telegraph Engineer.

I would go further, and include even statistical information respecting the nature and growth of telegraphic correspondence, without which it is impossible to adapt the construction of lines and of working instruments to the acquirements of particular cases. The invention of a telegraphic instrument, for instance, is only of practical value if it is suited to the circumstances of the particular traffic for which it is intended, and to the electrical condition of the lines which it is proposed to work, and when the early pioneers of telegraphic progress elaborated ingenious instruments for sending and recording messages automatically or for printing them in Roman type, they invariably failed, because the then-existing lines were insufficient in every way for such refinement, and the simple needle instrument seemed to suffice for all practical purposes. It was only when the exigencies of the traffic demanded a change that instruments of this nature proved to be valuable inventions.

In like manner the long underground lines that were established on the Continent at an early date had to give way to suspended line-wire, whereas the present practice and necessities undoubtedly tend toward a reversion to the former, as being less liable to interruption by accident or by atmospheric influences, and because an unlimited number of underground wires may be established between any two stations without encumbering the public thoroughfares. The best mode of insulating and protecting these underground wires with a view to reducing the inductive influence of the one upon the other, and of facilitating access to the one, for the purpose of repairs, without disturbing the others, are questions of practical interest for the present day.

The Electric Telegraph is applicable with the greatest positive advantage for the intercommunication between two points a great distance apart; through its agency New York and Calcutta are as near to us in point of time as are the suburbs of our metropolis from one another.

It is probable, indeed, that in telegraphing from one suburb to another the message has to be oftener retransmitted than in going from the City of London to India or America, because a direct transmission from any one part of London to another would involve almost an infinite number of line-wires in all directions. For this reason there must be a limit to the applicability of the Electric Telegraphs in populous districts, and it behoves us to examine whether another agent may not be preferable in dealing with a traffic of this description. The pneumatic tube seems to be well adapted to these circumstances, and having been first applied for short distances by Latimer Clark, and subsequently modified and extended by others, it will fall within the province of our society to examine fully into this and kindred methods that may be devised for effecting rapid interchange of intelligence in towns.

THE BRITISH COAL-FIELDS

ONE distinguished geologist, at least, disbelieves in the speedy exhaustion of our coal-measures, so frequently predicted of late. At the annual meeting of the Dudley and Midland Geological and Scientific Society, Prof. Ramsay delivered an address on the existence of coal beneath the New Red and Permian strata, in the course of which he observed that for fifteen years he had been preparing to attack this subject, but it was not until he became a member of the Royal Coal Commission he had given it a really searching consideration. There could be no doubt that the various coal-fields of the Northern and Midland districts once formed one great coal-field, but had been separated by extensive denudation. Another great coal-field was formed by the now distinct fields of Devonshire, South Wales, Somersetshire, and the Forest of Dean. Between these two great divisions, the north and the south, there was no connection formed by the coal-measures, the poorer measures possibly having been deposited there, but not the rich deep ones in the carboniferous era. Referring more especially to the Midland district, he thought it highly probable that coal-measures would be found to exist between the present boundary of the South Staffordshire district and the Forest of Wyre; but it was questionable whether it would be of a workable depth. On the west side of the South Staffordshire boundary, in the direction of Bridgenorth, Shropshire, he also believed coal to exist beneath the Permian strata, at a depth of 1,500 feet, or possibly more in some places. At the north of the South Staffordshire boundary, a line drawn from Wyrley right across to the Shropshire district would, he believed, include some valuable coal-beds, a considerable part, but not all, of which would be at a workable depth. He entertained no doubt that the coal-measures were continuous between the South Staffordshire and Shropshire districts, which, although in some places disturbed by denudations, might, throughout the greater part of the area, be profitably worked. In the North Warwickshire coal-field were found, in the direction of the Staffordshire boundary, five beds of coal, which gradually amalgamated, until on nearing Coventry they formed only two measures. The shale and sandstone were split up in like manner. These features constituted most important evidence in support of the theory that the Warwickshire, Staffordshire, and Shropshire districts were united by continuous coal-measures, the peculiarities referred to in the coal, shale, and sandstone strata being identical in all three districts. In that theory Prof. Ramsay was a firm believer. From Warwickshire to the south end of the South Staffordshire boundary, there was, he believed, coal, but not profitable. Towards the northern end of the South Staffordshire boundary, however, a line drawn from Coventry would include rich and valuable coal-measures. Between Staffordshire and Leicestershire the measures were also, he believed, continuous. From Wales to the Forest of Wyre there was profitable ground; but from Wyre on to Charnwood Forest, and east of that, there were no coals of value. The speaker expressed opinions equally assuring as to the presence of coal under the area lying between the north of the South Staffordshire boundary and the mountain limestones of Derbyshire. In one part of that district—viz., north-west of Cannock Chase—Prof. Ramsay said he should not feel the slightest hesitation in recommending a search for coal; and his belief in the presence of coal at a workable depth in the neighbourhood of Uttoxeter was equally strong. Now, supposing that his calculations were only approximately correct, the result would be surprising. It would amount to this—the coal now reckoned as available in the South Staffordshire and Shropshire districts was, in round numbers, 3,201,000,000 tons. If his belief were a true one, this supply would be further augmented by 10,000,000,000 tons. In Warwickshire the proved coal-measures are estimated to yield 458,000,000 tons, and the measures he believed to exist in addition would be 2,494,000,000, or five times more than the present estimate. The Leicestershire field was calculated to possess 836,000,000 tons, and this would be supplemented to the extent of 1,790,000,000. What was the case in regard to these districts was, he believed, equally applicable to many other parts of Great Britain. The South Wales, Forest of Dean, Bristol and Somerset districts were exceptions to this rule, the coal there lying in basins caused by denudations, the surrounding measures being destroyed. In the Midland districts these small basins are not found, the whole forming one great basin. Lancashire, Derby, and the Yorkshire coal-fields were, however, subdivided by the process of denudation. Still, he had no hesitation in believing that the estuary of the Dee and the Mersey have lying between them beds of coal, although probably at too great a depth to be of practical value.

MR. TODHUNTER ON THE ARC OF THE MERIDIAN MEASURED IN LAPLAND

MR. TODHUNTER has forwarded us a reprint from the "Transactions of the Cambridge Philosophical Society," in which he discusses the observations made in connection with the measurement of the arc of the meridian in Lapland in the last century. He states that having recently had occasion to study the details of the two measurements of the arc, he has been surprised to find that the accounts of these operations, although written by very distinguished astronomers, contain numerous and serious errors. We must refer our readers to the memoir itself for a complete account of the various points raised, for it is too long for adequate notice in the space at our disposal. A curious point, however, is raised as to the effect of theory upon observation in a paragraph which we quote *in extenso* :—

"It would be a curious subject of speculation whether the theoretical opinions of persons engaged in geodetical surveys could have exercised any influence on their observations; I mean of course unconsciously, for it would be wrong to suspect any deliberate unfairness in any of the operations which I have examined. From a passage in the article 'Figure de la Terre,' by D'Alembert in the original *Encyclopédie*, it would appear that the school of Cassini originally believed that in consequence of the oblate form of the earth, the length of a degree of the meridian *would decrease* from the equator to the pole. It seems strange, perhaps, now to suppose that such an error could be seriously maintained; but there can be no doubt of it; for example, the error was vehemently maintained by Keill, a man of some reputation, who was ultimately a

Savilian professor at Oxford. See Keill's 'Examination of Dr. Burnet's Theory of the Earth,' page 140. It is certainly a remarkable coincidence that the school of Cassini starting with the erroneous theoretical notion that the degrees of the meridian *ought* to decrease from the equator to the pole arrived at the same result by observation and measurement.

"There can, I think, be no doubt that at least Maupertuis and Clairaut, who were the most eminent of the French party, held the correct Newtonian theory as to the figure of the earth; and their result was rather too decided in its confirmation of this theory. Now the geodetical angles could scarcely be influenced by the theoretical opinions of the observers; because it would not be obvious in what way the result would be affected by an error in an angle. But in measuring the base it would of course be obvious that the larger was the value obtained, the stronger was the evidence for an oblate form. Similarly in estimating the amplitude, the smaller the value obtained the stronger was the evidence for the oblate form. In these two parts of the survey then it would be necessary to be on the watch lest the conviction of what the result ought to be should influence the impression of what the observation really gives.

"It is curious that Maupertuis and his party seem to have thought at first that their success was too decided, and therefore their amplitude too small; and that on their second determination they should have made it between 3″ and 4″ larger than at first."

THE BEGINNINGS OF LIFE[*]

I.

AFTER a careful perusal of this important and suggestive work, a prominent feeling is one of regret that its value and popularity should be endangered owing to purely technical faults of composition and arrangement. It is so full of curious and novel facts and experiments, it contains so much excellent reasoning and acute criticism, and it opens up such new and astounding views of the nature and origin of life, that one feels it ought to and might have ranked with such standard works as the "Origin of Species" and the "Principles of Biology," if equal care had been bestowed upon it as a literary composition. But, unfortunately, it altogether lacks their powerful condensation and lucid arrangement. Its vast masses of facts are stated too diffusely, and are often so scattered as to lose the cumulative force that might have been given to them; while the arguments are broken up and weakened by a too minute classification of the subjects treated, leading to repetition and confusion rather than to clearness. Haste of composition is further indicated by the quantity of additional matter given in foot-notes that should have found a place in the text; and we often find it difficult to follow the special argument in hand, or to see the connection and relevance of much of the detailed evidence brought forward.

Notwithstanding these defects, which will undoubtedly diminish its popularity, it is a book which will make its mark, and must produce a powerful sensation.

It brings together a large body of facts, either new or hitherto almost ignored, which, unless they can be otherwise explained, prove much more than the mere production of low living organisms from dead matter; for these low forms have been seen to combine and give rise to higher forms, and these again to still higher and more complex organisms. Vegetable cells or their contents develop into various low animals; while animal as well as vegetable organisms of specialised forms and some elaboration of structure seem to be mutually transformable by processes quite unlike any of the hitherto accepted modes of multiplication or reproduction. These processes have been traced stage by stage, so that there seems no possibility of mistake; and they do not rest on the observations of Dr. Bastian alone. Facts of this nature have been repeatedly published for more than twenty years by many Continental and English naturalists, but, being so entirely opposed to current theories, have been all silently ignored, just as true facts and careful observations relating to the antiquity of man were so long ignored. Our author has, however, repeated and tested many of these observations, and finds them to be strictly accurate; and they harmonise perfectly with the views on the origin of life founded on his own experiments, and so energetically advocated by him.

Looked at merely as curiosities of science, and as an unveiling of mysteries hitherto thought to be inscrutable, these observations are of supreme interest; while their importance in connection with modern theories of development and the origin of species can hardly be overrated. Setting aside all the prejudices and dogmas of the existing schools of biology, it must be admitted that the views here presented of the perpetual origination of low forms of life now, as in all past epochs, is in perfect harmony with the doctrine of evolution, and does away with many of the physical and geological difficulties which are undoubtedly among the most serious which beset those special views of the origin of life which Mr. Darwin holds, but which are by no means necessary inferences from his theories.

The present work is essentially one that to be judged soundly cannot be judged hastily. The subject is of overwhelming importance to the future progress of scientific biology, and the facts and observations on which it is founded are so numerous and so precise, and have been tested by such a body of distinct and competent observers, that no *à priori* arguments and no authoritative dicta can have any weight against them. Observation alone can demonstrate whether they are facts or delusions. They will no doubt be fully criticised by those whose special studies render them competent to do so; but if the past history of science has any value whatever, the result cannot be doubtful. Facts observed and tested by a succession of careful and accurate observers, such as those whose evidence is adduced by Dr. Bastian, have never yet proved to be fallacies.

We now propose to lay before our readers a sketch of the more interesting matters treated of in these volumes, citing a few of the most striking of the new facts and the most important of the arguments founded upon them.

More than half of the first volume is devoted to an account of the Nature and Source of the Vital Forces and of Organisable Matters, and we have an excellent summary of modern views on the correlation of vital and physical forces, on the vital principle, on theories of organisation, and on the modes of origin of reproductive units and cells. As bearing upon subjects to be discussed further on, there is an important remark on the origin of germs or specks of living protoplasm in the fluids of the living body. These fluids, it is maintained, are not alive, and, therefore, the living germ does originate in a dead organic fluid. Even if it is held that blood and all the other secretions are alive, yet as they have been formed out of dead matter taken into the stomach there must be some point at which the particles of dead matter become transformed into living matter, and the circumstance of this occurring *within* an organism does not alter the fact of its occurrence, or render it at all more easy to conceive or explain. Why, then, should it be so absolutely incredible that specks of living protoplasm should arise in suitable fluids out of a living body? It is certain that as soon as the fact that they do so arise is established, the one will be as easy to conceive and be as credible as the other. The only other point that calls for notice in this part of the work is the discussion on the supposed "vital force," in which the views of the "vitalists" seem to be hardly fairly stated. Dr. Bastian says :—"If the vital or directive power resident in each particle of a living being be other than a transformed physical force it must be one which—in spite of the well-known formula '*ex nihilo nihil fit*'—is capable of indefinite self-multiplication. Either such force must be continually springing into being without cause—originating itself or growing out of nothing—which is an absurdity; or else, within the ovum of any animal, there must be locked up the whole of the peculiar vital power which is afterwards to diffuse itself throughout the body," &c. But this is by no means a necessary conception of the "vital force" or "vital principle." That force or principle need not, and cannot "reside" in any particles of matter. If it exists it is cosmical, and acts *on* matter just as gravitation does. Is it any argument against the reality of gravitation that any particle of matter, however small, attracts any mass, however great; that, as Prof. De Morgan puts it, each grain of salt and pepper in a million salt-cellars and pepper-castors, individually and separately *pull*, and actually move, the sun and every fixed star? This is a *reductio ad absurdum* against the notion that the force of gravitation resides *in* matter; but it does not touch the notion of gravitation as an inscrutable cosmical force (probably the source of *all* force) acting on matter. It appears to me, therefore, that as long as consciousness, thought, and will cannot be conceived of as manifestations of the "correlated series of physical forces," we must postulate some universal "vital principle" as co-extensive with, if not superior to and the source of, the "physical forces;" and if such exists it is natural to impute to it some share in the production of these wonderful *organisms* through which alone we see consciousness manifested. In another place Dr. Bastian says that living protoplasm is believed by a large section of the physiological world "to contain no special and peculiar 'force;' but to owe its qualities entirely to the ordinary physical properties of the elements entering into its composition." It may not *contain* a peculiar force, but surely it does *manifest* some other properties than the ordinary physical properties of its elements, just as the thundercloud, when it sends out a destructive lightning flash to the earth beneath, manifests other than the "ordinary physical properties" of the oxygen and hydrogen of which it is composed. Electricity is an extra-ordinary property of matter, and *vitality* seems to me to be still more extra-ordinary. The *force* both exhibit may be correlated with other forces; but that does not account for the special *mode* in which the force is manifested in the one case more than in the other.

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[*] "The Beginnings of Life: being some account of the Nature, Modes of Origin, and Transformations of Lower Organisms." By H Charlton Bastian, M A, M D, F.R S. (2 vols. London: Macmillan and Co. 1872.)

In the second division of his work, "Archebiosis," Dr. Bastian commences with a history of the discussion on Spontaneous Generation from the time of Aristotle to that of Pouchet and Pasteur. He then gives an outline of the evidence as to the production of low organisms in infusions. These are chiefly Bacteria and Torulæ, names which are of such frequent occurrence that we reproduce a woodcut (Fig. 1), in which they are represented, the straight objects *c* and *d* being Bacteria, while *h*, *i*, and *k* represent Torulæ; the small dots *a* are Monads, Microzymes, or plastide particles, or they may be Bacteria seen endways; while the other objects are Torula cells, or fungus germs variously combined. These are the simplest and most minute organisms; but others a little larger and more complex are shown in the next cut (Fig. 2), under the names of Vibriones, Leptothrix, Spirilla, and Mycelial filaments. These all exhibit unmistakeable signs of life, growth, and reproduction, and they appear in immense abundance in a great variety of infusions of animal and vegetable matter, however perfectly they may be shut out from the surrounding atmosphere. Most experimenters have conceived that the presence of air was necessary in order to develop organisms, and with the air it has been supposed that germs or ova have been always introduced. These germs are, however, admitted to be invisible by the highest powers of our microscopes; their very existence is therefore hypothetical, and our author shows very forcibly that Pasteur's supposed demonstration of their existence, and of their being the source of the organisms which appear in infusions, is wholly fallacious. He assumes at critical points of the argument the impossibility of his opponent's views being the true ones; and imputes his negative results to his having eliminated germs, when they can be equally well shown to be due to unfavourable conditions for development. But in order to avoid such complicated and inconclusive experiments as those carried on during the celebrated discussion between Pasteur and Pouchet, Dr. Bastian adopts a totally distinct method, which so narrows the issue as to render it possible to arrive at something like absolute certainty in the results. Instead of introducing air, purified by various chemical means, into the flasks after the infusions have been boiled, he hermetically closes their narrow necks during violent ebullition, thus producing an almost perfect vacuum above the liquid contents. After this he submits the whole flask to a heat varying from 212° to over 400° F., and then places them in favourable positions as regards light and heat. Under these rigid conditions he finds large quantities of organisms produced, which exhibit such unmistakeable signs of life as growth and multiplication. Now here the issue is reduced to its very narrowest limits, viz., what degree of heat will destroy all these low forms of life; and to determine this he adduces a series of experiments, detailed in his chapter on "The Limits of Vital Resistance to Heat." M. Pasteur found that the greatest tenacity of life was possessed by the spores of certain fungi of the family *Mucedineæ*, which germinated after being exposed to a dry heat of 248° to 257° F. for a few minutes, but half an hour's exposure to the same dry heat killed them. A Commission appointed in 1860 by the Société de Biologie found that of the lower animals, the Rotifers, "Sloths," and Anguillules found in moss, &c., were most tenacious of life, but they were all killed by a lower temperature than that above stated, so that we may fairly conclude a heat of 266° F. for thirty minutes in dry air to be the limit of vital resistance hitherto ascertained. In fluids, however, a much lower temperature suffices. Hardly any low organisms can resist 167° F., while 212° F. for even one minute is admitted by all experimenters on this subject to be fatal to all classes of organisms met with in infusions, with which alone we have now to deal. Bacteria and Vibriones, however, are killed by a much lower temperature (130°-140° F.) for ten minutes, as ascertained by a careful series of experiments; while several degrees lower was equally fatal if prolonged for four hours. It has been objected that the flasks being only partially filled, some germs or organisms may escape the liquid and survive on the sides of the glass; but as they must be exposed to almost pure steam of the same temperature as the water, and as the heat actually employed was often greater than any such organism can withstand, even in dry air, the objection cannot be held to be valid.

What, now, are we say to such experiments as the following:—Prof. Jeffries Wyman found Vibrios and Bacteriums moving with great rapidity in mutton juice which had been exposed in a hermetically-sealed flask to a heat of 120·6° C. for five minutes. Prof. Mantegazza found living Bacteria in a decoction of lettuce which had been similarly exposed for 30 minutes to 284° F. Prof. Cantoni, of Pavia, heated a solution of yolk of egg in a hermetically-sealed flask up to 242° F., and found in it after two days a large number of Vibrios. Dr. Bastian himself exposed a strong infusion of turnip in a hermetically-sealed flask to a temperature of 270°-275° F. for twenty minutes. After two months the contents were examined, and found to contain numbers of organisms, of which the annexed cut (Fig. 3) represents a specimen. Again, a solution of ammonic tartrate and sodic phosphate in distilled water was heated to a temperature of 295°-307° F. for four hours. It was at first colourless and clear, and being carefully watched was found after some days to become slightly flocculent; a small speck then appeared, which grew for several days till it could be seen with the naked eye. On being opened and examined, the speck was found to be the remarkable fungus represented in Fig. 4.

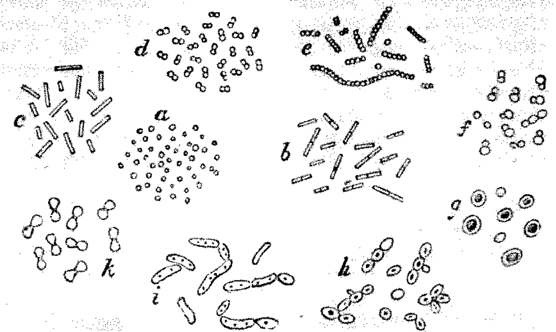

FIG. 1.—SOME OF THE MOST COMMON PRIMORDIAL FORMS OF LIFE: BACTERIA, TORULÆ, &c. (× 800)

Besides this class of experiments, others have been made with the same infusion heated to different temperatures, by which it has been ascertained that for each substance there is a different maximum, if heated beyond which no organisms appear. The juice of meat, for instance, produced Vibrios if heated to 112° C., but none if heated to 114° C. Cows' milk produces them, if heated to 113·5 C., but remains unproductive if heated to 114·5 C.; while a decoction of pumpkin produces them at 110° C., and not at 112° C. Prof. Cantoni naturally asks why, if the Vibrios are produced from germs, it requires such different amounts of heat to kill them in different solutions; and why these hypothetical germs should require such a vastly higher temperature to kill them than suffices to destroy their parents? A large number of comparative experiments made by Dr. Bastian further shows that the presence or

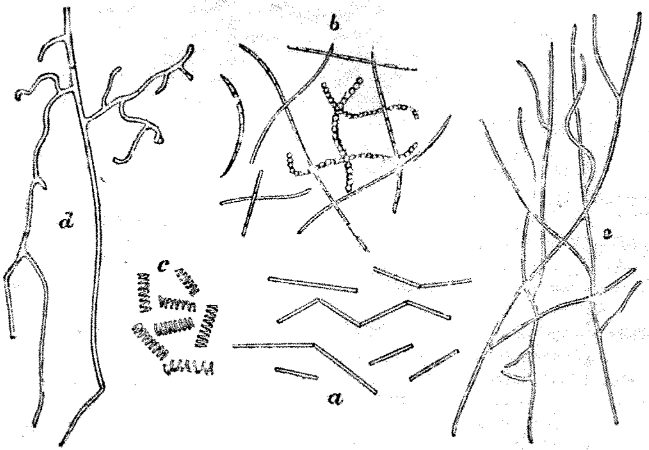

FIG. 2.—OTHER EARLY FORMS OF LIFE FROM ORGANIC INFUSIONS.
a. Vibriones. *b.* Different kinds of simple Leptothrix. *c.* Spiralla. *d.* Mycelial Filaments of an Incipient Fungus (Hallier). *e.* Branched Leptothrix or Mycelia Filaments (Pasteur).

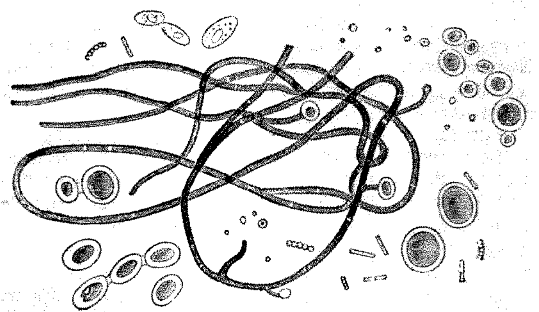

FIG. 3.—BACTERIA, TORULÆ, FUNGUS-MYCELIUM, AND SPORES OF DIFFERENT SIZES, FROM A NEUTRALISED TURNIP INFUSION (× 800).

absence of Bacteria and other low organisms in infusions often bears little or no relation to the facilities for the admission of germs from the atmosphere, but seems to depend on a variety of special conditions only to be learnt by long practice. The temperature at which the infusion was made, its quantity, the presence of dense or rarefied air in the flask, a few degrees more or less of temperature of the room where the flasks are kept, and a variety of other circumstances, so affect the results, that in some cases organisms refuse to appear when there is every facility for the hypothetical germs to gain admission; while, as we have seen, they are often plentifully produced when every possible precaution is taken to keep them out and to destroy them. The only way of escaping from the results of such a series of experiments as that here recorded is by asserting that, although the *organisms* which are produced in the flasks are killed by a temperature much below that to which the flasks have been subjected, the *germs* from which they have been produced are not so killed. We are asked, therefore, to accept as facts three pure suppositions: first, that such excessively minute and simple organisms as Bacteria, whose only mode of multiplication is by fission or gemmation, have germs which possess different physical properties from themselves; secondly, that these germs, as well as many others, are omnipresent in the atmosphere; and, thirdly, that they are not injured by an exposure for four hours to vapour heated to over 300° F.; and, finally, we are to accept all these suppositions as facts in order to avoid admitting that specks of living protoplasm are originated *de novo* in some fluids just as specks of crystalline matter originate in other fluids, and although some organisms can be seen to make their appearance in fluids independently of all pre-existing visible germs, just as crystals do.

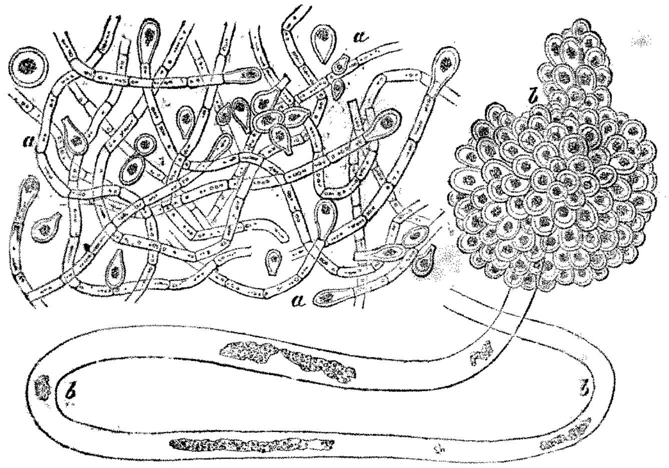

FIG. 4.—FUNGUS FOUND IN A SOLUTION OF AMMONIC TARTRATE AND SODIC PHOSPHATE (× 600).

It must, we think, be admitted that in the portion of his work we have now been considering, Dr. Bastian has fairly met and fully answered all the objections that were made to his earlier experiments. He has, moreover, shown the fallacy of many of the arguments of M. Pasteur and his supporters; and, by a series of careful and well-devised experiments, the results of which agree with those arrived at by a large number of other workers both in this country and on the Continent, has proved the *de novo* origin of various living organisms in air-tight flasks. This alone is a great step gained; but it is, as we propose to show in our next article, only the stepping-stone to more important observations and more startling facts.

DR. LIVINGSTONE

THE despatches and private correspondence of Dr. Livingstone, after a long detention, have at last been delivered, and we are now able to give extracts from the explorer's reports which throw further light on his discoveries. He appears to have ascertained, by a journey round the south-eastern side of Tanganyika, that that lake has no outlet. He has also explored the drainage to the eastward for nearly 600 miles. We learn also that his present object is to examine the hills to the south-west of Lake Bangweolo, where he had been told that there are four fountains, which he confusedly connects with the sources of the Nile, as described by Herodotus. There is marvellous heroism in this persistency, and it is sad to reflect that the grand old traveller is doomed to disappointment. But there can scarcely be any doubt that these rivers to the eastward of Tanganyika have no connection with the Nile. Apart from other considerations, Livingstone's own observations show that his Lualaba, where he saw it, was only at the same height above the sea as Gondokoro, and the error of his instrument would increase rather than diminish the height. This makes it impossible that his discoveries can be connected with the Nile. Doubtless the mass of waters is lost in some inland swamp.

The measures for Dr. Livingstone's relief were conducted with zeal and good faith, and he is now well supplied from stores sent up by Dr. Kirk and by his son. Mr. Stanley has also done excellent service in pushing on to Ujiji, in accompanying Livingstone to Unyanyembe, and in bringing home the letters and despatches. The President of the Geographical Society, in the name of the council, has promptly and cordially acknowledged this service, and the perseverance and energy with which it has been performed, in a letter addressed to Mr. Stanley as soon as the despatches were received. There was no delay or hesitation in giving him the credit that was his due; and equal promptitude has been shown by the Secretary of State for Foreign Affairs, whose letter has already been published. But the American correspondent's subsequent conduct, though doubtless agreeable to his employers, deserves no thanks from the countrymen of Livingstone. The ungenerous attack upon Dr. Kirk is sufficiently refuted by the evidence of Dr. Livingstone's own son, whose letters will, we trust, dispell the delusions with which his father's mind had been filled. Even now we can scarcely believe that Mr. Stanley is justified in his assertion that Dr. Livingstone, the great enemy of slavery, commissioned him to send up sets of slave chains to be used by Her

Majesty's Consul! The statement of Lieutenant Henn, which induced him to abandon the Relief Expedition, that Dr. Livingstone desired all such parties to be turned back, led directly to the miserable alternative of sending up supplies in charge of a native; while Mr. Stanley's secresy and concealment of all particulars respecting Livingstone's wants and intentions while at Zanzibar was unnecessary and injurious. These proceedings, the objects of which are transparently obvious, necessarily detract from the warmth of our gratitude to the man who has certainly done no small service in accompanying Livingstone to Unyanyembe, and in bringing home his letters and journal.

The supplies at Unyanyembe, and those procured from the funds of the Relief Expedition, and sent on by his son, will doubtless have enabled Livingstone to continue his exploration, and, though the disappointment to which his notions about the Nile sources must inevitably lead is to be regretted, there can be no doubt that his contemplated further discoveries will lead to valuable results, and we sincerely trust that the brave old traveller will, after one more difficult journey, live to be cordially welcomed home, and to see some good fruits from his truly heroic perseverance. We now proceed to give some extracts from his despatches.

Dr. Livingstone, in the following passage, describes his march from the ridge overhanging the western shore of Tanganyika to the great river Lualaba:—

"In going west of Bambarre, in order to embark on the Lualaba, I went down the Luamo, a river from 100 yards to 200 yards broad, which rises in the mountains opposite Ujiji, and flows across the great bend of the Lualaba. When near its confluence I found myself among people who had lately been maltreated by the slaves, and they naturally looked on me as of the same tribe as their persecutors. Africans are not generally unreasonable, though smarting under wrongs, if you can fairly make them understand your claim to innocence, and do not appear as having your 'back up.' The women here were particularly outspoken in asserting our identity with the cruel strangers. On calling to one vociferous lady, who gave me the head trader's name, to look at my colour and see if it were the same as his, she replied, with a bitter little laugh, 'Then you must be his father!' The worst the men did was to turn out in force, armed with their large spears and wooden shields, and show us out of their districts. Glad that no collision took place, we returned to Bambarre, and then, with our friend Muhamad, struck away due north, he to buy ivory, and I to reach another part of Lualaba and buy a canoe. The country is extremely beautiful, but difficult to travel over. The mountains of light grey granite stand like islands in new red sandstone, and mountain and valley are all clad in a mantle of different shades of green. The vegetation is indescribably rank. Through the grass—if grass it can be called, which is over half an inch in diameter in the stalk, and from 10 to 12 feet hight—nothing but elephants can walk. The leaves of this megatherium grass are armed with minute spikes, which, as we worm our way along elephant walks, rub disagreeebly on the side of the face where the gun is held, and the hand is made sore by fending it off the other side for hours. The rains were fairly set in by November, and in the mornings, or after a shower, these leaves were loaded with moisture, which wet us to the bone. The valleys are deeply undulating, and in each innumerable dells have to be crossed. There may be only a thread of water at the bottom, but the mud, mire, or (*Scottice*) 'glaur' is grievous; 30 or 40 yards of the path on each side of the stream are worked by the feet of passengers into an adhesive compound. By placing a foot on each side of the narrow way one may waddle a little distance along, but the rank crop of grasses, gingers, and bushes cannot spare the few inches of soil required for the side of the foot, and down he comes into the slough. The path often runs along the bed of the rivulet for 60 or more yards, as if he who first cut it out went that distance seeking for a part of the forest less dense for his axe. In other cases the Muale palm, from which here, as in Madagascar, grass cloth is woven, and called by the same name, 'lamba,' has taken possession of the valley. The leaf stalks, as thick as a strong man's arm, fall off and block up all passage, save by a path made and mixed up by the feet of elephants and buffaloes; the slough therein is groan-compelling and deep. Every now and then the traders, with rueful faces, stand panting; the sweat trickles down my face, and I suppose that I look as grim as they, though I try to cheer them with the hope that good prices will reward them at the coast for ivory obtained with so much toil. In some cases the subsoil has given way beneath the elephant's enormous weight; the deep hole is filled with mud, and one, taking it all to be about calf deep, steps in to the top of the thigh, and flaps on to a seat soft enough, but not luxurious; a merry laugh relaxes the facial muscles, though I have no other reason for it than that it is better to laugh than to cry.

"Between each district of Manyema large belts of the primeval forest still stand. Into these the sun, though vertical, cannot penetrate except by sending down at midday thin pencils of rays into the gloom. The rain-water stands for months in stagnant pools made by the feet of elephants; and the dead leaves decay on the damp soil, and make the water of the numerous rivulets of the colour of strong tea. The climbing plants, from the size of whipcord to that of a man-o'-war's hawsers, are so numerous that the ancient path is the only passage. When one of the giant trees falls across the road it forms a wall breast high to be climbed over, and the mass of tangled ropes brought down makes cutting a path around it a work of time which travellers never undertake."

In another despatch we have a more general review of the results of his explorations, as follows:—"I have ascertained that the watershed of the Nile is a broad upland between 10° and 12° south latitude, and from 4,000 feet to 5,000 ft. above the level of the sea. Mountains stand on it at various points, which, though not apparently very high, are between 6,000 feet and 7,000 feet of actual altitude. The watershed is over 700 miles in length, from west to east. The springs that rise on it are almost innumerable—that is, it would take a large part of a man's life to count them. A bird's-eye view of some parts of the watershed would resemble the frost vegetation on window-panes. They all begin in an ooze at the head of a slightly depressed valley. A few hundred yards down, the quantity of water from oozing earthen sponge forms a brisk perennial burn or brook a few feet broad and deep enough to require a bridge. These are the ultimate or primary sources of the great rivers that flow to the north in the great Nile valley. The primaries unite and form streams in general larger than the Isis at Oxford or Avon at Hamilton, and may be called secondary sources. They never dry, but unite again into four large lines of drainage, the head waters or mains of the river of Egypt. These four are each called by the natives Lualaba, which, if not too pedantic, may be spoken of as lacustrine rivers, extant specimens of those which, in pre-historic times, abounded in Africa, and which in the south are still called by Bechuanas 'Melapo,' in the north, by Arabs, 'Wadys;' both words meaning the same thing—river-bed in which no water ever now flows. Two of the four great rivers mentioned fall into the central Lualaba, or Webb's Lake River, and then we have but two main lines of drainage as depicted nearly by Ptolemy. The prevailing winds on the watershed are from the south-east. This is easily observed by the direction of the branches, and the humidity of the climate is apparent in the number of lichens, which make the upland forest look like the mangrove swamps on the coast. In passing over 60 miles of latitude, I waded

32 primary sources from calf to waist deep, and requiring from 20 minutes to an hour and a quarter to cross stream and sponge; this would give about one source to every two miles. A Suaheli friend, in passing along part of the Lake Bangweolo during six days, counted 23 from thigh to waist deep. This lake is on the watershed, for the village which I observed on its north-west shore was a few seconds into 11° south, and its southern shores and springs and rivulets are certainly in 12° south. I tried to cross it in order to measure the breadth accurately. The first stage to an inhabited island was about 24 miles. From the highest point here the tops of the trees, evidently lifted by the mirage, could be seen on the second stage and the third stage; the mainland was said to be as far as this beyond it. But my canoe men had stolen the canoe, and got a hint that the real owners were in pursuit, and got into a flurry to return home. 'They would come back for me in a few days truly,' but I had only my coverlet left to hire another craft, if they should leave me in this wide expanse of water, and being 4,000 feet above the sea, it was very cold, so I returned. The length of this lake is, at a very moderate estimate, 150 miles. It gives forth a large body of water in the Luapula; yet lakes are in no sense sources, for no large river begins in a lake, but this and others serve an important purpose in the phenomena of the Nile. It is one large lake, and unlike the Okara, which, according to Suaheli, who travelled long in our company, is three or four lakes run into one huge Victoria Nyanza, gives out a large river which, on departing out of Moero, is still larger. These men had spent many years east of Okara, and could scarcely be mistaken in saying that of the three or four lakes there only one, the Okara, gives out its waters to the north. The 'White Nile' of Speke, less by a full half than the Shire out of Nyassa (for it is only 80 or 90 yards broad), can scarcely be named in comparison with the central or Webb's Lualaba, of from 2,000 to 6,000 yards, in relation to the phenomena of the Nile. The structure and economy of the watershed answer very much the same end as the great lacustrine rivers, but I cannot at present copy a lost despatch which explained that. The mountains on the watershed are probably what Ptolemy, for reasons now unknown, called the Mountains of the Moon. From their bases I found that the springs of the Nile do unquestionably arise. This is just what Ptolemy put down, and is true geography. We must accept the fountains, and nobody but Philistines will reject the mountains, though we cannot conjecture the reason for the name. Mounts Kenia and Kilmanjaro are said to be snow-capped, but they are so far from the sources and send no water to any part of the Nile, they could never have been meant by the correct ancient explorers, from whom Ptolemy and his predecessors gleaned their true geography, so different from the trash that passes current in modern times. Before leaving the subject of the watershed, I may add that I know about 600 miles of it, but am not yet satisfied, for, unfortunately, the seventh hundred is the most interesting of the whole. I have a very strong impression that in the last hundred miles the fountains of the Nile mentioned to Herodotus by the Secretary of Minerva in the city of Sais do arise, not like all the rest, from oozing earthen sponges, but from an earthen mound, and half the water flows northward to Egypt, the other half south to Inner Ethiopia. These fountains, at no great distance off, become large rivers, though at the mound they are not more than ten miles apart. That is, one fountain rising on the north-east of the mound becomes Bartle Frere's Lualaba, and it flows into one of the lakes proper, Kamolondo, of the central line of drainage; Webb's Lualaba, the second fountain rising on the north-west, becomes (Sir Paraffin) Young's Lualaba, which, passing through Lake Lincoln and becoming Loeki or Lomame, and joining the central line too, goes north to Egypt. The third fountain on the south-west, Palmerston's, becomes the Liambia or Upper Zambesi; while the fourth, Oswell's, fountain becomes the Kafue, and falls into Zambesi in Inner Ethiopia. More time has been spent in the exploration than I ever anticipated."

He then sums up the results of his work as follows:—"The Geographical results of four arduous trips in different directions in the Manyema country are briefly as follows:— The great river, Webb's Lualaba, in the centre of the Nile valley, makes a great bend to the west, soon after leaving Lake Moero, of at least 180 miles; then turning to the north for some distance, it makes another large sweep west of about 120 miles, in the course of which about 30 miles of southing are made; it then draws round to north-east, receives the Lomame, or Loeki, a large river which flows through Lake Lincoln. After the union a large lake is formed, with many inhabited islands in it, but this has still to be explored. It is the fourth large lake in the central line of drainage, and cannot be Lake Albert; for, assuming Speke's longitude of Ujiji to be pretty correct, and my reckoning not enormously wrong, the great central lacustrine river is about 5° west of Upper and Lower Tanganyika. In my attempts to penetrate further and further I had but little hope of ultimate success, for the great amount of westing led to a continual effort to suspend the judgment, lest, after all, I might be exploring the Congo instead of the Nile, and it was only after the two great western drains fell into the central main, and left but the two great lacustrine rivers of Ptolemy, that I felt pretty sure of being on the right track. The great bends west probably form one side of the rivers above that geographical loop, the other side Upper Tanganyika and the Lake River Albert. A waterfall is reported to exist between Tanganyika and Albert Nyanza, but I could not go to it; nor have I seen the connecting link between the two—the upper side of the loop—though I believe it exists."

His despatches conclude with the following account of his future intentions:—"Geographers will be interested to know the plan I propose to follow. I shall at present avoid Ujiji, and go about south-west from this to Fipa, which is east of and near the south end of Tanganyika; then round the same south end, only touching it again at Pambette, thence resuming the south-west course to cross the Chambeze and proceed along the southern shores of Lake Bangweolo, which being in latitude 12° south, the course will be due west to the ancient fountains of Herodotus. From them it is about ten days north to Katanga, the copper mines of which have been worked for ages. The malachite ore is described as so abundant, it can only be mentioned by the coalheavers' phrase, 'practically inexhaustible.' About ten days north-east of Katanga very extensive underground rock excavations deserve attention as very ancient, the natives ascribing their formation to the Deity alone. They are remarkable for all having water laid on in running streams, and the inhabitants of large districts can all take refuge in them in case of invasion. Returning from them to Katanga, 12 days N.N.W. take to the southern end of Lake Lincoln. I wish to go down through it to the Lomame, and into Webb's Lualaba and home. I was mistaken in the information that a waterfall existed between Tanganyika and Albert Nyanza. Tanganyika is of no interest, except in a very remote degree in connection with the sources of the Nile. But what if I am mistaken, too, about the ancient fountain? Then we shall see! I know the rivers they are said to form—two north and two south—and in battling down the central line of drainage, the enormous amount of westing it made caused me at times to feel as if running my head against a stone wall. It might after all be the Congo, and who would care to run the risk of being put into a cannibal pot and be converted into black man for anything less than the grand old Nile? But when I found that Lualaba forsook its westing, and received through Lake Kamolondo Bartle Frere's great river, and that afterwards farther down it takes in Young's great stream through Lake Lincoln, I ventured to think that I was on the right track. Two great rivers arise somewhere on the western end of the watershed, and flow north—to Egypt (?). Two other large rivers rise in the same quarter and flow south, as the Zambesi, or Liambai, and the Kafue, into Inner Ethiopia. Yet I speak with diffidence, for I have no affinity with an untravelled would-be geographer who used to swear to the fancies he collected from slaves till he became blue in the face. I know about six hundred miles of the watershed pretty fairly. I turn to the seventh hundred miles with pleasure and hope. I want no companion now, though discovery means hard work. Some can make what they call theoretical discoveries by dreaming. I should like to offer a prize for an explanation of the correlation of the structure and economy of the watershed with the structure and economy of the great lacustrine rivers in the production of the phenomena of the Nile. The prize cannot be undervalued by competitors even who have only dreamed of what has given me very great trouble, though they may have hit on the division of labour in dreaming, and each discovered one or two hundred miles. In the actual discovery so far, I went two years and six months without once tasting tea, coffee, or sugar; and, except at Ujiji, have fed on buffaloes, rhinoceros, elephants, hippopotami, and cattle of that sort; and have come to believe that English roast-beef and plum pudding must be the real genuine theobroma, the food of the gods, and I offer to all successful competitors a glorious feast of beefsteaks and stout. No competition will be allowed after I have published my own explanation, on pain of immediate execution without benefit of clergy! I send home my journal by Mr. Stanley, sealed, to my daughter Agnes. It is one of Letts' large folio diaries, and is full except a few (five) pages reserved for altitudes which I cannot at present copy. It contains a few private memoranda for my family alone, and I adopt this course in order to secure it from risk in my concluding trip."

NOTES

We are in a storm of Congresses. Scarcely has the British Archæological Institute finished its work at Southampton before the British Archæological Association, the Iron and Steel Institute, and the British Medical Association have thrown Wolverhampton, Glasgow, and Birmingham respectively into unwonted excitement; while by this time next week the British Association will be in full swing at Brighton. With regard to this latter Congress, we are happy to announce that admirable arrangements for the reception of visitors are being made, and that a large and satisfactory meeting, under the presidency of Dr. Carpenter, may be anticipated.

At the moment of going to press we hear that a M. Delaunay, "director of the Astronomical Observatory" (*sic*), has been drowned by the upsetting of a boat at Cherbourg. We sincerely trust this does not refer to the distinguished director of the Observatory of Paris. But the telegram is ominous.

The French Association for the Advancement of Science will hold its first annual meeting at Bordeaux, from the 5th to the 12th of September. The Administrative Council, consisting of MM. Claude Bernard, Broca, Delaunay, A. d'Eichthal, De Quatrefages, Wurtz, Cornu, secretary of the Association, Masson, treasurer, and Gariel, secretary of the Council, have issued a circular inviting many of our most eminent men of science to be present; and, as the new French society is based upon an old English one, it will be both graceful and useful that as many shall attend as possible. We have also received the first publication of the society, containing a *compte rendu* of the inauguration meeting of the society, held in April in Paris, and the statutes of the society, which have already appeared in Nature nearly in their final form. We wish the society every success; its object is essentially decentralisation. Would we in England were in a position to decentralise!

Dr. Cobbold, F.R.S., has been appointed to a Professorship of Helminthology at the Royal Veterinary College. During the ensuing winter months he will give a course of lectures on the Parasites and Parasitic Diseases of our domesticated animals.

Dr. Brown-Séquard has resigned the Chair of Comparative and Experimental Pathology in connection with the Faculty of Medicine in Paris, which he has occupied several years. M. Vulpian has applied for the Chair.

Dr. Gerhart, of Jena, has been appointed successor to Dr. Bamberger as Professor of Clinical Medicine at Wurzburg.

We regret to learn that Mr. M. C. Webster, the acting collector at Trichinopoli, to whose exertions the Eclipse Expedition owed so much, has been ordered home without delay for two years on sick certificate.

One of the most recent applications of science to practical purposes, which, according to most people is the only part of science which is worth anything, is sufficiently curious. In the "Mors Electrique" of M. Sidot, we have electricity employed in a manner to combine the study of electricity with a ride or drive into the country in company with a restive horse. Nothing is more simple. In the carriage, or even in the saddle, we have a pile *système hermétique Trouvé*, and a small induction coil, along the reins the magic wires are laid, and on either side the animal's lower jaw we have a *couronne metallique*. These are the data. The inventor is under the impression that when the quadruped's motion becomes too rapid it will be instantly brought to zero by the passage of the spark through the aforesaid jaw, but we do not learn that he has tried it. We would suggest that in a cavalry charge it would be most effective. This should be tried, at the forthcoming manœuvres. In war the principle might be extended. The horses might be armed unicorn-wise, with blunderbusses and Abel's fuzes, the *couronne metallique* being of course removed from the lower jaw to the novel weapon. Probably in this way the functions of the riders might be abolished altogether; this would bring about a great saving in the army estimates, and in this way cause the Government to think that there may be something in science after all.

No. 6 of the Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College, just come to hand, contains the supplement to the Ophiuridæ and Astrophytidæ by Theodore Lyman, with two plates, in which figures are given of the most important of the singular deep-sea Ophiurans brought up by the dredgings off the coast of Florida, and described in Bulletin No. 10, vol. i. of the Catalogue.

Our common sparrow, as most of our readers are aware, has been for some time naturalised in the New York parks and elsewhere in the United States, for the purpose of keeping in check a plague of caterpillars, in which office it is doing yeoman service We regret to learn by a paragraph in *The New York Industrial Monthly* that our compatriots are in danger of extermination by a race of feathered rowdies, also bent upon turning the balance of creation their own way. The sparrow's enemy is the great northern shrike; and the *Industrial Monthly* states that one of these "butcher-birds," which eat only the brains of their victims, recently killed a sparrow "by squeezing its head into a crotch made by the fork of two branches, each about half an inch thick."

The Paris Academy of Sciences has submitted the names of MM. Loewy and Wolf to the Minister of Public Instruction to fill the vacancy in the Bureau des Longitudes, occasioned by the death of M. Langier.

M. Folie has recently called the attention of the Royal Academy of Sciences of Belgium to the question of the density of the earth. He criticises some points in Sir George Airy's discussion of his observations, which gave the value 6·566, and states his opinion that 6·439 is nearer the mark.

In the *Canadian Journal* for July, Prof. H. Alleyne Nicholson discusses the Contemporaneity of Strata and the Doctrine of Geological Continuity with the view of demonstrating "that groups of strata presenting the same fossils, if widely removed from one another in point of distance, can only exceptionally be 'contemporaneous' in the strict sense of the term. On the contrary," he contends, "in so far as we can judge from the known facts of the present distribution of living beings, the recurrence of exactly the same fossils in beds far removed from one another, is *prima facie* evidence that the strata are *not* exactly contemporaneous; but that they succeed one another in point of time, though by no long interval, geologically speaking." Dr. Nicholson concludes "that it is not correct to say that we are living in the Cretaceous period in any other sense than one might say we are living in the Silurian period; with this difference only, that the Cretaceous period is much nearer to us in point of time than the Silurian, and that we can thus trace a relationship between certain living types and certain Cretaceous forms, such as we cannot hope to establish in the case of Silurian forms." Farther, "the conditions present in the deep Atlantic cannot be exactly similar to those of the Cretaceous seas; for the *Cephalopoda* of the chalk seem to have no representatives in the abyssal mud of the Atlantic, while this class was well represented in the Carboniferous times; so that there is, if anything, a closer genetic connection between the chalk and the Carboniferous limestone than between the chalk and the Atlantic 'ooze.'"

The principal articles in the Transactions of the Royal Microscopical Society for August are the conclusion of Dr. L. S. Beale's paper on the Nerves of Capillary Vessels and their probable action in Health and Disease, a discussion of the vexed question of the Nomenclature of Objectives, by Dr. J. J. Woodward, of the U. S. Army, and a useful paper, by Dr. R. H. Ward, on Our Present Medical and Students' Microscopes.

The July number of the Proceedings of the Geologists' Association contains a paper, by Mr. Henry Woodward, on Relics of the Carboniferous and other old Land Surfaces, and an essay, by Mr. M. Hawkins Johnson, towards a solution of the chalk-flint enigma.

There are only two papers in the recent issue of the Transactions of the Institution of Engineers and Shipbuilders in Scotland, one on the Manufacture of Cast Steel, by Mr. B. D. Healey, the other, by Mr. Alexander Morton, on the Expansion of Water. The former is copiously illustrated.

Other things besides history have a tendency to repeat themselves. Vice-Consul Green, in his report concerning the Tunisian fisheries, states that a large portion of the fish supply of the capital of that African state comes from the Bisuta Lake, a distance exceeding forty miles, on pack animals, and, consequently, frequently arrives in an unmarketable state. It is calculated that if proper and expeditious conveyance could be obtained, the supply of the fishery would be sufficient to augment its yearly value by 15,000*l.* or 20,000*l.* But owing to the present fish farmer being able, without any considerable outlay in guards and assistance from preservative enactments, to secure a handsome return from the fisheries conceded to him, great waste and destruction of fish exist.

The Proceedings of the Asiatic Society of Bengal for May consist mainly of the third instalment of Dr. F. Stoliczka's Notes on the Reptilian and Amphibian Fauna of Kachh or Cutch.

According to Dr. Henry Seueur, the twenty-eight weeks' siege of Paris cost upwards of 50,000 lives to the civil population. He ascertains that 300,000 Parisians left Paris by rail before the investment; but 190,000 regular troops and 170,000 refugees from the suburbs entered the city; so that the population of Paris, on the whole, was raised from 1,890,000 to about 2,000,000, the excess consisting chiefly of men between twenty and forty years of age. The mean mortality of the four preceding years and of the following year for these twenty-eight weeks was 24,928; that for the twenty-eight weeks of 1870-71 was 77,231—an excess of 52,303. The mortality fell unevenly on persons of various ages. Between fifteen and twenty-five, it was multiplied sixfold. The general mortality was tripled. The mortality was least among men from forty to sixty; they took no part in active service, and had comparatively greater facilities for resisting cold and privation. The diseases which contributed chiefly to the immense mortality were six—small-pox, bronchitis, pneumonia, typhoid fever, diarrhœa, and dysentery.

The seventh annual meeting of the Quekett Microscopical Club took place on the 26th ult., when Dr. L. S. Beale, F.R.S., the retiring president, delivered his valedictory address. The club now numbers about 550 members. Dr. R. Braithwaite, F.L.S., is the president for 1872-3. The four vice-presidents are Dr. L. S. Beale, F.R.S., Mr. Arthur E. Durham, Mr. Henry Lee, and Dr. Matthews. The following gentlemen have been re-elected to serve during the ensuing year:—As treasurer, Mr. Robert Hardwicke, F.L.S.; as secretary, Mr. T. Charters White; and as secretary for foreign correspondence, Mr. M. C Cooke, M.A.

The *British Medical Journal* thinks "it cannot be doubted that one moral will be finally drawn from the difficulties of the southern districts of London as to their water-supply. An adequate supply of drinking water to a crowded part of a great city is of as great importance as a supply of fresh air; and it may reasonably be doubted whether any pains or penalties will suffice to protect a great population from the shortcomings of companies who undertake to furnish a water-supply on purely commercial grounds, and who neither have nor can be expected to have any benevolent interest in the health and comfort of their customers. The water companies of the metropolis hold in their hands the lives of a very large mass of people; and very slight *laches,* as in the case of the East London epidemic of cholera, suffice to slay hundreds of helpless and perfectly innocent water-drinkers." The *Journal* holds out to "the consideration of all classes of statesmen and of electors, whether the water-supply of great cities, such as London, should be left to the tender mercies of companies, whose opinion as to the importance of 'living organisms' and 'previous sewage contamination' are notoriously sceptical, and are much influenced by the annual arguments presented by the auditors." The Lambeth Water Company, it remarks, "is alike a terror to drunkards and teetotallers; it adds a fresh terror to adulteration, and another curse to the list of those that afflict great cities."

We have received Part iv. of the fourth edition of Yarrell's "British Birds," edited by Prof. Newton, and in course of publication by Van Voorst. The birds described in this part are chiefly of the families *Turdidæ* and *Silvidæ.*

At the recent half-yearly meeting of the Grand Trunk Railway Company of Canada, in respect to the rails being exposed to severe cold for a great length of time, the President said that from 3,500 to 4,000 rails on the line break every winter! But he found comfort in the fact that, in about 110 miles of steel track, only eight or ten rails have broken. This would seem to indicate that Bessemer rails are suitable for cold climates.

CAPTAIN MAJENDIE, in his report of the Stowmarket explosion, says that he resisted as long as possible the suggestion that the catastrophe was due to foul play on the part of some one who wilfully added acid to properly purified and manufactured gun-cotton. Step by step, however, his examination established the theory of foul play as the correct one; and in face of the evidence no other verdict than that given by the jury was possible. He thinks the balance of probability leads to the belief that whoever added the acid was unaware of the terrible consequences it would produce.

THE pupils of the Trade School at Keighley, in Yorkshire, established by the Schools Inquiry Commissioners for the higher education of the children of the artisan classes of that town, have distinguished themselves in the recent examinations. The results are as follows:—Acoustics, Light and Heat: Nine first-class, thirteen second-class.—Theoretical Mechanics: Three first-class, thirteen second-class.—Physical Geography: Twelve first-class, twenty-two second-class.—Steam and the Steam Engine: Three first-class, twenty-five second-class.—Applied Mechanics: Four first-class, twenty-five second-class.—Building Construction: Two first-class, eight second-class.—Animal Physiology:—Two first-class, thirty-one second-class.—Machine Drawing: Nine first-class, twenty-three second-class.—Inorganic Chemistry: Eight first-class, fifteen second-class. (Six second-class by students from Cullingworth.)—Laboratory Practice: Six first-class, four second-class.—Mathematics: Three first-class, twelve second-class. In all, sixty-one first-class passes, and one hundred and ninety-one second-class passes. The Keighley School of Art is attended by about one hundred young men of the town. The examiners at South Kensington have passed the drawings of eighteen of them in the elementary section, and of five in the advanced section, while the works of three—Annie Preston, Thomas Ramsden, and J. Midgley—have been laid out for national competition.

THE fruit crop of 1872 (says the *Gardeners' Magazine*) is probably the smallest that the most experienced and observant cultivator can call to remembrance. It is certainly but little better than no crop at all; and in many fruit-growing districts will not pay for gathering, and, therefore, perhaps, will be lost entirely. The imports of fruit from the Continent have been very much below the average hitherto this year; and the fact suggests itself that our neighbours across the Channel are in much the same plight as ourselves as respects this season's product of fruit. Usually in seasons notable for short supplies of fruit, some kinds are sufficiently plentiful to compensate in part for the general deficiency, but the present is an exceptional season in that respect, for the failure is complete. There can be no mystery about the cause of this general barrenness. The trees made a good growth last year, and the wood was sufficiently ripened. Hence there was a good show of bloom when vegetation was roused into activity by the genial weather which occurred in the month of February. The crop was ruined by the second winter that distinguished the month of March and greater part of the month of April.

IT is estimated that the whole available stock of the famous "Torbane Hill mineral" does not now exceed 50,000 tons, for the extraction of which a pit is about to be sunk. A trustworthy authority states the quantity already worked at about 1,800,000 tons.

IN a communication from Natal, Mr. G. R. Blanche states that Mr. B. Bouwer had seen, in a stone cave in Namaqua-land, about twelve days from Lake Ngami, pictures of all sorts of animals, drawn by Bushmen, in which the unicorn was distinctly delineated. Mr. Bouwer added that an old Bushman at Ghanze told him that he had many years ago seen the animal, that it was very fierce, but that it had now gone away. He had heard, besides, other Bushmen speak in similar terms, of the reputed fabulous beast. Mr. Blanche concludes:—"My opinion is, that the unicorn existed recently in Africa, and that it is *not proved* to be extinct now, but that the probability of its being in existence now is not very great." He rests this conclusion on the general accuracy of such rude sketches by savages in other parts of the world besides Africa, asking, if the unicorn never did exist, why should drawings of it be made in Namaqua-land, Natal, the Transvaal Republic and Cape Colony, possessing the same general and one particular characteristic.

FRANK CLOWES, Esq., B.Sc., London, F.C.S., has been appointed Science Master at Queenwood College, Hants.

THE *American Naturalist* for July contains a pretty exhaustive account of the Wyandotte Cave and its fauna, by Prof. E. D. Cope. The animals catalogued are fifteen in number, but as this collection was the result of only two days' exploration, Prof. Cope considers that the Wyandotte is richer in life than the better known Mammoth Cave, which has yielded only seventeen species after frequent examination. He describes a curious parasite—a Lernæan—on the blind fish of the cave. The representatives in the Wyandotte of two of the blind genera in the Mammoth Cave are furnished with eyes.

IN his paper on the Wyandotte Cave, in the July number of the *American Naturalist*, Prof. Cope incidentally remarks:—"I believe that wild animals betake themselves to caves to die, and that this habit accounts in large part for the great collections of skeletons found in the cave deposits of the world. After much experience in woodcraft, I may say that I never found the bones of a wild animal which had not died by the hand of man lying exposed in the forest."

THE small white butterfly (*Pieris rapæ*) which has quite recently become naturalised in North America, is likely to spread over the more temperate parts of that continent, to the serious detriment of farmers and gardeners. In a paper on the subject in the *Canadian Entomologist*, Mr. G. J. Bowles, of Montreal, states that the insect has already spread over the province of Quebec and the New England States, and is estimated to have destroyed 500,000 dols. worth of cabbages last year in the vicinity of New York alone. Mr. Bowles communicates some interesting facts in the life history of the immigrant butterfly. "The species," he says, "in its new habitat, has to pass through extremes of temperature to which it has not been accustomed in England, from which country it was most probably introduced; and while the increased summer heat of Canada appears to have made it more prolific, by augmenting the number of broods, the greater cold of winter has balanced the account by killing off, while in the chrysalis state, the surplus which otherwise would have rendered the insect an intolerable pest. The 'compensating' principle in the laws of nature," he adds, "is thus in useful operation with regard to *P. rapæ;* and as the power of cold decreases in effectiveness through the butterfly becoming acclimatised (which will probably happen in course of time), no doubt other agencies will arise, in the shape of new parasitic enemies, to keep the species within due bounds."

MR. H. HAUPT, C.E., in an article in *Van Nostrand's Engineering Magazine* (N.Y.), proposes a system of narrow gauge wooden railways in rural districts and sparsely-settled localities in America, which he describes at some length, and asserts can be constructed more cheaply than ordinary country roads, and upon which transportation can be conducted at less expense than on ordinary railroads. The system, he adds, has been tested to a limited extent, and found to answer admirably.

A SUPPLEMENT to the fifth annual report of the United States' Geological Survey has just been published. It consists of an enumeration, with descriptions by M. Lesquereux, of tertiary fossil plants collected by Dr. F. V. Hayden in 1870, from which some important climatic and other conclusions are drawn.

IT is stated that a plan has been submitted to the Spanish Government for a tunnel under the Straits of Gibraltar, which might be connected with the shortest route to India. The length to be traversed would be 13,800 metres, while that of the contemplated Dover and Calais tunnel is stated as 32,000.

THE late thunderstorms have done considerable damage to the Postal Telegraphs of the United Kingdom. Demagnetisation of needles, and in a large number of instances the fusing and complete destructions of the instrument coils, show that the want of an efficient lightning protector is still much felt.

CORK trees are being extensively introduced into Southern California.

FORMS OF SOLAR PROTUBERANCES *

PROFESSOR TACCHINI gave a full account of some of the work recently done by the Italian Society of Spectroscopists, which will be read with interest. At the beginning of his discourse he dealt specially with the observations on the solar protuberances, made with the view of throwing light on the question, whether the strata below the sun's chromosphere are solid, liquid, or gaseous. If we suppose that the protuberances have the form of jets, that is to say, narrow at the base and spreading out like a fan, as in the jets of gas which issue from terrestrial volcanoes, and if, moreover, instead of being composed of one element or a small number of elements, they are composed, from base to summit, of numerous materials, then it will appear probable that they are produced by eruptions taking place through a strongly resisting medium; and consequently that there must be already formed, on the surface of the sun, a crust solid enough to resist, for the most part, the powerful tension of the internal incandescent gases, which, breaking through this crust at certain points, give rise to violent eruptions, constituting the phenomenon of the solar protuberances. On the other hand, if all or most of the protuberances have a wide base and taper upwards like a pyramid, if their composition is simple, perhaps of the same materials as the chromosphere—a complex composition occurring only in a few of them, and at the base or at a small height above it—then the protuberances, properly so called, must be regarded, not as true eruptions, but as alterations of the chromosphere in those parts, where, through special circumstances, the composition of the subjacent strata becomes modified, either by an outflow of the internal constituents of the solar sphere—in which case the phenomenon is brought about by internal causes—or by disturbances arising in particular zones in consequence of movements developed in the sun's atmosphere, in which case the protuberances are produced by external causes; in other cases both these causes may concur in the production of the phenomena in question.

These considerations are sufficient to show the great importance of establishing the general character of the solar protuberances, and for this purpose, and to avoid certain sources of error, Prof. Tacchini invited P. Secchi, at Rome, and Prof. Lorenzoni, at Padua, to join with him in making contemporaneous observations of the solar protuberances. The proposal was favourably received, and it was agreed that from the 1st to the 13th of July, 1871, observations of the entire limb of the sun should be made from 7 to 10 o'clock.

The spectroscope employed by Tacchini at Palermo is formed of three direct vision prisms, constructed by Tauber, of Leipzig; that of P. Secchi is an instrument with angular vision, somewhat inferior in power to the Palermo instrument; and that of Prof. Lorenzoni is a direct-vision spectroscope which was used in Sicily in the observation of the total eclipse of 1870.

A comparison of the observations made at Rome and at Palermo led to the following results:—

1. All the masses are found indiscriminately in the drawings made at Rome and at Palermo; the most remarkable peculiarities of the chromosphere are likewise reproduced in both.

2. The principal characters of the forms of the protuberances are identical in the two sets of drawings; the direction and position of the plumes, the luminous masses, and frequently the regions of the chromosphere where the flames have a peculiar appearance, are perfectly identical.

3. The heights of the protuberances are for the most part the same, notwithstanding the diversity of the methods employed for measuring them.

4. The differences in the two sets of delineations are of two kinds: the first arising from the mode of drawing, the second from the greater distinctness of vision at Palermo. Other differences are real, being due to the rapid changes taking place in the protuberances.

At Padua the observations were limited to the delineation of individual protuberances; these were found to be the same in form and altitude as those seen at Rome and Palermo, thus affording proof that at the three stations, with different means of observation, the objects seen were identical, and removing any doubt that might previously have existed as to the power of the spectroscope to afford accurate results respecting the chromosphere and the forms of the protuberances.

Tacchini next proceeded to consider the general form of the protuberances. By observations with the spectroscope, continued from March 1871 to February 1872, he found that out of 2,903 protuberances, only 234, or abour 8 per cent., have the form of a tree or of a fan, that is to say, are narrow at the base and spread out towards the upper part, as if they were produced by volcanic eruptions, whereas the remaining 92 per cent. have a broad base and taper upwards like a pyramid, seem, therefore, to be due rather to a simple throwing up of the substance of the chromosphere. He, therefore, regards the general form of the protuberances as inconsistent with the existence of a solid crust on the surface of the sun. This is entirely in accordance with the English work.

When the chromosphere is observed with large instruments—and under peculiarly favourable conditions—it does not present the appearance of a continuous level stratum, as should be the case if it were solid, pasty, or liquid, but often appears to be formed of a continuous series of very distinct flames.* It looks indeed like a general conflagration, more or less developed, which is incessantly renewed with greater or less force, and with especial violence in particular parts, where it gives rise to the protuberances. In small instruments, on the contrary, the chromosphere appears smooth, excepting certain parts where the flames rise to an unusual height. In like manner the details of the protuberances, and especially their outlines, as observed with small instruments, are not comparable with those made with large telescopes, which must necessarily afford a higher degree of definition.

Tacchini next described certain observations which tend to show that the so-called jets, projected upwards from the chromosphere, have their counterpart in a descent of matter from above, in a kind of solar rain, when a mass or cloud of luminous hydrogen suspended in the sun's atmosphere throws off filaments on both sides, which gradually descend and unite at the sun's edge, thereby forming a protuberance which exhibits the arborescent or fan-like form usually attributed to an eruptive jet. This is a new point of great interest.

The theory which attributes the solar protuberances to violent eruptions forcing their way through a solid crust, or a liquid of great resisting power, may, Tacchini remarks, appear to derive some support from the chemical composition of the protuberances. If the masses which project above the chromosphere were found to contain many materials different from those which compose the chromosphere itself, there would be good reason for regarding them as projected from the interior of the sun. And, in fact, some of the protuberances have a somewhat complex chemical composition, the bright lines observed in their spectra often corresponding to magnesium, iron, sodium, titanium, calcium, barium, nickel, chromium, copper, together with eight other lines which may belong to as many different substances; in all therefore eighteen elements, besides hydrogen and the element provisionally named Helium, which is never absent, and represents the constant material of the entire chromosphere. On August 27 last in a single protuberance nine different substances, represented by a brilliant spectrum of twenty-four bright lines, nine of which belonged to iron were seen.

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* "On the Forms of the Solar Protuberances and the Regions of Magnesium and Iron on the Surface of the Sun." By P. Tacchini (Public Conference held on Feb. 18, 1872, in the Royal University of Palermo).

* See Lockyer, Proc. R.S. vol. xvii. 1870, p. 354.

But is this rich composition common to all the protuberances, or limited to a certain number? and do these materials extend throughout the protuberance, or are they confined to a limited portion of it?

Tacchini's observations show that the varied composition in question is limited to a very small height; in other words, to the mere base of the protuberance, whereas the higher portions are composed exclusively of hydrogen and the element D_3. Now, if the protuberances were the result of violent eruptions, the substances ejected with such force could not fail to attain a considerable height above the base of the protuberance, which is contrary to observation. Moreover, it is not all the protuberances that give a mixed spectrum; that is to say, they are not all formed of numerous materials, as they should be, at least in the majority of cases if they were produced by violent eruptions. According to Tacchini's observations, only 10 out of 100 protuberances give a mixed spectrum, the remaining 90 giving spectra which exhibit only the hydrogen lines and the line D_3. He also finds that in all the spectra which he has observed, either of protuberances near the sun's edge, or of clouds and filaments of greater height, the hydrogen-lines never occurred alone, but always accompanied by the line D_3. In one case only were the hydrogen lines and D_3 accompanied by other bright lines in the distinctly higher portions of the protuberances. This was observed on December 19, 1871, on a magnificent protuberance resembling a great conflagration, the central part of which exhibited two red lines.

If the protuberances were formed of materials violently ejected from the interior of the sun, it would follow that at every point of the sun's edge where a mixed spectrum occurs, that is to say, where there is evidence of the presence of numerous materials, there also the corresponding protuberance should exist; but observation shows the contrary.

From his first examination of the protuberances, indeed, Tacchini had been led to conclude that the bright protuberances alone afforded certain indication of a rich variety of materials;[*] but observing afterwards some parts of the sun's edge which, though free from protuberances, nevertheless had a peculiar structure, inasmuch as they were formed of a series of flames higher and brighter than usual, he was led to examine the spectrum of these portions more attentively; and he found indeed that there also, in addition to the lines of hydrogen and D_3, lines appeared corresponding to many other substances. A very careful examination of the sun's edge was accordingly made in August and September 1871, the result of which was to show that in very extensive tracts of the edge, amounting to ⅙ of the whole, the entire chromosphere was invaded by the vapours of various metals, although these same parts of the edge were not covered by protuberances, an observation before made by Lockyer.[†]

These tracts continued to show themselves more or less extensively for many days afterwards in the same part of the sun's limb, representing in their aggregate vast regions of the surface where the hydrogen of the chromosphere was mixed with many other substances which commonly exist at the base of the chromosphere. Thus on the 28th of August and 17th of September, 1871, throughout an area of 60°, from the position 30° to 90°, the edge gave a mixed spectrum, although the constituent substances were not found to be present in the same number at all points of this arc; the greater number indeed were found at the middle of the arc, while at the ends they were reduced to magnesium, hydrogen and D_3 of the chromosphere, so far as this last-mentioned line may be regarded as belonging to a peculiar substance. This order in the number of substances on the portions of the edge which give a mixed spectrum, has been observed so frequently, that the regions of the sun corresponding to the aggregate of these successive tracts may be regarded as isolated portions of the solar surface, in the centre of which there is found a large number of different substances, this number diminishing towards the edge of each of these regions, where it reaches its minimum; and if we leave out of consideration the materials of the chromosphere which are common to the whole surface of the sun, we may say that at the borders of these regions there remains nothing but magnesium. In these tracts or regions, indeed, though the composition might be different in different parts, magnesium was never absent; for this reason Tacchini designates these portions of the surface as *magnesium regions*.

Instead then of special points marked by eruptions, we find on the sun's surface large regions exhibiting throughout a complex spectrum, but not covered by protuberances; and this affords the strongest argument for not regarding the protuberances as a phenomenon of true eruption. We cannot therefore admit the existence of a solid or highly resisting liquid stratum, but rather a purely gaseous envelope, such as may permit of the ready mixing of the internal materials with those of the chromosphere on a vast scale, thereby giving rise to the regions above designated as regions of magnesium.

This view, however, does not absolutely exclude the occurrence of eruptive phenomena; for if these materials show themselves in the chromosphere at certain determinate points, they must have issued or been ejected from the centre towards the circumference. Tacchini, however, thinks that he has proved that these eruptions are not violent, and do not take place through a strongly resisting medium.

Contemporaneously with the determination of the angles of position of the protuberances, and of the portions of the chromosphere which exhibited a mixed spectrum, similar determinations were made of the positions of the faculæ and regions of faculæ visible or near the edge. A comparison of the two series of determinations thus made showed the coincidence of the magnesium regions with the regions of the faculæ. The positions of the protuberances on the other hand did not coincide with either.

The limits of the regions of magnesium and of the faculæ, on either side of the sun's equator, were found to be as follows :—

Regions of Magnesium, August 1871	+ 60° − 27
September	+ 60° − 32
Regions of the Faculæ, August	+ 43° − 32
September	+ 64° − 32

Considering now the magnesium regions above described, and the intensity of the phenomena there exhibited, it is clear that at any given epoch, the luminous intensity of the solar disc may vary considerably, and exhibit great differences as compared with that of the general envelope. At such times the solar atmosphere will not be uniformly illuminated, but will include a number of cones, varying in extent and brightness according to the different magnesium regions existing on the sun; and Tacchini suggests that in the case of total eclipses of the sun, during which, the solar atmosphere becomes visible to us in the form of an aureola, this aureola which is differently illuminated in different parts, and therefore presents the appearance of plumes perpendicular or oblique to the edge of the moon, may arise from the cones embracing a large extent of surface corresponding to that of the magnesium regions.

Whilst Tacchini was studying the magnesium regions, Lorenzoni obtained evidence that the temperature of the sun's surface is least at the poles. The regions occupied by the metallic vapours corresponding to a certain line, which probably belong to the spectrum of iron, are called by Tacchini iron regions; they do not coincide with the regions of magnesium and of the faculæ, or with those of the protuberances, which are more limited.

Since the vapours of iron are diffused in the chromosphere on so vast a scale, and the magnesium regions also are so large as has been previously shown, Tacchini asks is it possible to admit the existence of a state of solidity or viscosity in those envelopes, or a temperature so low as some persons suppose? He regards such a view as totally inadmissible, and considers that all the observations above detailed point to the conclusion that the time is yet very distant when the sun will approach to those transformations which have reduced the earth to its actual state; and that the sun is still an entirely gaseous mass, relatively hotter at the centre, and cooler in the superficial strata, which we distinguish by the names of photosphere and chromosphere.

ASTRONOMY

On the Meteors of April 30-May 1[*]

Prof. Schiaparelli, in his list of meteoric showers whose radiant-points are derived from observations made in Italy within the last few years, describes one as occurring on April 30 and May 1, the apparent position of whose radiant is in the Northern

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[*] Tacchini's observations divide prominences as Lockyer has divided them.

[†] Proc. R.S. vol. xviii. p. 75 1879

[*] From *Silliman's Journal* for July, 1872.

Crown, R.A. 237°, N.P.D. 55°. The same shower has also been recognised by Robert P. Greg, F.R.S., of Manchester, England. This meteor-stream, it is now proposed to show, is probably derived from one much more conspicuous in ancient times.

In Quetelet's "Physique du Globe," pp. 290-297, we find meteoric displays of the following dates. In each case the corresponding day for 1870 is also given,* in order to exhibit the close agreement of the epochs.

1. A.D. 401, April 9; corresponding to April 29, for 1870
2. ,, 538 ,, 6 ,, ,, 25 ,,
3. ,, 839 ,, 17 ,, May 1 ,,
4. ,, 927 ,, 17 ,, April 30 ,,
5. ,, 934 ,, 18 ,, May 1 ,,
6. ,, 1009 ,, 16 ,, April 28 ,,

The epochs of 927 and 934 suggest as probable the short period of seven years. It is found accordingly that the entire interval of 608 years—from 401 to 1009—is equal to 89 mean periods of 6·8315 years each. With this approximate value the six dates are all represented as follows:—

From A.D. 401 to A.D. 538 we have 20 periods of 6·85 years
,, 538 ,, 839 ,, 44 ,, 6·84 ,,
,, 839 ,, 927 ,, 13 ,, 6·77 ,,
,, 927 ,, 934 ,, 1 ,, 7·00 ,,
,, 934 ,, 1009 ,, 11 ,, 6·82 ,,

This period corresponds closely to those of several comets whose aphelion distances are somewhat greater than the mean distance of Jupiter. So long as the cluster occupied but a small arc of the orbit, the displays would evidently be separated by considerable intervals. The two *consecutive* showers in the tenth century indicate, however, an extensive diffusion of the cluster at that epoch; so that the preceding part passed the node April 30, 927, and the following part, May 1, 934; the interval being somewhat more than one complete period. The comparative paucity of meteors in modern times may be partially explained by the fact that the ring has been subject to frequent perturbations by Jupiter.

It is not impossible that this meteor-stream was connected in its origin with the comet which passed its perihelion on April 29, B.C. 136.

DANIEL KIRKWOOD

SOCIETIES AND ACADEMIES

LONDON

Geologists' Association.—The excursion to Ludlow and the Longmynds on July 22 and five following days, the concluding and most important field meeting of the season, was under the direction of Prof. Morris, F.G.S., Mr. R. Lightbody, F.G.S., and the Rev. J. D. La Touche, B.A. After the members had assembled in Ludlow Castle the Upper Ludlow rocks on the right bank of the Teme were examined, and Mr. Lightbody gave his reasons for considering the Aymestry Limestone to be represented at a point near the old bridge, although *Pentamerus Knightii* is not found here. The Upper Ludlow contains *Chonetes lata* in great abundance, and scarcely a fragment of the rock was picked up without this species being seen on the surface. From the high ground by the river side the valley of the Teme may be advantageously seen. The river here flows through a gorge in the Upper Ludlow rocks, with the castle and town of Ludlow picturesquely situated on the left bank; while the "Old Red" country extends beyond to the Clee Hills, the igneous summits of which commandingly rise to the east, surrounded by the coal measures. The well-known section in Ludford Lane, showing the "bone-bed," was next visited. This bone-bed, or "gingerbread," as it is sometimes called from its appearance, is in places not more than a quarter of an inch thick, and is found only after careful search. The fragments of the remains of fish, of which it is partly made up, were until comparatively recently the oldest fish remains known. The following day (Tuesday) was devoted to an examination of sections of the Aymestry Limestone, Lower Ludlow, and Wenlock rocks, occurring in the course of a route of about twenty miles. The journey was performed by means of carriages, but abundant occupation was given for the hammers of the party at the various exposures of the richly fossiliferous rocks above named. Graptolites from the Lower Ludlow were obtained in abundance, and fine specimens of *Phacops longicaudatus* were found in the Wenlock, exposed in the bed of the Teme, near Burrington; while *Pentamerus Knightii* was seen in great profusion in the Aymestry Limestone of Ruacktree. Near Comus Wood (so called from being the scene of Milton's "Comus") a very extensive view is obtained of the "Old Red" district of Herefordshire, with the Malverns distinctly seen in the distance. In the evening the members were entertained at a *soirée* given by H. Salway, Esq., of "The Cliff," Ludlow. During Wednesday the Upper Ludlow rocks in the valley of the Teme were subjected to further examination, and the party proceeded as far as Downton, where the uppermost members of the series are seen at the Tin Mills section. At one point on the road to Downton the physiography of the district to the north of Ludlow is well seen, and here Prof. Morris pointed out the principal features of the extensive landscape, and showed how entirely due they were to their geological structure, and that the coal measures of the Clee Hills had been preserved by the old volcanic outbursts which had formed the central masses of hard "Dhu-stone" composing the summits and caves of these lofty hills. On Thursday the party ascended the Longmynds at their southern extremity, where masses of quartzose conglomerate of Cambrian age protrude from the surface. The Rev. Mr. La Touche described the topography and the geology of the district seen from the elevated ground on which his hearers were assembled, and, subsequently, Dr. Hicks explained the order of the succession of the Cambrian and Lower Silurian rocks of St. David's, which he had been re-examining during the previous fortnight, and which he considers to have representatives in some of the beds of the Longmynds. The Llandovery conglomerates lying on the uplifted beds of the Cambrians at an angle of 22° were found to be exposed on the eastern slope of the hill, and the "Pentamerus limestone," with its characteristic *Pentamerus oblongus*, was also seen. The party then proceeded to the quarries of Caradoc sandstone in the Onney valley, at one of which Prof. Morris gave a general description of the Silurian system and the extension in England of its various members. Friday was occupied by a journey in carriages to the mining district of Shelve, and by an inspection of the very interesting hill-country between that place and Church Stretton. Quarries in the Cambrian rocks at the south end of the Longmynds and in Llandovery beds near Norbury occasioned stoppages, and afterwards a visit was paid to Linley Hall, the residence of Mr. Jasper Mare, who courteously invited the party to inspect his fine model of the South Shropshire mining districts, the famous pig of lead of Roman age, with the name of the Emperor Hadrian upon it, found near Shelve, and specimens of the mineral products of the locality of extraordinary size and beauty. The members were then entertained at luncheon, after which they left Linley Hall and traversed a long, narrow, and very beautiful valley in the park, and terminating at the Stiper Stones. At a little distance from the park enclosure a mass of felspathic ash in Lower Llandeilo rocks is quarried for road metal, and the Llandeilo beds thus laid bare were eagerly and most successfully searched for fossils. After a brief visit to the White Grit Mine, the carriages were finally left, and the party commenced the ascent of the Stiper Stone ridge, from the summit of which is seen a fine panorama of the Welsh mountains, with the old volcanic Corndon in the foreground, and Cadir Idris and Plynlimmon in the extreme distance. The extraordinary masses of obtruding hard white quartzite rocks called the Stiper Stones were objects of great interest to the party, some of the members of which were not satisfied with the evidence of their being the equivalents of the Lingula flags. The Longmynds ridge, extending for nearly fifteen miles, bounds the view to the east, and this range had now to be crossed. The intervening valley affords several sections, at one of which was seen what was considered by Murchison to be the junction of the Silurians with the Cambrians. Near the summit of the Longmynds a very fine exposure of Cambrian conglomerates occurs; and further along the edges of the vertical green and purple shales and slates are seen beneath the feet as the mountain road is traversed. On the eastern side of the range the rocks are well exposed, and the indenting gorges numerous and picturesque. Caer Caradoc stands boldly out at a little distance to the north-east, with the Wenlock and Aymestry limestone ridges beyond, and bounding a valley of great beauty and extent, terminated northwards by the volcano like cone of the Wrekin, at the foot of which the Severn flows through a deep gorge. The morning of the concluding day, Saturday, was given to an examination of the Upper Ludlow rocks, the "Bone-bed," and the Downton sandstone in the neighbourhood of Morton

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* Making proper allowance for the precession of the equinoxes.

Camp and Ovibury; and the week's proceedings concluded with a visit to Stokesay Castle, in which the Rev. James Parker gave an interesting account of the curious old pile. Thanks were most warmly accorded to the Rev. Mr. La Touche and to Prof. Morris for their able conduct of the excursion, and the members then took their departure from the Craven Arms Station, congratulating each other on the very interesting, instructive, and successful character of the visit of the Geologists' Association to Shropshire.

KENT

East Kent Natural History Society, August 1.—A communication was made by Mr. Gulliver, F.R.S., in relation to the shark (*Lamna Cornubica*) taken last November off Rye. As this is the first description of this important skeleton of this huge fish, which may now be seen at the College of Surgeons, we give it at some length. This shark is the Porbeagle of many authors and the Beaumaris Shark of Pennant. Every anatomist knows more or less how an ordinary natural skeleton is made; but as this of the Porbeagle is an extraordinary one, it is well worth while to note some of the means employed in its preparation. In the first instance careful measurements were made of the different appendages, and kept for guidance in regulating their due position, since in the drying there would be much distortion or displacement which could only be corrected by a constant reference to their state in the fresh fish. Then came the question, how to get out the brain; and this it was found could be easily done through a natural opening—a sort of fontanelle—more than an inch in diameter, in the upper and front part of the skull. Next, it was foreseen that, in such a large fish, there would be great shrinking in its length from the contraction by drying of the intervertebral substances, as had happened to the skeleton of this shark at Haslar; and this fault was prevented by the insertion between the bodies of the vertebræ of temporary wedges or plugs of wood. And as the skull and orbits, being cartilaginous, would shrink and curl into a shapeless and ugly mass, unless means could be devised to preserve their form, all these parts were supported by plaster casts, while the foramina were kept open by wooden plugs. In this state six weeks were passed in the drying, although this was often hastened by artificial heat. The plugs and plaster being removed, the skeleton parts were left in their natural form and position, as now so admirably preserved for the instruction of anatomists. The eyes, too, are shown *in situ* without the least shrinking. The spine has been strengthened by a strong cane introduced along the neural canal, and remaining permanently there, but not visible without curious inspection. It is remarkable that there is but little fatty matter in the skeleton. Among the manifold parts of the skeleton are seen, in their natural position, the five pairs of Branchial Arches; the Hyoid Arch with its three pieces on each side, and the Branchiostegous Rays; the Scapular and Pelvic Arches; and, as appendages of the pelvis, the pair of osseous Claspers, each of two pieces and a curious Spine of hard bone, particularly noticed by Prof. Flower, at the free end. The Vertebræ, of which the number has not hitherto been recorded in this species, are, as counted by Prof. Flower and Mr. Gulliver, no less than 152, of which 60 belong to the tail. These caudal vertebræ turn abruptly upwards at an angle of about forty degrees from the straight vertebral column of the trunk, and run straight along the upper border of the superior lobe of this caudal fin. The frame-work of this fin-lobe is chiefly formed of the caudal vertebræ, with their broad and flat inferior spinous processes; the lower lobe of the caudal fin is composed of a densely-packed layer or plate of parallel rays proceeding from above downwards, and apparently of fibro-cartilaginous texture. The vertebral column has no ribs. The Rays of the front Dorsal Fin are distantly jointed; the joints like those of soft-finned bony fishes, but much further apart in each ray; and this is so remarkable in the Pectoral Fins of this fish as to remind us of the digital phalanges of mammalia. Of course, every ichthyologist well knows that the caudal fins of the Plagiostomes are unequal (heterocercal); but it is not so familiarly known that the caudal vertebræ in several of these fishes, and also in some other fishes, pursue a different course. Indeed, the disposition of the caudal vertebræ of osseous and cartilaginous fishes, both in adults and in the different stages of development, affords, as Agassiz and Huxley have recognised, a very interesting subject for more research than has yet been devoted to this branch of ichthyology. Meanwhile we have in this skeleton of the Porbeagle a noble contribution to the osteology of the Selachians.

PARIS

Academy of Sciences, July 22.—A paper was read by Prof. Cayley on the conditions enabling a family of given surfaces to form part of an orthogonal system.—M. A. de Caligny communicated a note on a liquid vein formed in part by a current, and in part by the blows of the waves against two convergent breakwaters.—A note on the vibrations of cords under the influence of a diapason by M. E. Gripon was read.—M. F. Lucas communicated the results of experiments made by him in the Seine during the siege of Paris for the purpose of ascertaining how far the waters of the river would convey sounds which might be employed for telegraphic purposes. He found that the sounds produced by heavy bells were not transmitted more than 1,500 to 1,800 metres.—M. W. de Fonvielle described a new example of the danger caused by large masses of metal during thunderstorms.—M. Le Verrier read a memoir on the masses of the planets and the parallax or the sun, in which he indicated that in the present day the exact determination of these and some collateral matters had become a necessity, and dwelt especially upon the desirableness of a new direct measurement of the velocity of light. Upon this subject MM. Fizeau and d'Abbadie made some remarks.—M. Boussingault communicated a note on the determination of iron in the blood of an invertebrate animal. The animal employed was the common garden slug; its blood contains only 0·00069 per cent. of iron.—A note was read by M. P. Thenard on a new process for the quantitative determination of ozone, and a second by the same author on the action of permanganate of potash or oxygenated water in the midst of a freezing mixture.—M. Sace presented a memoir on a new process for the preservation of alimentary substances by means of acetate of soda.—M. Berthelot communicated a note on the constitution of acid salts in solution; and MM. P. Champion and H. Pellet a note on the theory of the explosion of detonating compounds.—M. A. Boillot described a process for the preparation of ozone by means of a new mode of production of the electrical effluvia.—M. C. Bernard communicated a further note by M. Oré on M. Liebreich's endeavour to demonstrate that strychnine is an antidote to chloral.—M. C. Robin presented a note by M. Rabuteau on the physiological properties of quinic acid, and on the reduction of perchloride of iron in the organisms; and M. C. Sainte-Claire Deville communicated a letter from M. Diego Franco on the late eruption of Vesuvius.

PAMPHLETS RECEIVED.

ENGLISH.—Cassell's Book of Birds, Part XXIII.—The Lead and Zinc Mines of the Mendip: H. B. Woodward, F.G.S.—What determines Molecular Molecular Motion? the Problem of Nature.—The Industrial Monthly, No. 5, Vol. vii.—The Journal of Applied Chemistry, No. 7, Vol. vii.—A Letter to the Most Noble the Marquis of Salisbury on the Public Health Bill: G. W. Child, M.A.—The Building and Ornamental Trades of Great Britain and Foreign Countries. E. Hill.—Grevillea, No. 2: M. C. Cooke.—Proceedings of the Geologists' Association, No. 6, Vol. ii.—The Monthly Microscopical Journal, August.—The Astronomical Register, August.—The Publishers' Circular, August.—Journal of the Chemical Society, July.—The Food Journal, No. 31, Vol. iii.

AMERICAN AND COLONIAL.—The American Chemist, No. 12, Vol. ii.—The Canadian Naturalist, No. 4, Vol. xiii.—The American Naturalist, No. 7, Vol. vi.—The American Journal of Science and Art, No. 18, Vol. iii.—The Cincinnati Medical News, Nos. 5 and 6, Vol. i.—The Indiana Journal of Medecine, Nos. 1 and 2, Vol. iii.—Van Nostrand's Eclectic Engineering Magazine, No. 44, Vol. vii.

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THURSDAY, AUGUST 15, 1872

THE BRITISH ASSOCIATION

THE recurrence of our annual Congress of Science naturally leads us to reflect on the position which the British Association occupies in our social economy, and on the part it is qualified and, perhaps, destined to play. Whilst other scientific societies occupy themselves in giving publicity to results and speculations, and in rewarding with their medals successful labours, the British Association alone systematically undertakes to distribute the greater part of its income, about 2,000*l.* per annum, in grants to enable men of science to conduct scientific investigations, and to institute inquiries with a view to possible future action. Stated briefly, this constitutes the broad distinction between the British Association and other scientific societies. As a publishing society it cannot vie with some other bodies, as, for instance, with the Royal Society. The great bulk of the papers it receives are published in abstract and but few *in extenso;* and it allows a greater latitude than other societies with regard to the reception of subjects which have been elsewhere made public, thus constituting its proceedings, to a great extent, a *résumé* of the year's work—a characteristic quite in keeping with its practice of meeting but once a year.

It is distinguished also by the wide range of subjects admissible in its various sections, by the facility with which membership is granted, by its attractiveness to foreign men of science, and, above all, by the tendency which its practice of meeting each year in a different town has to disseminate Science throughout the kingdom.

All these characteristics combine to make the British Association a truly national body. And the feeling that it is so has always made its leaders more ready to interest themselves in large national objects connected with Science, than, as members of other societies, the same men have elsewhere shown themselves to be. The British Association has thus always exhibited more self-assertion, and, in its communications with Government, more boldness, than other societies. In other scientific circles there is a disposition to regard any assistance given by the State as a favour to Science, and a timid reluctance to point out plainly cases in which the aid of the State is really necessary. The total Solar Eclipse of 1871 is a case in point. The Astronomical Society had not the heart to place before the Government a clear statemen of what was required. The duty of doing this, which obviously devolved on that Society, being thus neglected by it, was at once, without a moment's hesitation, successfully performed by the British Association, to the great benefit of knowledge.

But a more striking and far more important example of the wise vigour which has generally characterised its counsels, is afforded by the steps it took to obtain a thorough inquiry through a Royal Commission into the whole condition of scientific action and administration in England. Whether that inquiry leads to immediate reform and expansion of our scientific institutions, as it is generally expected it will, or not, the inquiry itself has already been productive of incalculable good. It will probably be found, when the evidence is published, that our "system" at present consists of a mass of inconsistencies, and deficiencies, of the existence of which not even those who originated the movement could have had any clear idea, much less any reliable proof. The utter absence of any guiding principle in the dealings of successive Governments with Science, and of any system for administering such imperfect and dislocated institutions as we possess, which we are convinced the inquiry must also establish, will so startle all thinking men, whether scientific or not, that sooner or later reform must come, although the causes of this state of things are not far to seek.

Another good result of the inquiry is that it has forced the large body of men of Science who have been examined to turn their attention from that too rapt contemplation each of his own labours to which English philosophers are addicted, towards the great fields which others are cultivating; and by forcing them to regard Science as a whole, to recognise and duly appreciate the individual value and the interdependence of its several parts. The change in scientific thought which has taken place in the course of the two years during which the Royal Commission has been sitting, is quite perceptible to those whose attention is turned to the subject.

But no change of thought is perceptible in the Ministry of the day. It is perfectly clear that now, as ever, any aid given to Science is a mere question of pressure. Sometimes it is yielded with apparent promptitude to the external force of numbers, importunity, or probable popularity. At other times it is as stubbornly refused. An example of each is of recent occurrence. Aid was given to expeditions to observe the two last total Solar Eclipses, in the shape of several thousand pounds, and the use of ships. The aid of 150*l.* was refused to the British Association for completing tidal investigations, on which that body had spent 600*l.* As it is impossible to refer these two acts to one and the same guiding principle, we must assume that different motives prompted each, and that, as no properly instructed mind could consider Eclipse Observations many times more important to a great naval and maritime country than Tidal Researches, compliance with the one demand cannot be set off against refusal of the other in assessing the real regard for Science to be credited to the Government.

These two well-marked cases, the miserable Hooker-Ayrton wrangle, the treatment of the Society of Antiquaries recorded in our last number, and the declaration of Mr. Gladstone at the Royal Society's anniversary dinner that Science must suffer if aided, or as he expressed himself, if "interfered with," by the State, are all indications that the Government do not yet know that it is possible to draw a boundary line separating the regions of scientific activity which should be occupied respectively by individuals or private bodies and by the State. And the truth does not yet seem to have dawned on them that the prolonged neglect of those scientific objects which State resources alone can attain is a positive dereliction of duty, the effect of which in overweighting England in the race of European civilisation is already perceptible.

Our immediate object in drawing attention to the unsettled and phlegmatic views of the Government with respect to Science is to raise the question whether the British Association cannot reinforce the healthy tone of thought they have brought about through the Royal Commission obtained by their influence. We believe that this question will be raised in a more formal manner at Brighton; but as it was first suggested in these columns,* we may, without impropriety, give it our advocacy.

It is thought by many that the perplexed and perplexing way in which the relative functions of individual and of State action in Science are now confounded, has its origin in a great measure in neglect of classification on the part of private persons and private bodies. And the opinion is spreading widely that the British Association itself has not sufficiently discriminated, in distributing its funds, between objects which individuals are perfectly able to compass, and which they should be encouraged to undertake, and those which the State alone can successfully grapple with, and which, by reason of their evident importance to the community at large, the State is therefore bound, as a matter of duty, effectively to provide for.

We shall not here attempt to indicate the tests by which these two classes of scientific objects may be distinguished. If the principle be but admitted that a distinction does exist, the necessary rules for enforcing it may safely be left to the wisdom of the Association to draw up. Its experience is very wide, and its records will supply ample materials for ascertaining what are the purposes which, with the best intentions, it has been unable to attain, and on which its grants have been virtually wasted. These will afford data sufficient for the construction of a code of rules applicable to almost every case that can come before it.

The next question is, how should these tests be applied? We are satisfied that the time has arrived when the Association may with perfect propriety, and with the certainty of the most beneficial consequences, decline to allot any portion of its funds to purposes which should by rights be undertaken by Government. We are far from counselling this step as a retaliation for such refusals of State help as that respecting, for instance, the tides. Any such feeling would be quite unworthy of such a body as the British Association. Its grounds for doing as is proposed would be perfectly clear, and entirely free from any suspicion of antagonism or irritability. First, many of the objects which the Association has attempted to attain have been distinctly proved to be too large for its resources, and to require official machinery which it cannot command. The question of Sewage is a marked example of this class. But the fact that the Association has taken up such a subject leads to the mistaken belief that it is properly provided for; and it is not till some years have elapsed that the truth breaks upon us that the time and money expended upon it have been almost wholly wasted, and that the question remains pretty much in its original condition—not appreciably advanced. The attempt to deal with such problems with insufficient means results, therefore, in delusion and delay. Secondly, to deal with all classes of scientific questions without discrimination, perpetuates and deepens the obscurity which prevails in England as to the duties of the State. No one has yet been bold enough to maintain that the State should do nothing whatever for Science, and that what is at present done should be discontinued; but scarcely any one seems to have a clear idea of the principles on which such duties should be defined, and on which expansion should proceed. If once such a definition is arrived at, the main difficulty will have been overcome. If it is once settled by competent authorities that certain inquiries, or experiments, or observations, should, by reason of their expensiveness, of their value to the community, or of the length of time they must occupy, be undertaken by Government, the first step will have been taken towards that organisation of State Science which it is clear must not be much longer delayed.

Now, there can be no more practical mode of arriving at such a definition than that of firmly refusing to apply private funds to public purposes, as here proposed. There will at first be some difficulty in effecting the necessary classification, and it will be well not to apply it at the outset too rigorously; but by degrees the difficulty will vanish, and it will be as easy to say what subjects devolve on the State as it now is to say what subjects appertain to particular sections of the Association.

Another great advantage which will ensue will be the amount of funds thus set free for assisting those objects which can be effectually attained by individual enterprise; the number of which is very great. It is well known, and much to be lamented, that many of these invaluable undertakings are starved for want of those very funds which are now spent in the vain endeavour to do the State's work.

The subjects which the Association may thus pronounce to be not within its province should not be lost sight of. An enumeration of them should be submitted annually to Government, and the resulting action taken on them by Government should be regularly reported to the Association and published. The effect of this would be to assist the Government in arriving at some measure of the scientific work which must be done by them, if done at all. This will soon be shown to be enormous in extent and variety. Attention will next be called to the machinery existing for such purposes. The first question will be, If such and such investigations are to be undertaken, which department shall be made responsible for them? And this must bring out prominently the absurdity of our present arrangements, whereby the various scientific institutions of the State are scattered amongst the various departments, and must lead to what is the fundamental requisite—concentration of all such institutions under one department responsible for the whole.

Nothing that the Association can do would, in our opinion, conduce so directly to this desired end as the classification of the applications made to them for funds in aid. The object is one in strict keeping with its traditions, and quite worthy of its ambition, destined as it is to be the High Court of Appeal in Science, and the prime mover in all that concerns material and philosophical progress. In the present day no considerable measure is undertaken by the State except in obedience to an impulse from without. This will clearly be the case with respect to Science. The first impulse has been given already by the British Association. We foresee that the issue will depend materially on the persistent firmness with which its first efforts are followed up by that powerful and useful body. Success must crown them at last, and sooner perhaps than some at present anticipate.

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* *Vide* "The Tides and the Treasury," June 27, 1872. No. 139, vol. vi.

THE BEGINNINGS OF LIFE*

II.

LEADING on to the newer and more important observations in the latter portion of the work, we have a sketch of the relation of crystals and organisms, in which a variety of curious and suggestive facts are adduced, tending to show that there is a striking analogy, if nothing more, in their mode of origin. The influence of changed conditions is shown to produce very similar results to both, and the views of Mr. G. H. Lewes—that organisms are not always united by the link of a common heritage, but that many may owe their similarity to having originated under the influence of uniform organic laws acting under uniform conditions—is quoted with approval. Just as similar crystals are produced in similar liquids under like conditions, so may low organisms of similar or identical structure be produced; and just as the fragments of a crystal will, under favourable conditions, form each an entire and perfect crystal, so do low organisms multiply by fission, each part becoming a perfect whole. The difference between crystals and organisms is said to be less radical than has been supposed, and is mainly due to the much greater complexity and instability of the molecules which go to build up the latter. Crystals are statical; organisms, dynamical aggregations of molecules. Specks of new living matter soon aggregate into certain definite forms just as crystals do, but being much more complex and unstable, they are liable to much greater variations and successive modifications. The excessive variability and instability of low forms of life is dwelt upon as an anomaly on the ordinary theory, when viewed in connection with their supposed wonderful stability for immense periods of time. It is generally believed that every one of the lower animals is a descendant of other low forms which lived in ages far anterior to the Silurian epoch. Many of the foraminifera.

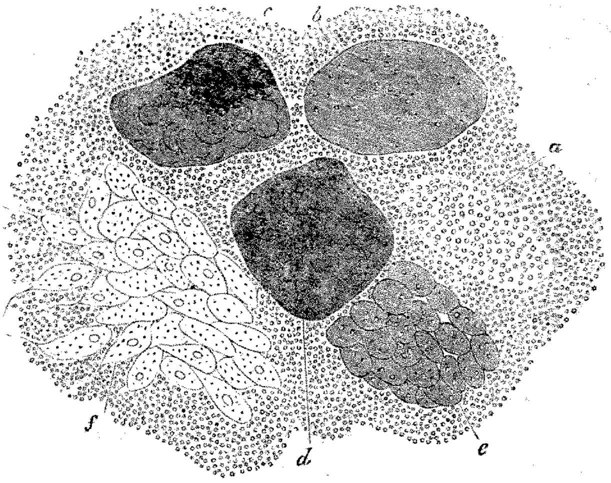

FIG. 1—SEGMENTATION OF EMBRYONAL AREAS INTO MONADS—(× 1,670).

a. First stage of differentiation. *b*. Second stage; area almost homogeneous and refractive. *c*. First traces of segmentation. *d*. Segmentation more complete; units highly refractive. *e*. Units less refractive; forming tailless corpuscles. *f*. Fully developed Monads derived from such corpuscles.

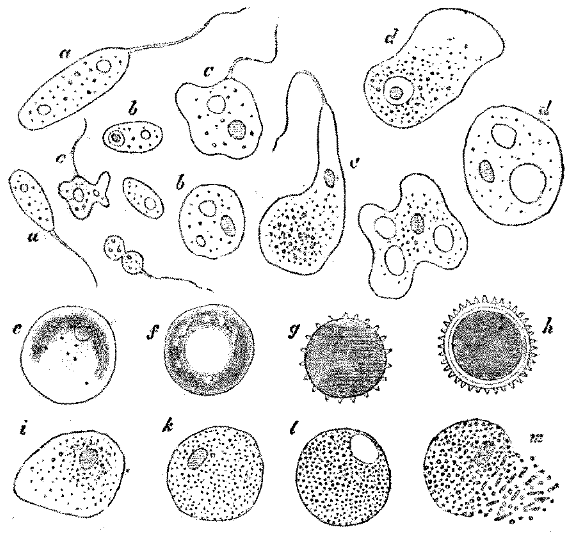

FIG. 2.—PHASES IN THE LIFE-HISTORY OF MONADS AND AMŒBÆ—(× 1,670).

a, a. Monads in different stages of growth. *b, b*. Similar Monads which have lost or retracted their flagella. *c, c*. Monads about to be transformed into Amœbæ. *d, d*. Resulting Amœbæ in active and motionless stages. *e, f, g, h*. Stages by which motionless Amœbæ become encysted. *i, k, l, m*. Stages by which other Amœbæ become resolved into Bacteria.

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* "The Beginnings of Life: being some account of the Nature, Modes of Origin, and Transformations of Lower Organisms." By H. Charlton Bastian, M.A., M.D., F.R.S. (2 vols. London: Macmillan and Co. 1872.)

for example, have hardly undergone any essential change, the same forms and varieties recurring at very distant geological periods. If, however, living matter does continually come into existence, the lowest forms will probably have been very similar in all ages; and it is only as these forms developed into more complex organisms that the varying conditions of the different periods will have led to the development of specialised groups.

The nature and mode of development of the low organisms found in infusions is next elaborately discussed, with the following result:—"No other conclusion remains for us, but that the several organisms are products of the direct developmental unfolding of new-born specks of living matter. And yet among these forms we see Bacteria, Vibriones, Leptothrix, and Torulæ; Fungus filaments, with and without fructification; Protamœbæ and flagellated Monads; Pediastreæ and Algoid filaments. All these are therefore proved with the greatest certainty to be interchangeable forms, which may be assumed on different occasions by newly evolved specks of living matter." Evidence is also adduced of the changes in other low forms. Green corpuscles thrown off from a single Lichen have been seen by Dr. Hicks to assume the forms and mode of growth characteristic of no less than twenty-three supposed species of Algæ; while gonidia from an Alga or from a Moss were developed into Lichens, Algæ, or Mosses, according to the conditions under which they were placed, while they may sometimes give birth even to active Monads.

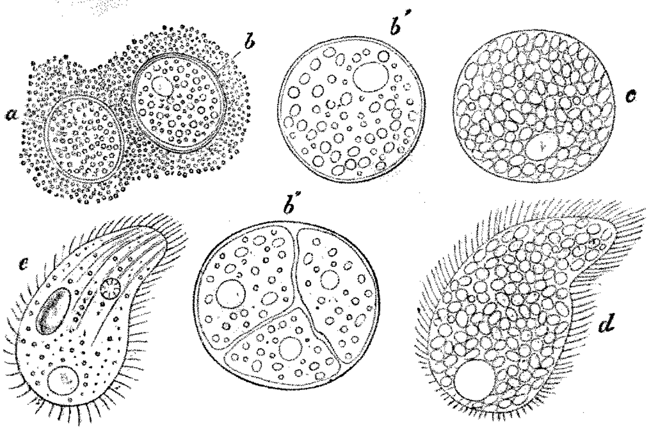

FIG. 3.—MODE OF ORIGIN OF PARAMECIA—(× 800).

a. First stage of differentiation. *b.* Later stage, in which vacuole has appeared. *b*¹. Similar stage of much larger embryo. *b*². Another embryo which has segmented into four (only three parts visible). *c.* Later stage; embryo filled with large particles, and revolving within its cyst. *d. Paramecium* after it emerges from its cyst. *e. Nassula*-like form into which many afterwards passed.

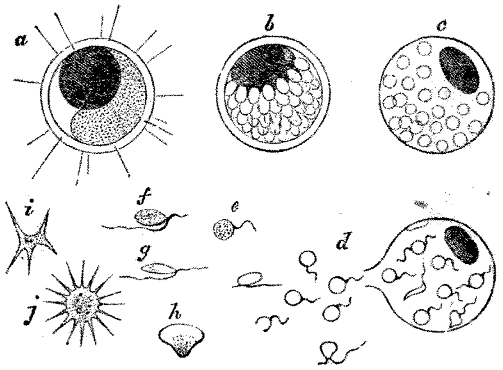

FIG. 4.—HETEROGENETIC ORIGIN OF MONADS FROM NITELLA (CARTER)—(× 350).

a. Contents of new-formed cyst separating into Protoplasm and dark brown refuse matter. *b, c, d.* Segmentation of the Protoplasm into Monads, which afterwards escape from the ruptured cyst. *e, f, g.* Different forms of the Monads. *h, i, j.* Forms of Amœbæ and Actinophrys which the Monads subsequently assume.

Having clearly proved that Bacteria and other low organisms, which form a pellicle on the surface of infusions and other liquids, are produced *de novo* in such infusions, the third part of the work, entitled "Heterogenesis," is devoted to a history of the microscopical examination of the changes which take place in this pellicle, and of all that is at present known of the transformations of the various classes of organisms to which it gives birth. To make this part of the subject clearly intelligible, it will be necessary to reproduce a considerable number of the woodcuts by which these changes are illustrated. One of the most simple series of changes—this transformation of motionless corpuscles into ordinary Amœbæ—was closely watched by Dr. Bastian, and seen with the most perfect distinctness in thousands of instances. Fig. 4 shows the stages by which the more highly organised Monads are developed. The first step was an increase of the amount of gelatinous matter between the corpuscles or Bacteria, which gradually became less defined, and at last scarcely visible in the protoplasmic mass, in which segmentation then began to take place, and continued till it separated into active Monads. After a time, however, these again began to change into Amœbæ, and these latter, passing through a motionless and encysted stage, became resolved into Bacteria (Fig. 5). The whole series of these changes occupied about ten days. In other cases similar corpuscles developed into fungi; while in some instances in the same pellicle the change into Amœbæ on the one hand, and into Fungus germs on the other, went on simultaneously. It was soon discovered that the temperature at which the infusion was made was of great importance. If it had been heated to 212° F. no development beyond Bacteria occurred; if at 149°—158° F. Fungus germs arose; while an infusion in all other respects similar, but prepared at a temperature of 120°—130° F. gave rise to actively-moving Monads.

A step further takes us to the "spontaneous eggs" of Pouchet, which are seen to be formed in the pellicle, and afterwards give birth to Paramecia—highly organised ciliated Infusoria. These never appear except in infusions made with cold water, and Dr. Bastian assures us that he has verified Pouchet's observations in all essential particulars, as represented in Fig. 6; and the still more complex Vorticellæ have been seen to arise in a similar manner. Now the germs of Ciliated Infusoria are comparatively very large and easily recognisable; they have never, or very rarely, been discovered in the atmosphere; and no competent observer could overlook them; so that it is almost impossible not to accept the fact of the origin of these organisms in the manner here described.

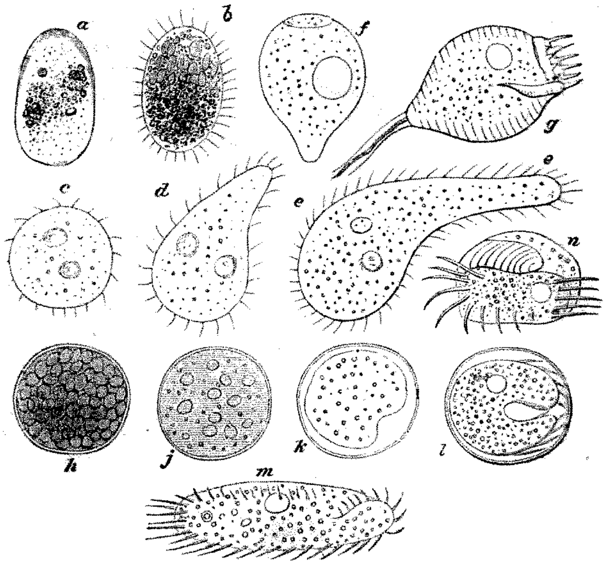

FIG. 5.—MODES OF ORIGIN AND DEVELOPMENT OF CILIATED INFUSORIA—(× 600).

a. A transforming Euglena with red "eye speck" still visible. *b* A similar body, having many of its chlorophyll corpuscles still green, fringed with almost motionless cilia. *c.* A completely decolourised sphere derived from a transformed Euglena, provided with a few partly motionless cilia. *d, e.* More advanced forms of a similar embryo developing into a Dileptus (?). *f.* Vorticella, soon after its emergence from a cyst of Euglena origin, which subsequently develops into a striated variety (*g*). *h.* A large Chlorococcus-vesicle whose contents gradually undergo decolourisation (*j*), and at last become converted into an animalised mass (*k*), which gradually shapes itself into the form of an Oxytricha (*l*); this after a time ruptures its cyst and soon takes on the characteristics shown at *m.* *n.* A form of Plæsconia derived from an embryo produced within other apparently similar Chlorococcus vesicles.

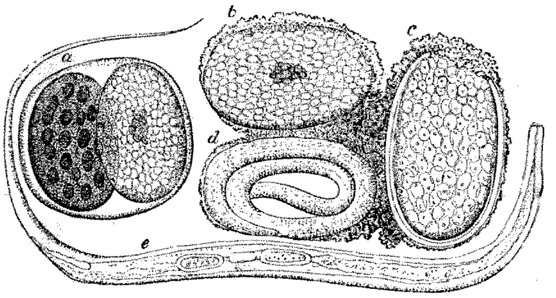

FIG. 6.—ORIGIN OF NEMATOIDS FROM EUGLENÆ (GROS).

a. A large Euglena which after encystment has undergone fission, whilst one of the halves has become decolourised. *b.* An Euglena which has become converted into a decolourised embryonic mass, leaving only a small coloured remainder. *c.* Another decolourised mass, which, after undergoing certain changes, becomes converted into a young Nematoid, as at (*d*). *e.* A female specimen of the developed Nematoid three weeks old, in whose ovaries two partially developed ova are seen.

The course of the argument is at this point interrupted by a chapter on the Atmospheric Germ Theory, which, though exceedingly interesting and well written, is quite out of place here; and we then come to some curious observations on the production of organisms within the closed cells of various plants. M. Trecul, a distinguished French botanist, has watched the formation of Amylobacters, low organism allied to Bacteria, and minute Fungi within the closed cells of living plants. In *Ficus carica* he discovered fungoid organisms within the completely closed cells of the medullary tissue, which, he believes, "negatives all ideas as to the introduction of germs from without." Minute crystalline tetrahedrons in cells of the bark of common elder and other plants were actually seen to be transformed into Amylobacters. The transformation of milk globules and a film of diluted cream-cheese into Fungus germs has also been closely watched by several observers, and has convinced them that these organisms have not arisen from accidentally introduced spores, but by a true heterogenetic transformation of the substance examined. Dr. Lionel Beale has discovered lowly vegetable organisms "in the interior of the cells of animals, and in the very centre of cells with walls so thick and strong that it seems almost impossible that such soft bodies could have made their way through the surrounding medium." Many other observers have even watched the transformation of the contents of healthy epithelial cells into Bacteria and Vibriones; and well-developed Fungi have been found within the uninjured eggs of birds and serpents. Now, all these facts, and a vast number of others detailed by Dr. Bastian, are claimed to be in complete harmony with the facts he has already established by his experiments with hermetically closed flasks, and with the theory of Archebiosis, while they have always offered immense difficulties to the advocates of Homogenesis, and have never been explained but by means of pure assumptions of a most improbable character.

We next come to the consideration of true Heterogenesis among lower organisms. Dr. Braxton Hicks has observed the production of Amœbæ by the transformation of the chlorophyll and protoplasmic contents of the cells of Moss radicles. Mr. H. J. Carter has closely followed the changes occurring in the cells of Nitella, one of the Characeæ, resulting in the formation of Monads and Amœbæ, as represented in Fig. 1. A vast number of observations of a similar character by many different observers are detailed, showing that the chlorophyll vesicles of Algæ are sometimes metamorphosed even into Pediastreæ, Desmids, and Diatoms.

But we must pass on to still more remarkable facts. The cell contents of Confervæ give rise to Euglenæ and Astasiæ, beautiful green organisms which abound in stagnant water, and these undergo transformations into a variety of higher or lower organisms, such as Diatoms, Amœbæ, and Ciliated Infusoria, the latter process being represented in Fig. 2. But Ciliated Infusoria themselves undergo transformation into various forms of lower animals, among others into Rotifers. The low Euglenæ are also transformed into either Rotifers, Tardigrades, or Nematoids, and the latter even grew into well-developed males and females (Fig. 3). Still more extraordinary, if possible, is the transformation of the minute Algoid Chlorococcus into the large, complex, and well-known Rotifer, *Hydatina senta.* Concerning the reality of these transformations, astounding as they are, Dr. Bastian assures us he entertains not the slightest doubt, having traced them through all their stages. The extreme prevalence and almost universal distribution of certain common forms of Rotifers, Tardigrades, and Nematoids, whose germs or ova are unusually large, and have been proved not to be universally present in the atmosphere, is inexplicable to those who disbelieve in the occurrence of heterogenetic transformation. Not only is it said to be proved that such transformations occur among Algæ, Fungi, Lichens, and Mosses, in every group of animals belonging to the class Scolecida, and in some of the lowest Annelida, but also in some of the lowest Arthropoda. In concluding this part of his subject, our author remarks:—"The fact that animals with such distinct and specific organs should arise in this definite manner from the reproductive products of a plant, will doubtless seem to many to flavour more of fable than of fact. After the observations which have been detailed, however, we must accept the occurrence of such phenomena as established facts, just as we are compelled, and are now quite accustomed, unhesitatingly to believe in the reality of other equally inexplicable phenomena. When we are able really to explain the reason of the processes by which one minute vesicular mass of fatty and albuminoid particles develops into a man, another into a fish, and another into an insect, we may then, with a little more show of reason, think of rejecting other more or less similar facts because they are incomprehensible."

Passing now from facts and observations of which we have only been able to indicate the character and extent by a few examples, Dr. Bastian proceeds to discuss the nature of "individuals" and "species" by the aid of the new light these researches have thrown upon them. He adopts the definition of an "individual" given by Herbert Spencer as being any organised mass "having a structure which enables it continually to adjust its internal relations to external relations, so as to maintain the equilibrium of its functions," and would define species to be any assemblage of individuals which are enabled for many generations to reproduce their like. But between these two he believes we must now establish a third category, for which he proposes the term "Ephemeromorphs," to include all those various forms which, although they sometimes produce their like, are shown to be interchangeable, and which, occasionally or regularly, arise from, or give birth to, forms quite distinct from themselves. All groups in which there is no differentiation of sexes are probably Ephemeromorphs, and the phenomenon of "alternate generations" in sexual animals is thought to be a recurrence to a partially Heterogenetic mode of reproduction.

The facts of Heterogenesis, if established, will undoubtedly largely modify our views as to the universality of the action of "Natural Selection." They seem to show that among the lower organisms, unknown laws of "polarity" akin to those which influence the production of crystals, but of infinitely greater complexity, directly cause the development of a vast variety of forms; while conditions of existence to a great extent determine the special forms that shall arise in each individual case. For such creatures "laws of heredity" hardly exist, and if so, Natural Selection can have little or no power. If we consider the enormous variety of forms that have been here shown to arise by Heterogenesis, it becomes evident that the field of action for Natural Selection becomes thereby considerably reduced. Again, the experiments detailed by Dr. Bastian prove the overwhelming importance of external conditions in determining the form that shall be assumed by many of the lower organisms, just the reverse of what has been found to obtain among the higher animals. And, what is still more important, the varying conditions do not act by producing changes in the adult organism which may be transmitted to their offspring, but actually so modify the developing germs as from a similar starting point to produce organisms which would rank as of distinct species, genera, or even families. The change produced seems to be quite incommensurate with the modified conditions which lead to it, and we are thus forced to accept some form of belief in innate tendencies or laws of progressive development, dependent on the polarities, forms of equilibrium, and attractive or repulsive properties of the complex physiological units of which organisms are built up. Such views are generally repudiated by modern thinkers; but Dr. Bastian believes they are necessitated by the facts now brought forward, and that they are really not only in harmony with, but almost necessary deductions from, the principles of the philosophy of evolution.

The phenomena of Heterogenesis also lead us to conclusions as to the rate of change in time of lower organisms exactly the reverse of those generally held. From having mainly studied the higher forms of life, and from having ascertained that the complex actions and reactions of such organisms on each other have been more efficient in producing specific changes than mere variability or the influence of changed conditions, Mr. Darwin has been led to the conclusion that the rate of change of the early forms of life, which had far less complex actions and reactions among themselves, must have been exceedingly slow.

This has almost the appearance of a paradox, in view of the admitted fact of the extreme variability and instability of these lower forms; yet it has been generally accepted as a sound inference from the law of natural selection, and has greatly increased the difficulty that has been felt as to the enormous time required for the development of all forms of life from the supposed primordial germs. But if the facts of Archebiosis and Heterogenesis are true, and all the lower forms of life are continually being produced *de novo*, under the influence of unknown laws of development, then we may fairly conclude that, when once the earth had arrived at conditions favourable to the production of living organic matter, the process of development would be rapid, and an immense variety of low forms of animals and vegetables would soon people it. It is a fair inference, too, that if such complex organisms as Ciliated Infusoria, Rotifers, Nematoids, and even simple Acari, can be developed independently of the slowly modifying influence of natural selection, the same laws of development will continue to act a subordinate part much higher in the scale, and, by assisting natural selection in its work, may have enabled a much more rapid progress to be made.

It is very strongly argued by Dr. Bastian that the conception of an origin of living organisms at a single remote epoch in past time, and the lineal descent of all existing organisms from those primal forms, is one quite opposed to the uniformitarian and the evolutional philosophy, and in the highest degree difficult to accept. It is almost inconceivable that Bacteria, Moulds, Monads, Amœbæ, and a thousand other minute and simple organisms, should still exist so universally over the earth, and under such an infinite variety of simple forms, if all were descended from ancestors which could hardly have been more simple in the almost infinitely remote past, and which throughout all that time had been subject to those same causes of change and advance in complexity of organisation which have resulted in the varied forms of all the higher animals. Whatever laws and conditions led to the production of the earliest organisms, they are hardly likely to have been of so exceptional a nature as never to have occurred since. It does not seem probable that the very existence of life upon the earth depended on so rare and improbable a set of conditions that, having once occurred, they should never occur again in the whole period between some remote pre-Laurentian epoch and the present day. If, therefore, there is good evidence of the continued *de novo* production of lower forms of life, and of the direct transformation of these into various higher and more complex organisms, such a view will have many *a priori* considerations in its favour, and will tend to bring the whole series of life-phenomena into greater harmony with those of inorganic nature, without in any way diminishing the mysterious grandeur that surrounds them.

But if these views should be established, we shall have to form an entirely new conception of the genealogical history of the various existing organisms. We shall no longer have one "tree of life," but a vast number of such trees, all having their roots in a similar substratum of the lowest organisms, evolved at various periods of the earth's history, but differing greatly in their subsequent development. It is probable that by far the greater number of these "trees of life" have become extinct at various periods of their growth, and that all existing living things belong to portions of but a few "trees," some of which may be comparatively recent, while others may have their roots far back in the past, anterior to the earliest epochs of which geology affords us a record. But notwithstanding this diversity and separateness of origin, through the whole life history of our globe the progress of organisation seems to have been essentially similar; which is readily explicable on the ground that living things, both as regards their origin and subsequent differentiation or development, are the immediate products of natural laws or material properties, which are probably the same now as they have ever been. Similar types of form may, therefore, again and again have arisen; and Dr. Bastian remarks, that even "the vertebrate grade of organisation *may* have been attained by ultimate branches of different trees of life." It remains to be seen how far this conception will throw light on obscure and difficult questions of biological classification, and on those facts of geological succession which are most difficult to reconcile with the usual view of all organisms whatever having originated from a single almost infinitely remote source.

It will now be seen, even from the very imperfect sketch of its subject-matter, how many questions of the highest scientific importance rise out of the facts adduced in Dr. Bastian's work. It is not too much to say that, if its main conclusions are established, it will create a revolution in organic philosophy of equal importance with that which was effected by Mr. Darwin, whose observations and most important theories will, however, remain unaffected by it. That gentleman has himself remarked that "analogy is a deceitful guide," and it is only by analogy that he extends the laws he has established for the higher animals and plants to those lower forms with which Dr. Bastian deals; and the establishment of facts proving that they come under a different category will even relieve the theory of natural selection from some of its greatest difficulties, and neutralise some of the most serious objections that have been brought against it. The whole question, however, is primarily one of facts, and, however it may be ultimately decided, every lover of science must admire the courage and energy with which Dr. Bastian has taken up an unpopular subject, the skill and patience with which he has experimented, the labour which he has bestowed in collecting the records of widely scattered and almost forgotten observations, and the logical force as well as the philosophical spirit with which he has worked out his conclusions. It is a book that cannot be ignored, and must inevitably lead to renewed discussions and repeated observations, and through these to the establishment of truth.

Alfred R. Wallace

NOTES

The Lords of the Committee of Council on Education having decided to transfer the instruction in Physics, Chemistry, and Natural History from the Royal School of Mines in Jermyn Street, and the College of Chemistry in Oxford Street, to the new buildings in Exhibition Road, South Kensington, notice has been given that in future the following courses of lectures and practical laboratory instruction will be given at South Kensington at the date specified:—Chemistry by Prof. Frankland, D C.L, F.R S. A course of forty lectures on Inorganic Chemistry commencing 21st of October, 1872. A course of thirty lectures on Organic Chemistry commencing 13th of January, 1873. Laboratory instruction consisting of an elementary and an advanced course commencing on 1st of October. Biology by Prof. Huxley, LL D., F.R.S., a course of eighty lectures on Biology (or Natural History, including Palæontology) with laboratory instruction, commencing the 7th of October, 1872. Physics by Prof. Frederick Guthrie. The course will consist of lectures, with laboratory work on the subject of the lectures, divided as follows:—Twelve lectures on Molecular Physics, Sound, &c., commencing 24th of February, 1873; fifteen lectures on Heat, commencing on 24th of March; fifteen lectures on Light, commencing on 12th of April; twenty lectures on Electricity and Magnetism, commencing on 19th of May. Each course will be complete in itself, and may be taken separately.

THE Trustees of the British Museum have nominated Dr. Albert Günther, F.R.S., to the post of "Assistant Keeper" in the Zoological Department, vacant by the decease of the late Mr. G. R. Gray. It need hardly be said that no fitter appointment could have been made. The vacancy occasioned by Dr. Gunther's promotion is, we understand, to be filled by Mr. R. B. Sharpe, F.Z.S., late Librarian to the Zoological Society, a young and rising Ornithologist, to whom the care of the National Collections of Birds will be entrusted.

IT is with great regret that we find there is too much truth in the report that the eminent astronomer and physicist M. Delaunay, Director of the Paris Observatory, has met his death by the upsetting of a boat while in an excursion on the Coast of Normandy. We hope in a future number to give a biography of this distinguished man. His loss is an irreparable one, not only to France, but to Science throughout the world.

THE meeting of the British Association for 1873 will be held at Bradford, under the presidency of Mr. J. P. Joule, D.C.L., F.R.S. At the present meeting the Association loses, with great regret, the services of one of its permanent officers, Dr. Thomas Thomson, one of the general secretaries, who will be succeeded by Dr. Michael Foster, F.R.S.

WE have to record the death of Sir Andrew Smith, K.C.B., Director-General of the Army Medical Department from 1851 to 1858, at his residence in Alexander Square, Brompton, at the at the age of 75. Sir Andrew Smith is well and favourably known to zoologists by his "Illustrations of the Zoology of South Africa." His complete and accurate knowledge of the various tribes of Southern Africa rendered his opinion of great value to successive Governments, and it was upon his representation and advice that the district of Natal was constituted a colony.

ALTHOUGH we can quite sympathise with those feelings which induced Mr. Fawcett to bring on the Ayrton-Hooker question in the House of Commons at all hazards, we consider that it is extremely unfortunate that Sir John Lubbock's determination to postpone it till next session—when the true opinion of the House, if necessary, would have been elicited—was not carried out. As it is, Mr. Ayrton has had an opportunity of exhibiting himself in his true character, which, however, was pretty well known before; and Mr. Gladstone has had an opportunity of again learning from the public press, in no hesitating tone, what is thought of his *protégé;* but the case itself has not progressed since the time we last referred to it.

IN his annual address as president of the Pharmaceutical Society, delivered at Brighton on Tuesday morning, Mr. H. B. Brady advocated the application of a portion of the surplus funds of the Society to the encouragement of scientific training. He enunciated the very sound proposition that beyond an investment sufficient to guarantee the means of carrying out the examining and governing functions entrusted to the Society by Government, there can be no excuse for the accumulation of wealth. Constantly recurring investments represent good left undone, opportunities unaccepted. Nor in this hoarding of money instead of science is the Pharmaceutical Society true to the spirit of its founders. The Society was formed to do in a collective capacity what could not be done by individuals. "It has seemed to me," he continued, "that the most substantial aid which could be rendered in the direction alluded to would be the setting apart of a number of free benches in the Society's laboratory for students who, having passed the Major examination with credit, might desire to continue their studies. These should even be endowed with a small annual income, under certain conditions, if found necessary. The only primary stipulation should be that, possessing the requisite preliminary knowledge, the recipient should be ready to work for the advancement of pharmacy under the direction of the professor. The effect of half-a-dozen or a dozen men so trained, sent out annually from Bloomsbury Square, would be to make a British school of pharmacy the like of which has never existed; and were this carried out, the most serious difficulties in the way of provincial education would resolve themselves in a few years." Should the Pharmaceutical Society follow this admirable advice of its president, it will place itself distinctly in the van of our learned societies, and will furnish an example which might be well followed by some of the others with their too abundant invested property.

A MONUMENT in honour of Jahn, the founder of the German Turnvereine, was unveiled on Saturday on the Haasenheide, near Berlin, amid enthusiastic acclamations. When shall we, as a nation, delight to honour in a similar manner the physical benefactors of mankind?

THE French Academy has elected M. Loewy, Titular Astronomer to the National Observatory, to a seat at the Bureau de Longitudes, vacant by the death of M. Laugier.

ONE of the biennial posts for practical work at the Laboratory of Cryptogamic Botany at Pavia is now open. There is attached to it a honorarium of 700 francs.

LORD NORTHBROOK has shown his appreciation of the value of scientific research by offering a gold medal for competition by the students of the Calcutta Medical College for the best essay on the exciting causes of fever, with special reference to the calamity which has for a long time devastated the Burdwan district in India, and the measures, sanitary or other, to be adopted for their remedy and prevention.

A METEOROLOGICAL Congress is being held at Leipzig from the 14th to the 16th inst. inclusive.

THE establishment of the College of Physical Science at Newcastle-on-Tyne has been followed by the formation of a similar scheme for another of our industrial centres, Birmingham, the necessary endowments being, on this occasion, given by the munificence of a single private individual, Mr. Josiah Mason, to whom Birmingham already owes so much in various ways. Being deeply convinced (Mr. Mason says, in his trust-deed) from long and varied experience in different branches of manufacture, of the necessity for and benefit of thorough systematic scientific instruction specially adapted to the practical, mechanical, and artistic requirements of the manufactures and industrial pursuits of the Midland district, and particularly of the boroughs of Birmingham and Kidderminster, he has determined to devote a portion of his remaining property to the foundation of an institution wherein such systematic scientific instruction may be given. With this object he assigns certain freehold and leasehold property situate in various parts of the town, which may be roughly estimated worth not less than 100,000*l.*, to a body of trustees in trust for the purposes of the college. Out of the net income a sum not exceeding one-tenth may be set apart annually for providing scholarships, exhibitions, and other prizes, premiums, or gratuities, for the pupils, the remainder going to the general support of the college, the payment of professors, &c. Instruction is to be provided by means of classes in mathematics, physics, chemistry, the natural sciences (especially geology and mineralogy, with their application to mines and metallurgy), botany, zoology, physiology, the English, French, and German languages, mechanical drawing, and architecture. In addition to these means of instruction, the trustees may arrange for popular or unsystematic teaching by means of additional lectures or classes upon any subjects comprised in the regular curriculum. While no person is to be admitted to the benefit of the institution who is not for the time being wholly or principally dependent for a livelihood upon his own skill or labour, or upon the support of his parents, or upon some other person or persons, the poorer classes of the community are not to be considered as having any exclusive right to the benefit of the institution. An excellent site for the new college has already been secured in the immediate vicinity of the Birmingham Town-hall, and now that the list of trustees is completed, the college may be expected to assume form very shortly.

THE announcement, a year or two ago, of the purchase by an American of the celebrated Hay collection of Egyptian antiquities, at the time on exhibition at the Crystal Palace in London, created quite a sensation, in view of its intrinsic value and the desire which had been manifested to procure it for the British Museum. In the increasing rarity of objects of this kind, resulting from the great demand on the part of national museums throughout the world, it is believed quite unlikely that such a collection will again be brought together. Its richness in mummies, objects in bronze, marble, alabaster, &c., together with those of smaller size usually found in Egyptian tombs and elsewhere, is very great. While this collection does not embrace many statues or immense sarcophagi, it is believed to be equal to any in the completeness of its series of the smaller objects of religious and domestic Egyptian antiquity, and not inferior to the best collections of Paris, London, Berlin, or Leyden. It was purchased by Mr. Samuel A. Way, of Boston, and removed to that city, and offered to the Museum of Fine Arts, under certain conditions, which the directors did not think best to accept. At the death of Mr. Way, however, it passed into the possession of Mr. Charles Granville Way, himself an artist of merit, who has in turn offered it to the same establishment without condition other than it is to be kept in a room by itself, and to be called the Way Collection. This stipulation, we learn from *Harper's Weekly*, was gladly agreed to, and the collection accepted by the trustees, and its treasures will doubtless before long be opened to the public.

Harper's Weekly states that among some collections of specimens of natural history and ethnology lately presented by Governor W. M. F. Arny, of New Mexico, to the Smithsonian Institution, were some mastodon remains, which were submitted by Prof. Henry to Prof. Leidy for examination. These were found to indicate the existence of a very remarkable species of mastodon (*M. obscurus*), very different from the common *M. americanus*, and previously known only by a few fragments from California and a tooth found many years ago in the Miocene formation of Maryland. One peculiarity of this species consists in the existence of enamel on the tusks of the upper jaw, which does not occur in the more modern *M. americanus*. It also had tusks in the lower jaw, projecting from the prolongation of the jaw, as in the adult of the Miocene *Mastodon angustidens* of Europe, and known only in the young animal of *M. americanus*. The specimen referred to will be figured by Prof. Leidy in his forthcoming report to Dr. Hayden on the vertebrate fossils of the Western Territories.

THE second part of "Mycological Illustrations of New and Rare Fungi," edited by W. W. Saunders, F.R.S., with illustrations by Mr. Worthington Smith, will be ready in a few days. Although more than a twelvemonth has elapsed since the publication of the first part, it is hoped that in future the parts will be issued more regularly at about quarterly intervals.

WE have just received the "Monthly Record of Results of Observations in Meteorology, Terrestrial Magnetism, &c., taken at the Melbourne Observatory during March, 1872; together with Abstracts from Meteorological Observations obtained at various localities in Victoria, under the superintendence of Robert L. J. Ellery, Government Astronomer." Prefixed is a useful table of the averages and extremes of different meteorological elements at Melbourne and other localities for a number of years; and the tables which follow, showing the daily registrations for the month of March, seem sufficiently minute in detail and drawn up with great care.

THE BRITISH ASSOCIATION MEETING AT BRIGHTON

FROM Edinburgh to Brighton is a great leap, and the change is not merely one of clime and latitude. Two towns could hardly be found presenting a greater contrast. We exchange an ancient seat of learning for a modern watering place, the narrow streets and lofty houses of the Old Town for the palatial dwellings of the Steyne, Arthur's Seat for the New Chain Pier, the memory of Scott for that of the Prince Regent. So far the migration has little to recommend it; but then we have the set-off of being within easy reach of London; the British Association has, in fact, never before held its meetings so near the capital, and the present may be looked on as an experimental trial of a metropolitan meeting.

The list of officers of the meeting, and of the sections, has already been announced. The following is the diary of proceedings for each day:—

Wednesday, August 14—General Committee in the Town Hall, at 1 P.M.; Committees of Sections, at 2 P.M.; Inaugural Address by the President, in the Dome, at 8 P.M. Thursday, August 15—Committees of Sections, at 10 A.M.; Sections, at 11 A.M.; *soirée* in the Dome, Corn Exchange and Museum, at 8 P.M. Friday, August 16—Committees of Sections, at 10 A.M.; Sections, at 11 A.M.; discourse in the Dome by Prof. P. Martin Duncan, F.R.S., on the Metamorphoses of Insects, at half-past 8 P.M. Saturday, August 17—Committees of Sections, at 10 A.M.; Sections, at 11 A.M. Excursions: lecture to working men by Wm. Spottiswoode, LL.D., F.R.S., on Sunshine, Sea, and Sky, in the Dome, at 8 P.M. Monday, August 19—Committees of Sections, at 10 A.M.; Sections, at 11 A.M.; General Committee in the Town Hall, at 3 P.M.; discourse in the Dome by Prof. W. K. Clifford, on the Aims and Instruments of Scientific Thought, at half-past 8 P.M. Tuesday, August 20—Committees of Sections, at 10 A.M.; Sections, at 11 A.M.; *soirée* in the Dome, Corn Exchange and Museum, at 8 P.M. Wednesday, August 21—General Committee in the Town Hall, at 1 P.M.; concluding General Meeting in the Dome, at half-past 2 P.M. Thursday, August 22—Excursions.

The reception room will be in the New Museum and Library, Pavilion, and the following will be the rooms for meetings of Sections:—A, Mathematical and Physical Science, Albion Room; B, Chemical Science, Lecture Room, New Museum; C, Geology, Town Hall; D, Biology, Pavilion; E, Geography, Concert Hall, Middle Street; F, Economic Science and Statistics, Old Ship Assembly Rooms; G, Mechanical Science, Friends' Meeting House, Ship Street.

An Exhibition of objects of interest and works of art will be on view, during the meeting, in the Corn Exchange, New Museum and Library, and the following Excursions have been arranged:—Saturday afternoon, August 17—(1) To Glynde Station, for Glynde Place, the Chalk Pits, and Mount Caburn; to Glynde Station, for Firle Place and Beacon; (2) to Lewes, Southover, and Mount Harry, returning to Stanmer Park; (3) to Worthing, thence to Cissbury, for the excavations by Captain Oliver, and then to Findon; (4) to Bramber, then to Steyning and Wiston. Thursday, August 22—(1) To Pevensey, Hastings, and Battle Abbey, thence to the Sub-Wealden Boring, or to Norman Hurst (residence of Mr. Thomas Brassey, M.P.); (2) to Arundel, Amberley, and Parham; (3) to Chichester and Goodwood; (4) to Portsmouth, for steamer through the Solent for Alum Bay, the Needles and Freshwater Bay, Isle of Wight; to Portsmouth, for the Dockyard, Shipping, &c.; (5) to Hayward's Heath, for Paxhill, Wakehurst Place, West Hoathly Rocks, and Whiteman's Green Quarry, Cuckfield.

The Marine Aquarium, of which we shall take an opportunity of giving an account when it is in a more complete state, will of course be one of the chief objects of attraction to all the members of the Association, who will be admitted free, but "only a limited number daily." The great number of Lady Associates already announced is a prominent feature of the present meeting.

The following distinguished foreigners have already announced their intention of being present, viz.:—

Prof. Hébert, President of the Geological Society of France; Prof. van Benéden, of Louvain, and his son, a naturalist of great ability; Prof. Janssen, of Paris; Prof. Panceri, of Naples; Prof. H. A. Nicholson, of Toronto; Prof. Zengler, of Prague; Prof. Hale, of Albany, U.S.; while invitations have been sent to the following, who have been compelled reluctantly to decline the invitation:—

Prof. Hofmeister, Prof. Sir W. G. Logan, of Montreal; Prof. Clebsch, of Göttingen; Prof. Daubrée, of Paris; Prof. Young, of Dartmouth College, U.S.; Prof. Asa Gray, of Cambridge, U.S.; Prof. Gibbs, of Cambridge, U.S.; Principal Dawson, of Montreal; M. Quatrefages, of Paris; Prof. Kirchhoff, of Heidelberg; Prof. Helmholtz, of Berlin; Prof. Shaler, of Harvard College, U.S.

The customary courtesy of the officers of the Association has enabled us to give our readers this week the President's Address, as well as the opening addresses in Sections A, B, C, and D.

Inaugural Address of Dr. William Carpenter, F.R.S., President

THIRTY-SIX years have now elapsed since at the first and (I regret to say) the only meeting of this Association held in Bristol—which Ancient City followed immediately upon our National Universities in giving it a welcome—I enjoyed the privilege which I hold it one of the most valuable functions of these Annual assemblages to bestow; that of coming into personal relation with those distinguished Men whose names are to every cultivator of Science as "household words," and the light of whose brilliant example, and the warmth of whose cordial encouragement are the most precious influences by which his own aspirations can be fostered and directed. Under the Presidency of the Marquis of Lansdowne, with Conybeare and Prichard as Vice-Presidents, with Vernon Harcourt as General Secretary, and John Phillips as Assistant Secretary, were gathered together Whewell and Peacock, James Forbes and Sir W. Rowan Hamilton, Murchison and Sedgwick, Buckland and De la Beche, Henslow and Daubeny, Roget, Richardson, and Edward Forbes, with many others, perhaps not less distinguished, of whom my own recollection is less vivid.

In his honoured old age, Sedgwick still retains, in the Academic home of his life, all his pristine interest in whatever bears on the advance of the Science he has adorned as well as enriched; and Phillips still cultivates with all his old enthusiasm the congenial soil to which he has been transplanted. But the rest—our fathers and elder brother,—"Where are they?" It is for us of the present generation to show that they live in our lives; to carry forward the work which they commenced; and to transmit the influence of their own example to our own successors.

There is one of these great men, whose departure from among us since last we met claims a special notice, and whose life—full as it was of years and honours—we should have all desired to see prolonged for a few months, could its feebleness have been unattended with suffering. For we should all then have sympathised with Murchison, in the delight with which he would have received the intelligence of the safety of the friend in whose scientific labours and personal welfare he felt to the last the keenest interest. That this intelligence, which our own Expedition for the relief of Livingstone would have obtained (we will hope) a few months later, should have been brought to us through the generosity of one, and the enterprising ability—may I not use our peculiarly English word, the "pluck"—of another of our American brethren, cannot but be a matter of national regret to us. But let us bury that regret in the common joy which both Nations feel in the result; and while we give a cordial welcome to Mr. Stanley, let us glory in the prospect now opening, that England and America will co-operate in that noble object which—far more than the discovery of the Sources of the Nile—our great Traveller has set before himself as his true mission, the Extinction of the Slave Trade.

At the last Meeting of this Association I had the pleasure of being able to announce that I had received from the First Lord of the Admiralty a favourable reply to a representation I had ventured to make to him, as to the importance of prosecuting on a more extended scale the course of inquiry into the Physical and Biological conditions of the Deep Sea, on which, with my colleagues Prof. Wyville Thomson and Mr. J. Gwyn Jeffreys, I had been engaged for the three preceding years. That for which I had asked was a Circumnavigating Expedition of at least three years' duration, provided with an adequate Scientific Staff, and with the most complete Equipment that our experience could devise. The Council of the Royal Society having been led by the encouraging tenor of the answer I had received, to make a formal application to this effect, the liberal arrangements of the Government have been carried out under the advice of a Scientific Committee which included Representatives of this Association. H.M. ship *Challenger*, a vessel in every way suitable for the purpose, is now being fitted out at Sheerness; the command of the Expedition is intrusted to Captain Nares, an Officer of whose high qualifications I have myself the fullest assurance; while the Scientific charge of it will be taken by my excellent friend Prof. Wyville Thomson, at whose suggestion it was that these investigations were originally commenced, and whose zeal for the efficient prosecution of them is shown by his relinquishment for a time of the important Academic position he at present fills. It is anticipated that the Expedition will sail in November next; and I feel sure that the good wishes of all of you will go along with it.

The confident anticipation expressed by my predecessor, that for the utilisation of the total Eclipse of the Sun then impending, our Government would "exercise the same wise liberality as heretofore in the interests of Science," has been amply fulfilled. An Eclipse-Expedition to India was organised at the charge of the Home Government, and placed under the direction of Mr. Lockyer; the Indian Government contributed its quota to the work; and a most valuable body of results was obtained, of which, with those of the previous year, a Report is now being prepared under the direction of the Council of the Astronomical Society.

It has been customary with successive occupants of this Chair, distinguished as Leaders in their several divisions of the noble Army of Science, to open the proceedings of the Meetings over which they respectively presided, with a Discourse on some aspect of Nature in Relation to Man. But I am not aware that any one of them has taken up the other side of the inquiry—that which concerns Man as the "Interpreter of Nature;" and I have therefore thought it not inappropriate to lead you to the consideration of the Mental processes, by which are formed those fundamental conceptions of Matter and Force, of Cause and Effect, of Law and Order, which furnish the basis of all scientific reasoning, and constitute the *Philosophia prima* of Bacon. There is a great deal of what I cannot but regard as fallacious and misleading Philosophy—"oppositions of Science falsely so called"—abroad in the world at the present time. And I hope to satisfy you, that those who set up *their own conceptions* of the Orderly Sequence which they discern in the Phenomena of Nature, as fixed and determinate *Laws*, by which those phenomena not only *are* within all Human experience, but always *have been*, and always *must be*, invariably governed, are really guilty of the Intellectual arrogance they condemn in the Systems of the Ancients, and place themselves in diametrical antagonism to those real Philosophers, by whose comprehensive grasp and penetrating insight that Order has been so far disclosed. For what love of the Truth, as it is in Nature, was ever more conspicuous than that which Kepler displayed in his abandonment of each of the ingenious conceptions of the Planetary System which his fertile Imagination had successively devised, so soon as it proved to be inconsistent with the facts disclosed by observation? In that almost admiring description of the way in which his enemy Mars, "whom he had left at home as a despised Captive," had "burst all the chains of the equations, and broke forth from the prisons of the tables," who does not recognise the justice of Schiller's definition of the real Philosopher, as one who always loves Truth better than his System? And when at last he had gained the full assurance of a success so complete that (as he says) he thought he must be dreaming, or that he had been reasoning in a circle, who does not feel the almost sublimity of the self-abnegation, with which, after attaining what was in his own estimation such a glorious reward of his life of toil, disappointment, and self-sacrifice he abstains from claiming the applause of his contemporaries, but leaves his fame to after ages in these noble words:—"The book is written; to be read either now or by posterity, I care not which. It may well wait a century for a reader, as God has waited six thousand years for an observer."

And when a yet greater than Kepler was bringing to its final issue that grandest of all Scientific Conceptions, long pondered over by his almost superhuman intellect—which linked together the Heavens and the Earth, the Planets and the Sun, the Primaries and their Satellites, and included even the vagrant Comets, in the *nexus* of a Universal Attraction—establishing for all time the truth for whose utterance Galileo had been condemned, and giving to Kepler's Laws a significance of which their author had never dreamed—what was the meaning of that agitation which prevented the Philosopher from completing his computation, and compelled him to hand it over to his friend? That it was not the thought of his own greatness, but the glimpse of the grand Universal Order thus revealed to his mental vision, which shook the serene and massive soul of Newton to its foundations, we have the proof in that beautiful comparison in which he likened himself to a Child picking up shells on the shore of the vast Ocean of Truth—a comparison which will be evidence to all time at once of his true Philosophy with his profound Humility.

Though it is with the Intellectual Representation of Nature which we call *Science*, that we are primarily concerned, it will not be without its use to cast a glance in the first instance at the other two principal characters under which Man acts as her Interpreter—those, namely, of the Artist and of the Poet.

The Artist serves as the Interpeter of Nature, not when he works as the mere copyist, delineating that which he sees with his bodily eyes, and which we could see as well as ourselves; but when he endeavours to awaken within us the perception of those beauties and harmonies which his own trained sense has recognised, and thus impart to us the pleasure he has himself derived from their contemplation. As no two Artists agree in the original constitutions and acquired habits of their Minds, all look at Nature with different (mental) eyes; so that to each, *Nature is what he individually sees in her.*

The Poet, again, serves as the Interpreter of Nature, not so much when by skilful word-painting (whether in prose or verse) he calls up before our mental vision the picture of some actual or ideal scene, however beautiful; as when, by rendering into appropriate forms those deeper impressions made by the Nature around him on the Moral and Emotional part of his own Nature, he transfers these impressions to the corresponding part of ours. For it is the attribute of the true Poet to penetrate the secret of those mysterious influences which we all unknowingly experience; and having discovered this to himself, to bring others, by the power he thus wields, into the like sympathetic relation with Nature,—evoking with skilful touch the varied response of the Soul's finest chords, heightening its joys, assuaging its griefs, and elevating its aspirations. Whilst, then, the Artist aims to picture what he *sees* in Nature, it is the object of the Poet to represent what he *feels* in Nature; and to each true Poet, *Nature is what he individually finds in her.*

The Philosopher's interpretation of Nature *seems* less individual than that of the Artist or the Poet, because it is based on facts which any one may verify, and is elaborated by reasoning processes of which all admit the validity. He looks at the Universe as a vast Book lying open before him, of which he has in the first place to learn the characters, then to master the language, and finally to apprehend the ideas which that language conveys. In that Book there are many Chapters, treating of different subjects; and as Life is too short for any one man to grasp the whole, the Scientific interpretation of this Book comes to be the work of many Intellects, differing not merely in the range but also in the character of their powers. But whilst there are "diversities of gifts," there is "the same spirit." While each takes his special direction, the general Method of study is the same for all. And it is a testimony alike to the truth of that Method and to the Unity of Nature, that there is an ever-increasing tendency towards agreement among those who use it aright—temporary differences of interpretation being removed, sometimes by a more complete mastery of her language, sometimes by a better apprehension of her ideas—and lines of pursuit which had seemed entirely distinct or even widely divergent, being found to lead at last to one common goal. And it is this agreement which gives rise to the general belief—in many, to the confident assurance—that the Scientific interpretation of Nature represents her not merely as she *seems*, but as she *really is.*

When, however, we carefully examine the foundation of that assurance, we find reason to distrust its security; for it can be shown to be no less true of the Scientific conception of Nature, than it is of the Artistic or the Poetic, that it is *a representation framed by the Mind itself* out of the materials supplied by the impressions which external objects make upon the Senses; so that to each Man of Science, *Nature is what he individually believes her to be.* And that belief will rest on very different bases, and will have very unequal values, in different departments of Science. Thus, in what are commonly known as the "exact" Sciences, of which Astronomy may be taken as the type, the data afforded by precise methods of observation can be made the basis of reasoning, in every step of which the Mathematician feels the fullest assurance of certainty; and the final deduction is justified either by its conformity to known or ascertainable facts—as when Kepler determined the elliptic orbit of Mars; or by the fulfilment of the predictions it has sanctioned—as in the occurrence of an Eclipse or an Occultation at the precise moment specified many years previously; or, still more emphatically, by the actual discovery of phenomena till then unrecognised—as when the Perturbations of the planets, shown by Newton to be the necessary results of their mutual attraction, were proved by observation to have a real existence; or as when the unknown disturber of Uranus was found in the place assigned to him by the computations of Adams and Le Verrier.

We are accustomed, and I think most rightly, to speak of these achievements as triumphs of the Human Intellect. But the very phase implies that the work is done by Mental Agency; and the coincidence of its results with the facts of observation is far from proving the Intellectual process to have been correct. For we learn from the honest confession of Kepler, that he was led to the discovery of the Elliptic orbit of Mars by a series of happy accidents, which turned his erroneous guesses into the right direction; and to that of the passage of the Radius Vector over *equal areas in equal times*, by the notion of a whirling force emanating from the Sun, which we now regard as an entirely wrong conception of the cause of orbital revolution.* It should always be remembered, moreover, that the Ptolemaic system of Astronomy, with all its cumbrous ideal mechanism of "Centric and Excentric, Cycle and Epicycle, Orb in Orb," did intellectually represent all that the Astronomer, prior to the invention of the Telescope, could see from his actual standpoint, the Earth, with an accuracy which was proved by the fulfilment of his anticipations. And in that last and most memorable prediction which has given an imperishable fame to our two illustrious contemporaries, the inadequacy of the basis afforded by actual observation of the perturbations of Uranus required that it should be supplemented by an assumption of the probable distance of the disturbing Planet beyond, which has been shown by subsequent observation to have been only an approximation to the truth.

Even in this most exact of Sciences, therefore, we cannot proceed a step without translating the actual Phenomena of Nature into Intellectual Representations of those phenomena; and it is because the Newtonian conception is not only the most simple, but is also, up to the extent of our present knowledge, *universal* in its conformity to the facts of observation, that we accept it as the only Scheme of the Universe yet promulgated, which satisfies our Intellectual requirements.

When, under the reign of the Ptolemaic System, any new inequality was discovered in the motion of a Planet, a new wheel had to be added to the ideal Mechanism—as Ptolemy said, "to save appearances." If it should prove, a century hence, that the motion of Neptune himself is disturbed by some other attraction than that exerted by the interior Planets, we should confidently expect that not an *ideal* but a *real* cause for that disturbance will be found in the existence of another Planet beyond. But I trust that I have now made it evident to you, that this confident expectation is not justified by any absolute necessity of Nature, but arises entirely out of *our belief* in her Uniformity; and into the grounds of this and other Primary Beliefs, which serve as the foundation of all Scientific reasoning, we shall presently inquire.

There is another class of cases, in which an equal certainty is generally claimed for conclusions that seem to flow immediately from observed facts, though really evolved by Intellectual processes; the apparent simplicity and directness of those processes either causing them to be entirely overlooked, or veiling the assumptions on which they are based. Thus Mr. Lockyer speaks as confidently of the Sun's Chromosphere of incandescent Hydrogen, and of the local outbursts which cause it to send forth projections tens of thousands of miles high, as if he had been able to capture a flask of this gas, and had generated water by causing it to unite with oxygen. Yet this confidence is entirely based on the assumption that a certain line which is seen in the Spectrum of a hydrogen flame *means* hydrogen also when seen in the spectrum of the Sun's chromosphere; and high as is the probability of that assumption, it cannot be regarded as a demonstrative certainty, since it is by no means inconceivable that the same line *might* be produced by *some other* substance at present unknown. And so when Dr. Huggins deduces from the different relative positions of certain lines in the spectra of different Stars, that these Stars are moving from or towards us in space, his admirable train of reasoning is based on the assumption that these lines have *the same meaning*—that is, that they *represent the same elements*—in every luminary. That assumption, like the preceding, may be regarded as possessing a sufficiently high probability to justify the reasoning based upon it; more especially since, by the other researches of that excellent observer, the same Chemical elements have been detected as vapours in those filmy cloudlets which seem to be stars in an early stage of consolidation. But when Frankland and Lockyer, seeing in the spectrum of the yellow Solar prominences a certain bright line not identifiable with that of any known Terrestrial flame, attribute this to a hypothetical new substance which they propose to call Helium, it is obvious that their assumption rests on a far less secure foundation; until it shall have received that verification, which, in the case of Mr. Crookes's researches on Thallium, was afforded by the actual discovery of the new metal, whose presence had been indicated by him by a line in the Spectrum not attributable to any substance then known.

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* See Drinkwater's "Life of Kepler," in the Library of Useful Knowledge, pp. 26 35.

In a large number of other cases, moreover, our Scientific interpretations are clearly matters of *judgment*; and this is eminently a *personal act*, the value of its results depending in each case upon the qualifications of the *individual* for arriving at a correct decision. The surest of such judgments are those dictated by what we term "Common Sense," as to matters on which there seems no room for difference of opinion, because every sane person comes to the same conclusion, although he may be able to give no other reason for it than that it appears to him "self-evident." Thus while Philosophers have raised a thick cloud of dust in the discussion of the basis of our belief in the existence of the world external to ourselves—of the Non Ego, as distinct from the Ego—and while every Logician claims to have found some flaw in the proof advanced by every other—the Common Sense of Mankind has arrived at a decision that is practically worth all the arguments of all the Philosophers who have fought again and again over this battle-ground. And I think it can be shown that the trustworthiness of this Common Sense decision arises from its dependence, not on any one set of Experiments, but upon *our unconscious co-ordination of the whole aggregate of our Experiences*—not on the conclusiveness of any one train of Reasoning, but on *the convergence of all our lines of thought towards this one centre.*

Now this "Common Sense," disciplined and enlarged by appropriate culture, becomes one of our most valuable instruments of Scientific inquiry; affording in many instances the best, and sometimes the only, basis for a rational conclusion. Let us take as a typical case, in which no special knowledge is required, what we are accustomed to call the "flint implements" of the Abbeville and Amiens gravel-beds. No logical proof can be adduced that the peculiar shapes of these flints were given to them by Human hands; but does any unprejudiced person now doubt it? The evidence of *design*, to which, after an examination of one or two such specimens, we should only be justified in attaching a probable value, derives an irresistible cogency from accumulation. On the other hand, the *im*probability that these flints acquired their peculiar shape by *accident*, becomes to our minds greater and greater as more and more such specimens are found; until at last this hypothesis, although it cannot be directly disproved, is felt to be almost inconceivable, except by minds previously "possessed" by the "dominant idea" of the modern origin of Man. And thus what was in the first instance a matter of discussion, has now become one of those "self-evident" propositions, which claim the unhesitating assent of all whose opinion on the subject is entitled to the least weight.

We proceed upwards, however, from such questions as the Common Sense of Mankind generally is competent to decide, to those in which special knowledge is required to give value to the judgment; and thus the interpretation of Nature by the use of that faculty comes to be more and more *individual*; things being perfectly "self-evident" to men of special culture, which ordinary men, or men whose training has lain in a different direction, do not apprehend as such. Of all departments of Science, Geology seems to me to be the one that most depends on this specially-trained "Common Sense;" which brings as it were into one focus the light afforded by a great variety of studies—Physical and Chemical, Geographical and Biological; and throws it on the pages of that Great Stone Book, on which the past history of our Globe is recorded. And whilst Astronomy is of all Sciences that which may be considered as most nearly representing Nature as she really is, Geology is that which most completely represents her as seen through the medium of the interpreting mind; the meaning of the phenomena that constitute its data being in almost every instance open to question, and the judgments passed upon the same facts being often different according to the qualifications of the several judges. No one who has even a general acquaintance with the history of this department of Science, can fail to see that the Geology of each epoch has been the reflection of the Minds by which its study was then directed; and that its true progress dates from the time when that "Common Sense" method of interpretation came to be *generally* adopted, which consists in seeking the explanation of past changes in the Forces at present in operation, instead of invoking the aid of extraordinary and mysterious agencies, as the older Geologists were wont to do, whenever they wanted—like the Ptolemaic Astronomers—"to save appearances." The whole tendency of the ever-widening range of modern Geological inquiry has been to show how little reliance can be placed upon the so-called "Laws" of Stratigraphical and Palæontological Succession, and how much allowance has to be made for local conditions. So that while the Astronomer is constantly enabled to point to the fulfilment of his predictions as an evidence of the correctness of his method, the Geologist is almost entirely destitute of any such means of verification. For the value of any prediction that he may hazard—as in regard to the existence or non-existence of Coal in any given area—depends not only upon the truth of the general doctrines of Geology in regard to the succession of Stratified Deposits, but still more upon the detailed knowledge which he may have acquired of the distribution of those Deposits in the particular locality. Hence no reasonably-judging man would discredit either the general doctrines or the methods of Geology, because the prediction proves untrue in such a case as that now about to be brought in this neighbourhood to the trial of experience.

We have thus considered Man's function as the Scientific Interpreter of Nature in two departments of Natural Knowledge; one of which affords an example of the strictest, and the other of the freest method, which Man can employ in constructing his Intellectual representation of the Universe. And as it would be found that in the study of all other departments the same methods are used, either separately or in combination, we may pass at once to the other side of our inquiry, namely, the origin of those Primary Beliefs which constitute the groundwork of all Scientific reasoning.

The whole fabric of Geometry rests upon certain Axioms which every one accepts as true, but of which it is necessary that the truth should be *assumed*, because they are incapable of demonstration. So, too, the deliverances of our "Common Sense" derive their trustworthiness from what we consider the "self-evidence" of the propositions affirmed.

This inquiry brings us face to face with one of the great Philosophical problems of our day, which has been discussed by Logicians and Metaphysicians of the very highest ability as Leaders of opposing Schools, with the one result of showing how much can be said on each side. By the *Intuitionalists* it is asserted that the tendency to form these Primary Beliefs is inborn in Man, an original part of his mental organisation; so that they grow up spontaneously in his Mind as its faculties are gradually unfolded and developed, requiring no other Experience for their genesis, than that which suffices to call these faculties into exercise. But by the advocates of the doctrine which regards *Experience* as the basis of all our knowledge, it is maintained that the Primary Beliefs of each individual are nothing else than generalisations which he formed of such experiences as he has either himself acquired or has consciously learned from others; and they deny that there is any original or intuitive tendency to the formation of such beliefs, beyond that which consists in the power of retaining and generalising experiences.

I have not introduced this subject with any idea of placing before you even a summary of the ingenious arguments by which these opposing doctrines have been respectively supported; nor should I have touched on the question at all, if I did not believe that a means of reconcilement between them can be found in the idea that the *Intellectual Intuitions of any one Generation are the embodied Experiences of the previous Race.* For, as it appears to me there has been a progressive improvement in the *Thinking Power* of Man; every product of the culture which has preceded serving to prepare the soil for yet more abundant harvests in the future.

Now as there can be no doubt of the Hereditary transmission in Man of acquired constitutional peculiarities, which manifest themselves alike in tendencies to Bodily and Mental disease, so it seems equally certain that *acquired mental habitudes* often impress themselves on his organisation, with sufficient force and permanence to occasion their transmission to the offspring as *tendencies to similar modes of thought.* And thus, while all admit that *Knowledge* cannot thus descend from one generation to another, an increased *aptitude* for the acquirement, either of knowledge generally, or of some particular kind of it, may be thus inherited. These tendencies and aptitudes will acquire additional strength, expansion, and permanance, in each new generation, from their habitual exercise upon the materials supplied by a continually enlarged experience; and thus the *acquired* habitudes produced by the Intellectual culture of ages, will become "a second nature" to every one who inherits them.*

We have an illustration of this progress in the fact of continual occurrence, that conceptions which prove inadmissable to the minds of one generation, in consequence either of their want of intellectual power to apprehend them, or of their preoccupation by older habits of thought, subsequently find a universal acceptance, and even come to be approved as "self-evident." Thus the First Law of Motion, divined by the genius of Newton, though opposed by many Philosophers of his time as contrary to all experience, is now accepted by common consent, not merely as a legitimate inference from Experiment, but as the expression of a necessary and universal truth; and the same Axiomatic value is extended to the still more general doctrine, that Energy of *any* kind, whether manifested in the "molar" motion of masses, or consisting in the "molecular" motion of atoms, *must* continue under some form or other without abatement or decay; what all admit in regard to the indestructibility of Matter, being accepted as no less true of Force, namely, that as *ex nihilo nil fit*, so *nil fit ad nihilum* †

But, it may be urged, the very conception of these and similar great truths is in itself a typical example of Intuition. The men who divined and enunciated them stand out above their fellows, as possessed of a Genius which could not only combine but create, of an Insight which could clearly discern what Reason could but dimly shadow forth. Granting this freely, I think it may be shown that the Intuitions of individual Genius are but specially exalted forms of endowments which are the general property of the Race at the time, and which have come to be so in virtue of its whole previous culture. Who, for example, could refuse to the marvellous aptitude for perceiving the relations of Numbers, which displayed itself in the untutored boyhood of George Bidder and Zerah Colburn, the title of an Intuitive gift? But who, on the other hand, can believe that a Bidder or a Colburn could suddenly arise in a race of Savages who cannot count five? Or, again, in the history of the very earliest days of Mozart, who cannot fail to recognise the dawn of that glorious Genius, whose brilliant but brief career left its imperishable impress on the Art it enriched? But who would be bold enough to affirm that an infant Mozart could be born amongst a tribe whose only musical instrument is a tom-tom whose only song is a monotonous chant?

Again, by tracing the gradual *genesis* of some of those Ideas which we now accept as "self-evident"—such, for example, as that of the "Uniformity of Nature"—we are able to recognise them as the expressions of certain Intellectual tendencies, which have progressively augmented in force in successive generations, and now manifest themselves as Mental Instincts that penetrate and direct our ordinary course of Thought. Such Instincts constitute a precious heritage, which has been transmitted to us with ever-increasing value through the long succession of preceding generations; and which it is for us to transmit to those who shall come after us, with all that further increase which our higher Culture and wider range of Knowledge can impart.

And now, having studied the working action of the Human Intellect in the Scientific Interpretation of Nature, we shall examine the general character of its products; and the first of these with which we shall deal is our conception of *Matter* and of its relation to *Force.*

The Psychologist of the present day views Matter entirely through the light of his own Consciousness: his idea of Matter in the abstract being that it is a "something" which has a permanent power of exciting Sensations; his idea of any "property" of Matter being the mental representation of some kind of sensory impression he has received from it; and his idea of any particular kind of Matter being the representation of the whole aggregate of the Sense-perceptions which its presence has called up in his Mind. Thus, when I press my hand against this table, I recognise its unyieldingness through the conjoint medium of my sense of Touch, my Muscular sense, and my Mental sense of Effort, to which it will be convenient to give the general designation of the Tactile Sense; and I attribute to that table a *hardness* which resists the effort I make to press my hand into its substance, whilst I also recognise the fact that the force I have employed is not sufficient to move its mass. But I press my hand against a lump of dough; and finding that its substance yields under my pressure I call it *soft.* Or, again, I press my hand against this desk; and I find that although I do not thereby change its *form*, I change its *place*; and so I get the Tactile idea of *Motion.* Again, by the impression received through the same Sensorial apparatus, when I lift this book in my hand, I am led to attach to it the motion of *weight* or ponderosity; and by lifting different solids of about the same size, I am enabled, by the different degrees of exertion I find myself obliged to make in order to sustain them, to distinguish some of them as *light*, and others as *heavy.* Through the medium of another set of Sense-perceptions which some regard as belonging to a different category, we distinguish between bodies that *feel* "hot" and those that *feel* "cold;" and in this manner we arrive at the notion of differences of Temperature. And it is through the medium of our Tactile Sense, without any aid from Vision, that we first gain the idea of *solid form*, or the Three Dimensions of Space.

Again, by the extension of our Tactile experiences, we acquire the notion of *liquids*, as forms of matter yielding readily to pressure, but possessing a sensible weight which may equal that of solids: and of *air*, whose resisting power is much slighter, and whose weight is so small that it can only be made sensible by artificial means. Thus, then, we arrive at the notions of *resistance* and weight as properties common to all forms of Matter; and now that we have got rid of that idea of Light and Heat, Electricity and Magnetism, as "imponderable fluids," which used to vex our souls in our Scientific Childhood, and of which the popular term "Electric Fluid is a "survival," we accept these properties as affording the practical distinction between the "material" and the "immaterial."

Turning, now, to that other great portal of Sensation, the Sight, through which we receive most of the messages sent to us from the Universe around, we recognise the same truth. Thus it is agreed alike by Physicists and Physiologists, that *Colour* does not exist *as such* in the object itself; which has mere the power of reflecting or transmitting a certain numb millions of undulations in a second; and these only that affection of our consciousness which we call Co they fall upon the retina of the living Percipient.

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* I am glad to be able to append the following extract from a letter which Mr John Mill, the great Master of the Experimental School, was good enough to write to me a few months since, with reference to the attempt I had made to place "Common Sense" upon this basis (*Contemporary Review*, Feb. 1872):—"When states of mind in no respect innate or instinctive have been frequently repeated, the mind acquires, as is proved by the power of Habit, a greatly increased facility of passing into those states; and this increased facility must be owing to some change of a physical character in the organic action of the Brain. There is also considerable evidence that such acquired facilities of passing into certain modes of cerebral action can in many cases be transmitted, more or less completely, by inheritance. The limits of this power of transmission, and the conditions on which it depends, are a subject now fairly before the scientific world; and we shall doubtless in time know much more about them than we do now But so far as my imperfect knowledge of the subject qualifies me to have an opinion, I take much the same view of it that you do, at least in principle"

† This is the form in which the doctrine now known as that of the "Conservation of Energy" was enunciated by Dr. Mayer, in the very remarkable Essay published by him in 1845, entitled "Die organische Bewegung in ihrem Zus ammenhange mit dem Stoffwechsel."

be that defect either in the retina or in the apparatus behind it, which we call "colour-blindness" or Daltonism, some particular hues cannot be distinguished, or there may even be no power of distinguishing any colour whatever. If we were all like Dalton, we should see no difference, except in form, between ripe cherries hanging on a tree, and the green leaves around them; if we were all affected with the severest form of colour-blindness, the fair face of Nature would be seen by us as in the chiaroscuro of an Engraving of one of Turner's Landscapes, not as in the glowing hues of the wondrous Picture itself. And in regard to our Visual conceptions it may be stated with perfect certainty, as the result of very numerous observations made upon persons who have acquired sight for the first time, that these do *not* serve for the recognition even of those objects with which the individual had become most familiar through the Touch, until the two sets of sense-perceptions have been co-ordinated by experience.*

When once this co-ordination has been effected, however, the composite perception of Form which we derive from the Visual sense alone is so complete, that we seldom require to fall back upon the Touch for any further information respecting the quality of the object. So, again, while it is from the co-ordination of the two dissimilar pictures formed by any solid or projecting object upon our two retinæ, that (as Sir Charles Wheatstone's admirable investigations have shown) we ordinarily derive through the Sight alone a correct notion of its *solid* form, there is adequate evidence that this notion, also, is a mental *judgment* based on the experience we have acquired in early infancy by the consentaneous exercise of the Visual and Tactile senses.

Take, again, the case of those wonderful instruments by which our Visual range is extended almost into the infinity of Space, or into the infinity of Minuteness. It is the mental not the bodily eye, that takes cognizance of what the Telescope and Microscope reveal to us. For we should have no well-grounded confidence in their revelations as to the *unknown*, if we had not first acquired experience in distinguishing the true from the false by applying them to *known* objects; and every interpretation of what we see through their instrumentality is a *mental judgment* as to the probable form, size, and movement of bodies removed by either their distance or their minuteness from being cognosced by our sense of Touch.

The case is still stronger in regard to that last addition to our Scientific *armamentum*, which promises to be not inferior in value either to the Telescope or the Microscope; for it may be truly said of the Spectroscope, that it has not merely extended the range of our Vision, but has almost given us a new sense, by enabling us to recognise distinctive properties in the Chemical Elements which were previously quite unknown. And who shall now say that we know all that is to be known as to any form of Matter; or that the science of the *fourth* quarter of this century may not furnish us with as great an enlargement of our knowledge of its Properties, and of our power of recognising them, as that of its *third* has done?

But, it may be said, is not this view of the Material Universe open to the imputation that it is "evolved out of the depths of our own consciousness"—a projection of our own Intellect into what surrounds us—an *ideal* rather than a *real* World? If all we know of Matter be an "Intellectual Conception," how are we to distinguish this from such as we form in our Dreams?—for these, as our Laureate no less happily than philosophically expresses it, are "true while they last." Here our "Common Sense" comes to the rescue. We "awake, and behold it was a dream." Every healthy mind is conscious of the difference between his waking and his dreaming experiences; or, if he is now and then puzzled to answer the question, "Did this really happen, or did I dream it?" the perplexity arises from the consciousness that it *might* have happened. And every healthy mind, finding its own experiences of its waking state not only self-consistent, but consistent with the experiences of others, accepts them as the basis of his beliefs, in preference to even the most vivid recollections of his dreams.

The Lunatic Pauper who regards himself as a King, the Asylum in which he is confined as a Palace of regal splendour, and his Keepers as obsequious attendants, is so "possessed" by the conception framed by his disordered intellect, that he *does* project it out of himself into his surroundings; his refusal to admit the corrective teaching of Common Sense being the very essence of his malady. And there are not a few persons abroad in the world, who equally resist the teachings of Educated Common Sense, whenever they run counter to their own preconceptions; and who may be regarded as—in so far—affected with what I once heard Mr. Carlyle pithily characterise as a "diluted Insanity."

It has been asserted over and over again, of late years, by a class of men who claim to be the only true Interpreters of Nature, that we know nothing but Matter and the Laws of Matter, and that Force is a mere fiction of the Imagination. May it not be affirmed, on the other hand, that while our notion of *Matter* is a Conception of the Intellect, *Force* is that of which we have the *most* direct—perhaps even the *only* direct—cognizance? As I have already shown you, the knowledge of Resistance and of Weight which we gain through our Tactile Sense is derived from our own perception of *exertion*; and in Vision, as in Hearing, it is the Force with which the undulations strike the sensitive surface, that affects our consciousness with Sights or Sounds. True it is that in our Visual and Auditory Sensations, we do not, as in our Tactile, *directly* cognosce the Force which produces them; but the Physicist has no difficulty in making sensible to us indirectly the undulations by which Sound is propagated, and in proving to our Intellect that the Force concerned in the transmission of Light is really enormous.*

It seems strange that those who make the loudest appeal to Experience as the basis of all knowledge, should thus disregard the most constant, the most fundamental, the most direct of all experiences; as to which the Common Sense of Mankind affords a guiding light much clearer than any that can be seen through the dust of Philosophical discussion. For, as Sir John Herschel most truly remarked, the universal Consciousness of mankind is as much in accord in regard to the existence of a real and intimate connection between Cause and Effect, as it is in regard to the existence of an Eternal World; and that consciousness arises to every one out of his own sense of *personal* exertion in the origination of changes by his individual agency.

Now while fully accepting the Logical definition of Cause as the "antecedent or concurrence of antecedents on which the Effect is invariably and unconditionally consequent," we can always single out one *dynamical* antecedent—the Power which does the work—from the aggregate of *material conditions* under which that Power may be distributed and applied. No doubt the term Cause is very loosely employed in popular phraseology; often (as Mr. Mill has shown) to designate the occurrence that immediately preceded the effect—as when it is said that the spark which falls into a barrel of gunpowder is the cause of its explosion, or that the slipping of a man's foot off the rung of a ladder is the cause of his fall. But even a very slightly trained Intelligence can distinguish the Power which acts in each case, from the Conditions under which it acts. The Force which produces the explosion is locked up (as it were) in the powder; and ignition merely liberates it, by bringing about new Chemical combinations. The fall of the man from the ladder is due to the Gravity which was equally pulling him down while he rested on it; and the loss of support, either by the slipping of his foot, or by the breaking of the rung, is merely that change in the material conditions which gives the Power a new action.

Many of you have doubtless viewed with admiring interest that truly wonderful work of Human Design, the Walter Printing Machine. You first examine it at rest; presently comes a man who simply pulls a handle towards him; and the whole inert mechanism becomes instinct with life—the blank paper continuously rolling off the cylinder at one end, being delivered at the other, without any intermediate human agency, as large sheets of print, at the rate of 15,000 in an hour. Now what is the *Cause* of this most marvellous effect? Surely it lies essentially in the Power of Force which the pulling of the handle brought to bear on the machine from some extraneous source of Power, which we in this instance know to be a Steam-engine on the other side of the wall. This Force it is, which, distributed through the various parts of the Mechanism, really performs the action of which each is the instrument; *they* only supply the vehicle for its transmission and application. The man comes again, pushes the handle in the opposite direction, detaches the Machine from the Steam-engine, and the whole comes to a stand; and so it remains, like an inanimate corpse, until recalled to activity by the renewal of its Moving Power.

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* Thus, in a recently recorded case in which sight was imparted by opera- on to a young woman who had been blind from birth, but who had never- ss learned to work well with her needle, when the pair of scissors she had ccustomed to use was placed before her, though she described their our, and glistening metallic character, she was utterly unable to re- *as scissors* until she put her finger on them; when she at once ughing at her own stupidity (as she called it) in not having fore.

* See Sir John Herschel's "Familiar Lectures on Scientific Subjects."

But, say the Reasoners who deny that Force is anything else than a fiction of the imagination, the revolving shaft of the Steam-engine is "Matter in Motion;" and when the connection is established between that shaft and the one that drives the Machine, the *Motion* is communicated from the former to the latter, and thence distributed to the several parts of the Mechanism. This account of the operation is just what an observer might give, who had looked on with entire ignorance of every-thing but what his *eyes* could *see;* the moment he puts his *hand* upon any part of the machinery, and tries to stop its motion, he takes as direct cognizance, through his sense of the Effort required to resist it, of the *force* which produces that motion, as he does through his *eye* of the motion itself.

Now since it is universally admitted that our notion of the External World would be not only incomplete, but erroneous, if our Visual perceptions were not supplemented by our Tactile, so, as it seems so me, our interpretation of the Phenomena of the Universe must be very inadequate, if we do not mentally coordinate the idea of Force with that of Motion, and recognise it as the "efficient cause" of those phenomena—the "material conditions" constituting (to use the old Scholastic term) only "their formal cause." And I lay the greater stress on this point, because the Mechanical Philosophy of the present day tends more and more to express itself in terms of *Motion* rather than in terms of *Force*—to become *Kinetics* instead of *Dynamics.*

Thus from whatever side we look at this question—whether the Common Sense of Mankind, the Logical Analysis of the relation between Cause and Effect, or the Study of the working of our own Intellects in the interpretation of Nature—we seem led to the same conclusion; that the notion of Force is one of those elementary Forms of Thought with which we can no more dispense, than we can with the notion of Space or of Succession. And I shall now, in the last place, endeavour to show you that it is the substitution of the Dynamical for the mere Phenomenal idea, which gives their highest value to our conceptions of that Order of Nature, which is worshipped as itself a God by the class of Interpreters whose doctrine I call in question.

The most illustrative as well as the most illustrious example of the difference between the mere Generalisation of Phenomena and the Dynamical conception that applies to them, is furnished by the contrast between the so-called Laws of Planetary Motion discovered by the persevering ingenuity of Kepler, and the interpretation of that Motion given us by the profound insight of Newton. Kepler's three Laws were nothing more than comprehensive statements of certain groups of Phenomena determined by observation. The *first,* that of the revolution of the Planets in Elliptical orbits, was based on the study of the observed places of Mars alone; it might or might not be true of the other Planets; for so far as Kepler knew, there was no reason why the orbits of some of them might not be the excentric circles which he had first supposed that of Mars to be. So Kepler's *second* law of the passage of the Radius Vector over equal areas in equal times, so long as it was simply a generalisation of facts in the case of that one Planet, carried with it no *reason* for its applicability to other cases, except that which it might derive from his erroneous conception of a whirling force. And his *third* law was in a like manner simply an expression of certain Harmonic relation which he had discovered between the times and the distances of the Planets, having no more *rational* value than any other of his numerous hypotheses.

Now the Newtonian "Laws" are often spoken of as if they were merely *higher generalisations* in which Kepler's are included; to me they seem to possess an altogether different character. For starting with the conception of two *Forces,* one of them tending to produce continuous uniform motion in a straight line, the other tending to produce a uniformly accelerated motion towards a fixed point, Newton's wonderful mastery of Geometrical reasoning enabled him to show that, if these *Dynamical* assumptions be granted, Kepler's *phenomenal* "Laws," being necessary consequences of them, must be *universally* true. And while that demonstration would have been alone sufficient to give him an imperishable renown, it was his still greater glory to divine that the fall of the Moon towards the Earth—that is, the deflection of her path from a tangential line to an ellipse—*is a phenomenon of the same order* as the fall of a stone to the ground; and thus to show the applicability to the entire Universe, of those simple Dynamical conceptions which constitute the basis of the Geometry of the Principia.

Thus, then, whilst no "Law" which is simply *a generalisation of Phenomena* can be considered as having any *coercive* action, we may assign that value to Laws which express *the universal conditions of the action of a Force,* the existence of which we learn from the testimony of our own consciousness. The assurance we feel that the Attraction of Gravitation *must* act under all circumstances according to its one simple Law, is of a very different order from that which we have in regard (for example) to the Laws of Chemical Attraction, which are as yet only generalisations of phenomena. And yet even in that strong assurance, we are required by our examination of the basis on which it rests, to admit a *reserve* of the *possibility* of something different; a reserve which we may well believe that Newton himself must have entertained.

A most valuable lesson as to the allowance we ought always to make for the unknown "possibilities of Nature," is taught us by an exceptional phenomenon so familiar that it does not attract the notice it has a right to claim. Next to the Law of the Universal Attraction of Masses of Matter, there is none that has a wider range than that of the *Expansion of Bodies by Heat.* Excluding Water and one or two other substances, the fact of such expansion might be said to be *invariable;* and, as regards bodies whose Gaseous condition is known, the Law of Expansion can be stated in a form no less simple and definite than the Law of Gravitation. Supposing those exceptions, then, to be unknown, the Law would be *universal* in its range But it comes to be discovered that Water, whilst conforming to it in its expansion from 39½° *upwards* to its boiling-point, as also, when it passes into Steam, to the special law of Expansion of Vapours, is exceptional in its *expansion* also from 39½° *downwards* to its Freezing-point; and of this failure in the Universality of the Law, no rationale can be given. Still more strange is it, that by dissolving a little *salt* in water, we should remove this exceptional peculiarity; for *sea*-water continues to contract from 39½° downwards to its Freezing-point 12° or 14° lower, just as it does with reduction of temperature at higher ranges.

Thus from our study of the mode in which we arrive at those conceptions of the Orderly Sequence observable in the Phenomena of Nature which we call "Laws," we are led to the conclusion that they are Human conceptions, subject to Human fallibility; and that they *may* or *may not* express the Ideas of the Great Author of Nature. To set up these Laws as self-acting, and as either excluding or rendering unnecessary the Power which alone can give them effect, appears to me as arrogant as it is unphilosophical. To speak of *any* Law as "regulating" or "governing" phenomena, is only permissible on the assumption that the Law is the expression of the *modus operandi* of a Governing Power. I was once in a great City which for two days was in the hands of a lawless mob. Magisterial authority was suspended by timidity and doubt; the force at its command was paralysed by want of resolute direction. The "Laws" were on the Statute book, but there was no Power to enforce them. And so the Powers of evil did their terrible work; and fire and rapine continued to destroy life and property without check, until new Power came in, when the Reign of Law was restored.

And thus we are led to the culminating point of Man's Intellectual Interpretation of Nature—his recognition of the Unity of the Power, of which her Phenomena are the diversified manifestations. Towards this point all Scientific inquiry now tends. The Convertibility of the Physical Forces, the Correlation of these with the Vital, and the intimacy of that *nexus* between Mental and Bodily activity, which, explain it as we may, cannot be denied, all lead upward towards one and the same conclusion; and the pyramid of which that Philosophical conclusion is the apex, has its foundation in the Primitive Instincts of Humanity.

By our own remote Progenitors, as by the untutored Savage of the present day, every change in which Human agency was not apparent was referred to a particular Animating Intelligence. And thus they attributed not only the movement of the Heavenly bodies, but all the phenomena of Nature, each to its own Deity. These Deities were invested with more than Human power; but they were also supposed capable of Human passions, and subject to Human capriciousness. As the Uniformities of Nature came to be more distinctly recognised, some of these Deities were invested with a dominant control, while others were supposed to be their subordinate ministers. A serene Majesty was attributed to the greater Gods who sit above the clouds; whilst their inferiors might "come down to Earth in the likeness of Men." With the growth of the Scientific Study of Nature, the conception of its Harmony and Unity gained ever-increasing strength. And so among the most enlightened of the Greek and Roman Philosophers, we find a distinct recognition of the idea of the Unity of the Directing Mind from which the Order of Nature proceeds; for they obviously believed that, as our modern Poet has expressed it—

> All are but parts of one stupendous whole,
> Whose body Nature is, and God the Soul.

The Science of Modern times, however, has taken a more special direction. Fixing its attention exclusively on the *Order* of Nature, it has separated itself wholly from Theology, whose function it is to seek after its *Cause*. In this, Science is fully justified, alike by the entire independence of its objects, and by the historical fact that it has been continually hampered and impeded in its search for the Truth as it is in Nature, by the restraints which Theologians have attempted to impose upon its inquiries. But when Science, passing beyond its own limits, assumes to take the place of Theology, and sets up its own conception of the *Order* of Nature as a sufficient account of its *Cause*, it is invading a province of Thought to which it has no claim, and not unreasonably provokes the hostility of those who ought to be its best friends.

For whilst the deep-seated instincts of Humanity, and the profoundest researches of Philosophy, alike point to Mind as the one and only source of Power, it is the high prerogative of Science to demonstrate the *Unity* of the Power which is operating through the limitless extent and variety of the Universe, and to trace its *Continuity* through the vast series of Ages that have been occupied in its Evolution.

SECTION A

MATHEMATICAL AND PHYSICAL SCIENCE

Opening Address by the President, Mr. Warren De La Rue, D.C.L., F.R.S.

My predecessors in this chair have addressed you on many subjects of high interest in Mathematical and Physical Science: I do not contemplate passing in review the recent discoveries in Astronomy and Physical Science, but intend to confine myself, in the main, to Astronomical Photography; and in selecting this branch of science as the subject of this introductory discourse, I think that I shall have your approval, not only because I have given special attention to that subject, but also because it is about to be applied to the determination of a fundamental element of our system, the solar parallax, by observations of the transit of Venus in 1874, and probably also in 1882.

Nothing is so lastingly injurious to the progress of science as false data, for they endure often through many centuries. False views, even if supported by some amount of evidence, do comparatively little harm, for every one takes a salutary interest in proving their falseness, and when this is done the path to error is closed, and the road to truth is opened at the same moment.

It will be conceded that Photography, when applied to scientific observation, undoubtedly preserves facts. But the question has sometimes been raised, Are photographic records absolutely trustworthy representations of the phenomena recorded? If not, what is the extent of truth, and where are the inlets for errors and mistakes? Not only has photographic observation gained a wide range of applications in astronomy, but in every other branch of physical science its help is daily more and more taken advantage of; and although, in speaking of this art, I shall confine myself to astronomy, the observations which I propose to make may be suggestive with reference to other branches of physics.

As an instance of the application of this art to optical physics, I may in this place call attention to the very successful delineation of the solar spectrum by Mr. Lewis M. Rutherfurd, of the United States. In Mr. Rutherfurd's spectrum, obtained by the camera, many portions and lines are shown (in the ultra-violet for instance), which, while imperceptible to the retina of the eye, impress themselves on the sensitive film. As a fact, lines which are single in Angstrom's and Kirchhoff's maps have been recorded by photography as well-marked double lines. I will now review the application of the art to astronomy.

Stellar photography was for some time applied at Harvard College Observatory, U.S., to double stars, for the purpose of determining by micrometric measurement their relative angle of position and distance. The zero of the angle of position was found by moving the telescope in right ascension after an impression had been taken, and taking a second one on the same plate; this process gave two sets of photographic images on the same plate, and the right line passing through the series gave the direction of the daily motion of the heavens. The probable error of a single measurement of the photographic distance of the images was found to be ±0″·12, or somewhat smaller than that of a direct measurement with the common filar micrometer. The late Prof. Bond, who applied photography to stellar astronomy, confining himself to stars brighter than the seventh magnitude, discussed the results in various numbers of the "Astronomische Nachrichten." No astronomer more unbiassed could have been selected to decide on the comparative value of the photographic and direct observational method. His discussion shows that the probable error of the centre of an image was ±0″·051, and that of the distance of two such centres was ±0″·072. Adopting the estimate of Struve, ±0″·217, as the probable error of a single measurement of a double star of this class with a filar micrometer, Prof. Bond shows that the measurement of the photographic images would have a relative value three times as great. He derived the further important conclusion, that deficiency of light can be more than compensated for by proportionate increase in the time of exposure. A star of the ninth magnitude would give a photographic image, after an exposure of ten minutes, with the Cambridge equatorial.

In the reproduction of stars by photography, recently undertaken by Mr. Rutherfurd, the objects to be secured being so minute, special precautions were found to be necessary in depicting them upon the sensitive film, so that their impressions might be distinguishable from accidental specks in the collodion plate. To prevent any such chance of mistake, Mr. Rutherfurd secures a double image of each luminary, the motion of the telescope being stopped for a short time (half a minute) between a first and second exposure of the plate; so that each star is represented by two close specks, so to speak, upon the negative, and is clearly to be distinguished by this contrivance from any accidental speck in the film. A map of the heavens is thus secured, very clear though delicate in its nature, but yet one upon which implicit reliance can be placed for the purposes of measurement. Prof. Peirce aptly says, "This addition to astronomical research is unsurpassed by any step of the kind that has ever been taken. The photographs afford just as good an opportunity for new and original investigation of the relative position of near stars as could be derived from the stars themselves as seen through the most powerful telescopes. They are indisputable facts, unbiassed by personal defects of observation, and which convey to all future times the actual places of the stars when the photographs were taken."

Mr. Asaph Hall, who shared with Prof. Bond the work of measuring the photographic images, and of reducing the measurements, has very recently subjected the photographic method to a critical comparison, with a view to deciding on its value when applied to the observation of the transit of Venus. He appears, as regards its application to stellar observations, to under-estimate the photographic method in consequence of want of rapidity; but he admits that in the case of a solar eclipse, or of the transit of a planet over the sun's disc, it has very great advantages, especially over eye-observations of contacts, inner and outer, of the planet and the sun's limb, and that the errors to which it is subject are worthy of the most thorough investigation. The observation of a contact is uncertain on account of irradiation, and is also only momentary: so that, if missed from any cause, the record of the event is irretrievably lost at a particular station, and long and costly preparations rendered futile. On the other hand, when the sky is clear, a photographic image can be obtained in an instant, and repeated throughout the progress of the transit, and even if all the contacts be lost, equally valuable results will be secured, if the data collected on the photographic plates can be correctly reduced, as will be proved hereafter to be undoubtedly possible. That the transit of Venus will be recorded by photography may now be announced as certain, as preparations are energetically progressing in England, France, Russia, and America for obtaining photographic records. There is also a possibility of Portugal taking part in these observations; for it is contemplated by Señor Capello to transport the Lisbon photoheliograph to Macao. There are at present five photoheliographs in process of construction for the observing parties to be sent out by the British Government, under the direction of the Astronomer Royal, Sir George B. Airy. The Russian Government will supply their own parties with three similar instruments; and I am also having constructed one of my own for this purpose and for future solar observations. All these instruments, made precisely alike, will embody the results of our experience gained during the last ten years in photoheliography at the Kew Observatory whilst belonging to this Association. One only of them, namely, the photoheliograph which has been at work for some years at Wilna, is of somewhat older pattern; but how great an advance even this instrument is on the original at Kew is proved by the delightful definition of the most delicate markings of the sun in the pictures which have reached this country from Wilna.

Hitherto sun-pictures have been taken on wet collodion; but a question has been raised whether it would not be better to use dry plates. On this point M. Struve informs us that, in two places—at Wilna, under the direction of Colonel Smysloff, and at Bothkamp, in Holstein, under Dr. Vogel—they have perfectly succeeded in taking instantaneous photographs of the sun with dry plates.

As far, however, as my own experience has gone, I still believe that the wet collodion is preferable to the dry for such observations.

Now, with reference to contact observations—which, it must be remembered, are by no means indispensable as far as photography is concerned—it may be conceded that there will attach to the record of the internal contact a certain amount of uncertainty, although not so great as that which affects optical observation. The photography which first shows contact may possibly not be that taken when the thread of light between Venus and the sun's disc is first completed, but the first taken after it has become thick enough to be shown on the plate; and this thickness is somewhat dependent on incidental circumstances—for example, a haziness of the sky, which, although almost imperceptible, yet diminishes the actinic brilliancy of the sun, and might render the photographic image of the small extent of the limb which is concerned in the phenomenon too faint for future measurements. On the other hand, having a series of photographs of the sun with Venus on the disc, we can, with a suitable micrometer—such as I contrived for measuring the eclipse pictures of 1860, and which, since then, has been in continuous use in measuring the Kew solar photograms [*]—fix the position of the centre of each body with great precision. But the reduction of the measured distances of the centre to their values in arc is not without difficulty. Irradiation may possibly enlarge the diameter of the sun in photographic pictures, and it may diminish the size of the disc of a planet crossing the sun, as is the case with eye-observations; but if the images depicted are nearly of the same size at all stations whose results are to be included in any set of discussions, then the ratio of the diameters of Venus and the sun will be the same in all the plates, and it will be safe to assume that they are equally affected by irradiation. The advantage which, therefore, will result by employing no less than eight instruments precisely alike, as are those now being made by Mr. Dallmeyer on the improved Kew model, is quite obvious. If other forms of instruments, such as will hereafter be alluded to, be used, it will be essential that a sufficient number of them be employed in selected localities to give also connected sets for discussion.

To give some idea of the relative apparent magnitudes of the sun and Venus, I may mention that at the epoch of the transit of 1874 the solar disc would, in the Kew photoheliograph, have a semi-diameter of 1965·8 thousandths of an inch, or nearly two inches; Venus a semi-diameter of 63·33 of these units; and the parallax of Venus referred to the sun would be represented by 47·85 such units, the maximum possible displacement being 95·7 units, or nearly $\frac{1}{10}$th of an inch.

When the photographs have been secured, the micrometric measurements which will have to be performed consist in the determination of the sun's semi-diameter in units of the scale of the micrometer, the angle of position of the successive situations of the planet on the disc, as shown on the series of photographs, and finally the distances of the centres of the planet and the sun. These data determine absolutely the chord along which the transit has been observed to within $0''\cdot1$; and an error of $1''$ in the measurement would give an error of only $0''\cdot185$ in the deduced solar parallax. Moreover the epoch of each photographic record is determinable with the utmost accuracy, the time of the exposure being from $\frac{1}{50}$ to $\frac{1}{100}$ of a second, or even less.

Now, although the truth of the foregoing remarks will be fully admitted, it will yet be well to point out in this place the inherent or supposed defects of the photographic method. These defects may principally be comprised under the head of Possibility of Distortion; and the importance of an investigation into this source of error will appear at once obvious in all cases where the position of a definite point with reference to a system of co-ordinates has to be determined from measured photographs, especially in such a refined application of it as that which it will have in the determination of the solar parallax.

The distortion of a photographic image, if such exist, may be either extrinsic or intrinsic—that is, either optical or mechanical. The instrumental apparatus for producing the image may produce optical irregularities before it reaches the sensitive plate; or an image optically correct may, by irregular contraction of the sensitive film in the process of drying, and other incidents of the process, present on the plate a faulty delineation. [*]

In general, two ways present themselves for clearing observations from errors. Either methods may be devised for determining the numerical amount of every error from any source, or by special contrivances the source of error may be contracted to such insignificant limits that its effect in a special case is too minute to exert any influence upon the result. Both these roads have been followed in the inquiry into the optical distortion of photographic images.

As regards the first, let it be supposed that, as in the Kew instrument, the primary image is magnified by a system of lenses before reaching the sensitive plate. The defects inherent to the optical arrangement will clearly affect every photographic picture produced by the same instrument; and hence a method suggests itself for determining absolutely the numerical effect of distortion at every point of the field. Let us assume that the same object, which may be a rod of unalterable and known length, be photographed in precisely the same manner in which celestial events are photographically recorded, the object being at a considerable distance; it may successively be brought into all possible positions in the field of the photoheliograph, and the length of the image on the photograph may be measured afterwards at leisure by means of a micrometer. These lengths will change relatively wherever distortion takes place; but by laying down these varying lengths we shall obtain an optical distortion-map of the particular instrument; and tables may be constructed giving in absolute numbers the corrections to be applied to measurements of positions on account of the influence of optical distortion. In this way the optical distortion of the combined object-glass and secondary magnifier is ascertained. The chief source of distortion, if such exist, will be in the secondary magnifier; and in order to ascertain its amount, a reticule of lines drawn at equal distances upon glass may (as has been done recently by Paschen and Dallmeyer) be placed in the common focus of the object-glass and secondary magnifier. The required data are then immediately given by the measurement of the resulting pictures of the parallelograms on the reticule. Mr. Dallmeyer has ascertained in this manner that no sensible distortion exists in the secondary magnifier constructed by him. The truth of the principle being granted, it was applied to a preliminary series for finding the distortion which affects the Kew instrument, which is not nearly so perfect as those more recently constructed; and the results were so far satisfactory that, instead of a single rod, a proper scale, fifteen feet in length, representing a series of rectangles distributed over half the radius of the field has been erected; and the process of absolutely determining the optical distortion of the Kew photoheliograph is now in active progress, and will be used for the new instruments to be employed in observing the transits of Venus.

The second method of dealing with optical distortion aims at total exclusion of this source of error. It has been proposed by American astronomers, who intend taking part in the coming observations of the transit of Venus, to exclude the secondary magnifier, and, in order to obtain an image of sufficient diameter, to employ a lens of considerable focal length, say 40 ft., which would give an image as large as with the Kew photoheliograph—

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[*] In this micrometer, which is capable of giving radial distances, angles of position, and also rectangular co-ordinates, the accuracy of linear measurements does not depend on the doubtful results given by a long run of a micrometer screw

[*] It has been proposed, in order to obviate any possible alteration of the sensitive surface, to use the Daguerreotype instead of the collodion process. The former, however, is so little practised, and, moreover, is so much more troublesome, that it does not seem to be advisable to adopt it, especially as the subsequent measurements would present greater difficulties than occur with collodion pictures.

namely, 4 in. in diameter. As it would be inconvenient to mount such an instrument equatorially, it is proposed to fix it in the meridian in a horizontal position, and reflect the sun in the direction of its axis by means of a flat mirror moved by a heliostat. There cannot be any doubt about the fact that the image so produced would be nearly free from optical distortion, if the interposed mirror did not introduce a new source of error. The difficulty of producing a plane mirror is well known; and there is a difficulty in maintaining its true figure in all positions; there is also a liability of the disturbance of the rays by currents of heated air between the mirror and object-glass: moreover, with such an instrument, position-wires could not be defined with sharpness on the photographs. On the whole, greater reliance may be placed on a method which admits the existence of a distorting influence, but has at the same time means of checking and controlling it numerically.

Great attention has been paid by me at various times to those effects of distortion which might arise from the process of drying. The results to which the experiments lead seem to prove that there is no appreciable contraction except in thickness, and that the collodion film does not become distorted, provided the rims of the glass plates have been well ground: this point is a fundamental one. But in such observations as that of the transit of Venus, no refinement of correction ought to be neglected; hence fresh experiments will be undertaken to set at rest the question whether distortion of the film really takes place when proper precautions are taken. This will be done both by the method I have employed before, and also in accordance with M. Paschen's proposal to measure images of such reticules as above described: this reticule might, as he has suggested, be photographed during the transit of Venus, so that each plate would thus bear data for the correction due to unequal shrinking, if such were to take place.

It has been objected by some astronomers who have casually examined solar photograms that the limb of the sun appears as a consequence of the gradual shading off, even under a small magnifying power, not bounded by a sharp contour; but the measurements of such photograms which have been made during the last ten years of pictures, taken under the most varying conditions which influence definition, have proved that even the worst picture leads to a very satisfactory determination of the sun's semi-diameter and centre; moreover, an independent examination of this question by M. Paschen gave as the result that the mean error of a determination is only ± 0·008 millimetre with a sun-picture of 4 Paris inches in diameter; this corresponds to ± 0″·135, and it is nearly three times less than that resulting from a measurement with the Koningsberg heliometer.

Nevertheless it will be seen from the foregoing remarks that I have not hesitated to arouse your attention to the fact that Astronomical Photography is about to be put to the severest test possible in dealing with such a fundamental problem of astronomy as the determination of the sun's distance from the earth. An intimate knowledge of the subject, however, and experience with respect to work already accomplished in the Kew ten-year solar observations, inspire me with a confident anticipation that it will prove fully equal to the occasion.

So much for performances to be looked forward to in the future: now let me briefly review what Astronomical Photography has already undoubtedly accomplished.

In the first instance the possibility proved of giving to the photographic method of observation a trustworthiness which direct observations can never quite obtain, will render the results of our discussion of the ten years' solar observations at Kew more free from doubts than those observational series on the Sun's spots which have preceded ours. The evidence of a probable connection between planetary positions and solar activity, and the evidence which we have published on the nature of spots as depressions of solar matter, could never have been brought forward but for the preservation of true records of the phenomena through a number of years, while the closer agreement of the calculated results in reference to solar elements is itself evidence of the intrinsic truthfulness of the method, and gives the highest promise that our final deductions, which will be completed in the course of the ensuing year, will not be unworthy the exertions which I, in conjunction with my friends B. Stewart and B. Loewy, have constantly devoted to this work during a period of fully ten years. Not only will some doubtful questions be set finally at rest by it, but new facts of the greatest interest will result, bearing on the laws which appear to govern solar activity.

By nothing, however, would the claims of photographic observation, as one of the most important instruments of scientific research, seem to be so thoroughly well established as by the history of recent solar eclipses. It will be recollected that in 1860 for the first time the solar origin of the prominences was placed beyond doubt solely by photography, which preserved a faithful record of the moon's motion in relation to these protuberances. The photographs of Tennant at Guntour, and of Vogel at Aden, 1868, and also those of the American astronomers at Burlington and Ottumwa, Iowa, in 1869, under Profs. Morton and Mayer, have fully confirmed those results. In a similar manner the great problem of the solar origin of that portion of the corona which extends more than a million of miles beyond the body of the sun has been, by the photographic observations of Col. Tennant and Lord Lindsay in 1871, set finally at rest, after having been the subject of a great amount of discussion for some years.

The spectroscopic discovery in 1869 of the now famous green line, 1474 K, demonstrated undoubtedly the self-luminosity, and hence the solar origin of part of the corona. Those who denied the possibility of any extensive atmosphere above the chromosphere received the observation with great suspicion; but in 1870 and again in 1871 it was fully verified. So far, therefore, the testimony of spectroscopic observations was in favour of the solar origin of the inner corona.

Indeed the observations of 1871 have proved hydrogen to be also an essential constitutent of the "coronal atmosphere," as Janssen proposes to call it—hydrogen at a lower temperature and density, of course, than in the chromosphere. Janssen was further so fortunate as to catch glimpses of some of the dark lines of the solar spectrum in the coronal light, an observation which goes far to show that in the upper atmosphere of the sun there are also solid or liquid particles, like smoke or cloud, which reflect the sunlight from below. Many problems, however, even with reference to the admittedly solar part of the corona, are unsettled. The first relates to the nature of the substance which produces the line 1474 K. Since it coincides with a line in the spectrum of iron, it is by many considered due to that metal; but then we must suppose either that iron vapour is less dense than hydrogen gas, or that it is subject to some peculiar solar repulsion which maintains it at its elevation, or other hypotheses may be suggested for explaining the fact. Since the line is one of the least conspicuous in the spectrum of iron and the shortest, and as none of the others are found associated with it in the coronal spectrum, it seems natural, as many have done, to assume at once that it is due to some new kind of matter. But the observations of Angström, Roscoe, and Clifton, and recently those of Schuster regarding the spectrum of nitrogen, render it probable that elementary bodies have only one spectrum, and since in all experimental spectra we necessarily operate only on a small thickness of a substance, we cannot say what new lines may be given out in cases where there is an immense thickness of vapour; and hence we cannot conclude with certainty that because there is an unknown line in the chromosphere or corona, it implies a new substance. Another problem, the most perplexing of all, is the reconciliation of the strangely discordant observations upon the polarisation of the coronal light; but I will at once proceed to the points on which photography alone can give us decisive information.

The nature and conditions of the outer corona (the assemblage of dark rifts and bright rays which overlies and surrounds the inner corona) was very incompletely studied; and the question whether it is solar was not finally settled in the opinions of astronomers of high repute. Some believed it to be caused by some action of our atmosphere; and others supposed it due to cosmical dust between us and the moon. The bright light of the corona and the prominences most undoubtedly cause a certain amount of atmospheric glare; and although it is difficult to see how this is to account for the rays and rifts, it would be rash to deny that it may do so in some manner yet to be discovered. It is quite certain that some of the phenomena observed just at the beginning and end of totality are really caused by it. A light haze of meteoric dust between us and the moon might give results much resembling those observed; but when we come to details this theory seems to be doubtful.

Here photography steps in to pave the way out of the existing doubts. If the rays and rifts were really atmospheric, it would hardly be possible that they should present the same appearance at different stations along the line of totality; indeed, they would probably change their appearance every moment, even at the same station. If they are cislunar, the same appearances could not be recorded at distant stations. It is universally admitted that proof of the invariability of these markings, and especially of their identity as seen at widely separated stations, would amount to a demonstration of their extra-terrestrial origin. Eye-sketches cannot be depended on; the drawings made by persons standing side by side differ often to an extent that is most perplexing. Now photographs have, undoubtedty, as yet failed to catch many of the faint markings and delicate details; but their testimony, as far as it goes, is unimpeachable. In 1870, Lord Lindsay at Santa Maria, Prof. Winlock at Jerez, Mr. Brothers at Syracuse, obtained pictures some of which, on account partly of the unsatisfactory state of the weather, could not compare with Mr. Brothers' picture obtained with an instrument of special construction;* but all show one deep rift especially, which seemed to cut down through both the outer and inner corona clear to the limb of the moon. Even to the naked eye it was one of the most conspicuous features of the eclipse Many other points of detail also come out identical in the Spanish and Sicilian pictures; but whatever doubts may have still existed in regard of the inner corona were finally dispelled by the pictures taken in India, in 1871, by Col. Tennant and Lord Lindsay's photographic assistant, Mr. Davis.

None of the photographs of 1871 show so great an extension of the corona as is seen in Mr. Brothers' photograph, taken at Syracuse in 1870; but, on the other hand, the coronal features are perfectly defined on the several pictures, and the number of the photographs renders the value of the series singularly great. The agreement between the views, as well those taken at different times during totality as those taken at different stations, fully proves the solar theory of the inner corona. We have in all the views the same extensive corona, with persistent rifts similarly situated. Moreover there is additional evidence indicated by the motion of the moon across the solar atmospheric appendages, proving in a similar manner as in 1860, in reference to the protuberances, the solar origin of that part of the corona.

It will be well here to mention a difficulty which occurs in recording the fainter solar appendages, namely the encroachment of the prominences and the corona on the lunar disc when the plates have to be over-exposed in order to bring out the faint details of the corona. It is satisfactory to find that whenever a difficulty arises it can be mastered by proper attention. Lord Lindsay and Mr. Ranyard have successfully devoted themselves to experiments on the subject. They tested whether reflections from the back surface of the plate played any part in the production of the fringes: for this purpose plates of ebonite and the so-called nonactinic yellow glass were prepared; and it was immediately found that the outer haze had completely disappeared in the photographs taken on ebonite, while on the yellow glass plates it is much fainter than on ordinary white glass plates. By placing a piece of wetted black paper at the back of an unground plate, the outer haze was greatly reduced; but by grinding both the back and the front surfaces of a yellow glass plate, and covering the back with a coating of black varnish, it was rendered quite imperceptible, thus showing the greatest part of the so-called photographic irradiation to be due to reflection from the second surface.

In connection with the solution of the most prominent questions connected with the solar envelopes, it may not be without great interest to allude to another point conclusively decided during the last annular eclipse of the sun, observed by Mr. Pogson on the 6th of June of this year, as described by him in a letter to Sir G. B. Airy. In 1870 Prof. Young was the first to observe the reversal of the Frauenhofer lines in the stratum closest to the sun. Now, in 1871 doubts were thrown upon the subject. It appears that the reversed lines seem to have been satisfactorily observed by Captain Maclear at Bekul, Colonel Tennant at Dodabetta, and Captain Fyers at Jaffna. The observations of Pringle at Bekul, Respighi at Poodocottah, and Pogson at Avenashi were doubtful, while Mosely at Trincomalee saw nothing of this reversal, which is, according to all accounts, a most striking phenomenon, although of very short duration. Mr. Lockyer missed it by an accidental derangement of the telescope. The reversal and the physical deductions from it are placed beyond doubt by Mr. Pogson's observation of the annular eclipse on June 6. At the first internal contact, just after a peep in the finder had shown the moon's limb lighted up by the corona, he saw all the dark lines reversed and bright, but for less than two seconds. The sight of beauty above all was, however, the reversion of the lines at the breaking-up of the limb. The duration was astonishing—five to seven seconds; and the fading-out was gradual, not momentary. This does not accord with Captain Maclear's observations in 1870, who reports the disappearance of the bright spectrum as "not instantly, but so rapidly that I could not make out the order of their going." Prof. Young again says that "they flashed out like the stars from a rocket-head." But discrepancies in this minor point may be accounted for by supposing differences in quietude of that portion of the sun's limb last covered by the moon.

The mention of the solar appendages recalls to mind another instance in which photography has befriended the scientific investigator. I allude to the promising attempt which has been made by Prof. Young to photograph the protuberances of the sun in ordinary daylight. A distinct reproduction of some of the double-headed prominences on the sun's limb was obtained; and although as a picture the impression may be of little value, still there is every reason to believe, now that the possibility of the operation is known, that with better and more suitable apparatus an exceedingly valuable and reliable record may be secured. Prof. Young employed for the purpose a spectroscope containing seven prisms, fitted to a telescope of $6\frac{1}{2}$-inch aperture, after the eye-piece of the same had been removed. A camera, with the sensitive plate, was attached to the end of the spectroscope, the eye-piece of which acted in the capacity of a photographic lens, and projected the image on the collodion film. The exposure was necessarily a long one, amounting to three minutes and a half. The eye-piece of the spectroscope was unsuitable for photographic purposes, and only in the centre yielded a true reproduction of the lines free from any distortion. A larger telescope, with a suitable secondary magnifier, will be required, in order to secure a more defined image.

I have hitherto spoken of the successful applications of photography to astronomy; but I must point out also some cases where it has failed. Nebulæ and comets have not yet been brought within the grasp of this art, although, perhaps, no branch of astronomy would gain more if we should hereafter succeed in extending to these bodies that mode of observing them. There is theoretically, and even practically, no real limit to the sensitiveness of a plate. Similarly with reference to planets great difficulties still exist, which must be overcome before their phases and physical features can be recorded to some purpose by photography; yet there is great hope that the difficulties may be ultimately surmounted. The main obstacle to success arises from atmospheric currents, which are continually altering the position of the image on the sensitive plate; the structure of the sensitive film is also an interfering cause for such small objects. A photograph taken at Cranford of the occultation of Saturn by the moon some time ago exhibits the ring of the planet in a manner which holds out some promise for the future.

The moon, on the other hand, has been for some time past very successfully photographed; but no use has hitherto been made of lunar photographs for the purposes of measurement.

The photographs of the moon are free from distortion, and offer therefore material of incalculable value as the basis of a selenographic map of absolute trustworthiness, and also for the solution of the great problem of the moon's physical libration. This question can be solved with certainty by a series of systematic measurements of the distance of definite lunar points from the limb. Mr. Ellery, Director of the Observatory of Melbourne, has sent over an enlargement of a lunar photograph taken with the Great Melbourne Telescope, in which the primary image is $3\frac{2}{10}$ inches in diameter. Such lunar negatives would be admirably adapted for working out the problem of the physical libration, and also for fundamental measurements for a selenographic map; the more minute details, however, would have to be supplied by eye-observations, as the best photograph fails to depict all that the eye sees with the help of optical appliances. On the other hand, selenographic positions would be afforded more free from error than those to be obtained by direct micrometrical measurements.

Although, as I have stated, I do not contemplate passing in review recent discoveries in astronomy, I must not omit to call your attention to some few subjects of engrossing interest. First, with reference to the more recent work of Dr. Huggins. In his observations he found that the brightest line of the three bright lines which constitute the spectrum of the gaseous nebulæ was coincident with the brightest of the lines of the spectrum of nitrogen; but the aperture of his telescope did not permit him to ascertain whether the line in the nebulæ was double, as is the case with the line of Nitrogen. With the large telescope placed in his hands by the Royal Society, he has found that the line in the nebulæ is not double, and in the case of the great nebula in Orion it coincides in position with the less refrangible of the two lines which make up the corresponding nitrogen-line. He has not yet been able to find a condition of luminous nitrogen in which the line of this gas is single and narrow and defined like the nebular line.

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* Mr. Brothers had, in 1870, the happy idea to employ a so-called rapid rectilinear photographic lens, made by Dallmeyer, of 4 inches aperture and 30 inches focal length, mounted equatorially, and driven by clockwork, and he was followed in this matter by both Col Tennant and Lord Lindsay in 1871. The focal image produced, however, is far too small ($\frac{3}{10}$ of an inch, about), therefore it will be desirable in future to prepare lenses of similar construction, but of longer focal length and corresponding aperture.

He has extended the method of detecting a star's motion in the line of sight by a change of refrangibility in the line of a terrestrial substance existing on the star to about 30 stars besides Sirius. The comparisons have been made with lines of hydrogen, magnesium, and sodium. In consequence of the extreme difficulty of the investigation, the numerical velocities of the stars have been obtained by estimation, and are to be regarded as provisional only. It will be observed that, speaking generally, the stars which the spectroscope shows to be moving from the earth, as Sirius, Betelgeux, Rigel, Procyon, are situated in a part of the heavens opposite to Hercules, towards which the sun is advancing; whilst the stars in the neighbourhood of this region, as Arcturus, Vega, and α Cygni, show a motion of approach. There are, however, in the stars already observed, exceptions to this general statement; and there are some other considerations, as the relative velocities of the stars, which appear to show that the sun's motion in space is not the only or even in all cases the chief cause of the observed proper motions of the stars. In the observed stellar motions we have to do probably with two other independent motions—namely, a movement common to certain groups of stars and also a motion peculiar to each star. Thus the stars, $\beta\ \gamma\ \delta\ \epsilon\ \zeta$, of the Great Bear, which have similar proper motions, have a common motion of recession; while the star α of the same constellation, which has a proper motion in the opposite direction, is shown by the spectroscope to be approaching the earth. From further researches in this direction, and from an investigation of the motions of stars in the line of sight in conjunction with their proper motions at right angles to the visual direction obtained by the ordinary methods, we may hope to gain some definite knowledge of the constitution of the heavens.

This discovery supports, in a somewhat striking manner, the views which Mr. Proctor has been urging respecting the distribution of the stars in space. According to these views there exist within the sidereal system subordinate systems of stars forming distinct aggregations, in which many orders of real magnitude exist, while around them is relatively barren space. He had inferred the existence of such systems from the results of processes of equal-surface charting applied successively to stars of gradually diminishing orders of brightness. He found the same regions of aggregation, whether the charts included stars to the sixth order only or were extended, as in his chart of the northern heavens, to the tenth and eleventh orders; and these regions of aggregation are the very regions where the elder Herschel found the faintest telescopic stars to congregate. Applying a new system of charting to show the proper motions of the stars, he found further evidence in favour of these views. The charts indicated the existence of concurrent motions among the members of several groups or sets of stars. Selecting one of the more striking instances as affording what appeared to him a crucial test of the reality of this *star-drift*, Mr. Proctor announced his belief that whenever the spectroscopic method of determining stellar motions of recess or approach should be applied to the five stars, $\beta\ \gamma\ \delta\ \epsilon$ and ζ Ursæ Majoris, these orbs (which formed a drifting set in the chart of proper motions) would be found to be drifting collectively either towards or from the earth: this has been confirmed.

The time has now come for more closely investigating the various theories which have been propounded by such profound thinkers as Tyndall, Tait, Reynolds, and others, to account for the phenomena of Comets. I do not propose to enter into a statement of these theories; but I venture to call attention to Zollner's views, which have recently given rise to a great amount of controversy. In doing so, I am solely influenced by a desire to give information on this subject, without implying thereby that I give my adherence, or even preference, to his theory.*

The vaporisation of even solid bodies at low temperatures suggests that a mass of matter in space will ultimately surround itself with its own vapour, the tension of which will depend upon the mass of the body (that is, upon its gravitative energy) and the temperature. If the mass of the body is so small that its attractive force is insufficient to give to the enveloping vapour its maximum tension for the existing temperature, the evolution of vapour will be continuous until the whole mass is converted into it. It is proved by analysis that such mass of gas or vapour in empty and unlimited space is in condition of unstable equilibrium, and must become dissipated by continual expansion and consequent decrease of density. It follows that celestial spaces, at least within the limits of the stellar universe, must be filled with matter in the form of gas.

A fluid mass existing in space at a distance from the sun or other body radiating heat would, if its mass were not too great, be converted entirely into vapour after the lapse of sufficient time. But if the fluid mass approach the sun, solar heat would occasion a more rapid development of vapour on the sunward side; and the total vaporisation would require an incomparably short time with reference to the interval necessary in the former case; this time would be shorter the smaller the mass of the body. Prof. Zollner points to the smaller comets, which often appear as spherical masses of vapour, as examples of such bodies, while the spectra of some of the nebulæ and smaller comets render the existence of fluid masses giving out vapour highly probable.

The self-luminosity and train of comets he refers to other causes. Two causes only are known through the operation of which gases become self-luminous—elevation of temperature (as by combustion), or electrical excitement. Setting aside the first as involving theoretical difficulties, the second cause is demonstrated by him to be sufficient to account for the self-luminosity and the formation of the train, provided it be granted that electricity may be developed by the action of solar heat, if not in the process of evaporation, at least in the mechanical and molecular disturbances resulting from it. The production of electricity by such processes within the limits of our experience, must be admitted as a well-known fact. The spectrum of the vaporous envelope of a comet, illuminated in this manner, must necessarily be that produced by the passage of an electrical discharge through vapour identical in substance with a portion of the comet's nucleus, from which the envelope is derived As, according to this supposition, water and liquid hydrocarbons are important constituents of these bodies, the spectra of the comets should be such as belong to the vapours of these substances; and in this manner the resemblance and partial coincidence of the observed cometic spectra with those of gaseous hydrocarbons is explained.

The form and direction of the train indicate undoubtedly the action of a repulsive force; and Prof. Zollner asserts that the assumption of an electrical action of the sun upon bodies of the solar system is necessary and sufficient to account for all the essential and characteristic phenomena of the vaporous envelope and the train. The direction of the train, towards or from the sun, is, according to this theory, to be easily explained by the supposition of a variability in the mutual electrical conditions. This accords perfectly with the phenomena observed in the development of electricity by vapour-streams in the hydroelectric machine, where the sign of electricity depends upon the presence or absence of various substances in the boiler or the tubes.

The theory acquires an additional interest from Schiaparelli's remarkable discovery of the identity of the paths of certain comets with great meteor-streams, since the meteoric masses must inevitably be converted into vapour on approaching the sun, with the exhibition of the characteristic appearances of the comets.

The intimate connection of planetary configuration and solar spots, of the latter and terrestrial magnetism and auroral phenomena, must tend to establish also a connection between solar spots and solar radiation. It is demonstrated by the researches of Piazzi Smyth, Stone, and Cleveland Abbe, that there is a connection between the amount of heat received from the sun and the prevalence of spots—a result clearly in harmony with those derived from recent investigations into the nature of the solar atmosphere. Further, in a paper by Mr. Meldrum, of Mauritius, which will be read before you during this session, most remarkable evidence is given on the close connection of these phenomena. It appears that the cyclones of the Indian Ocean have a periodicity corresponding with the sun-spot periodicity; so that if an observer in another planet could see and measure the sun-spots and cyclones (earth-spots), he would find a close harmony between them. Such a connection will probably be found to exist over the globe generally; but with reference to the Indian Ocean it may be stated as a matter of fact, from Mr. Meldrum's discussion of twenty-five years' observations, that in the area lying between the equator and 25° south latitude, and between 40° and 110° east longitude, the frequency of cyclones has varied during that period directly as the amounts of sun-spots. I am glad to be able announce that Mr. Meldrum, in order to place the deductions on a still broader foundation, proposes to investigate these laws on a plan perfectly in agreement with our method of determining the areas of solar disturbances, the results of which have been published from time to time during the last ten years. Moreover the observations on the periodic changes of Jupiter's appearance, and the observations of Mr. Baxendell that the convection currents of our earth vary according to the sunspot period—all these results, seemingly solitary, but truly in mysterious harmony, point to the absolute necessity for establishing constant photographic records of solar and terrestrial phenomena all over the world. No astronomer or physicist should lose any opportunity of assisting in this great aim, by which alone unbiassed truthful records of phenomena can be preserved. What is more, no system of observations can be carried on at a less expense.

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* See Appendix, p. 12.

We have hopes of seeing the photographic method as applied to sun-observations joined to the work of the Greenwich Observatory; but what is further wanted is the erection of instruments for photographic records and of spectroscopes in a number of observatories throughout the world, so as to obtain daily records of the sun and to observe magnetical and meteorological phenomena continuously in connection with solar activity. Meteorological observation is storing up useful facts; but they can only be dealt with effectually if investigated in close parallelism with other cosmical phenomena. Only when this is done may we hope to penetrate the maze of local meteorological phenomena and elevate meteorology to the rank of a science. The time has really come not only for relieving private observers from the systematic observation of solar phenomena, but for drawing close ties between all scattered scientific observations, so as to let one grand scheme embrace the whole; and no method seems to be so well adapted to bring about this great achievement than the method of photographing the phenomena of nature, which in its very principle carries with it all extinction of individual bias.

In conclusion I cannot refrain from making a passing allusion to a Royal Commision, presided over by the Duke of Devonshire, which has been sitting for some time past; for I believe that its labours will have an important bearing on all that relates to scientific education and the promotion of science in this country. The time has come when the cultivation of science must be protected and fostered by the State; it can no longer be safely left to individual efforts. If England is to continue to hold a high position among civilised nations, the most anxious care must be given to the establishment by the State of such an organised system for the advancement of science and the utilisation of the work of scientific men as will be in harmony with similar organisations in neighbouring states — for examples, France, Germany, and Russia.

APPENDIX

Certain conclusions at which Prof. Zollner arrives in the investigation of several points bearing on the theory which he defends are, quite independent of the latter, of high scientific value.

First, with reference to the density of atmospheric air, which (in accordance with the considerations mentioned in stating his views) he supposes to fill the interstellar space everywhere, he assumes for the purposes of calculation that the temperature of space is that of melting ice, and finds that the lower limit of density for a portion of gas in space is $\frac{1}{10^{246}}$ of that of the air at the earth's surface, a value so small that if a mass of air which, at its ordinary density upon the earth's surface, occupies a volume of one cubic decimetre (a litre), were reduced to the density expressed by this fraction, it would fill a sphere whose radius would not be traversed by a ray of light in less than 10^{98} years. These values indicate a density which would have no appreciable effect whatever upon rays of light or upon the motion of bodies in space, and which would become still less if the temperature of space be taken, with Fourier, at −60° C., or with Pouillet, at −132° C. But as every solid body must, by virtue of its gravitative energy, condense the gas into an atmospheric envelope round itself, the density of the latter will solely depend on the size and mass of the body. Prof. Zollner finds by calculation that, for instance, the density of air thus forming an atmosphere round the moon must be $\frac{1}{10^{332}}$ of that of the air of the earth's surface. This is in accord with the fact that no trace of a lunar atmosphere has as yet been detected. But the values become very great for the larger planets, quite great enough to manifest absorptive effects upon the light reflected from them. Considering that there are peculiarities in the spectra of Uranus, Neptune, and also of Jupiter, which appear to indicate atmospheric influences, Prof. Zollner's results are not without deep interest, and certainly suggestive of further inquiry.

Secondly, with reference to the supposition that a body may be at the same time under the influence of gravitative and electrical agencies, it was necessary for the author of this theory to discuss the quantitive difference in their effect upon ponderable masses at a distance. The discussion shows that, if the mass increases, gravitation preponderates over electricity; if the mass decreases sufficiently, the contrary takes place. It follows that the cometary nuclei, as masses, are subject to gravitation, while the attenuated vapours developed from them yield to the action of free electricity of the sun. Prof. Zollner has based upon Hankel's numerous and careful researches on the determination of atmospheric electricity, in absolute measure, an analytical inquiry into the motion of a small sphere under the action of gravity and atmospheric electricity, which leads to some remarkable results. Supposing the free electricity of the sun to be not greater than that repeatedly observed on the earth's surface, and to be uniformly distributed, it would communicate to a sphere having a diameter of 11 millimetres and a weight of $\frac{1}{100}$ of a milligramme, and starting from the sun, by the time it had moved as far away as the mean distance of Mercury, a velocity per second of 3,027,000 metres, or 408·4 German geographical miles.* This velocity is such that in two days it would pass over a space of 70,540,000 German geographical miles, a magnitude quite of the same order as those recorded by cometary astronomy. The discussion was undertaken to prove that there is no need for assuming the existence of any unknown repulsive agency, but that electrical energy not greater than that observed on the earth's surface is amply sufficient to account satisfactorily for the phenomena presented by cometic trains.

SECTION B

CHEMICAL SCIENCE

OPENING ADDRESS BY THE PRESIDENT, DR. J. HALL GLADSTONE, F.R.S.

ONE of my fellow-students in the laboratory of the late Prof. Graham began the study of Chemistry because he wanted to be a Geologist, and he had read in some Geological Catechism that, in order to be versed in that science, it was necessary, as a preliminary step, to gain a knowledge of Chemistry, Mineralogy, Zoology, Botany, and I know not what besides. My friend became a chemist, and found that enough for the exercise of his faculties. Yet the catechism had truth on its side; for so intertwined are the various branches of observational or experimental research, that a perfect understanding of one can only be obtained through an acquaintance with the whole cycle of knowledge.

Yet, on the other hand, who can survey the whole field even of modern Chemistry? There was a time, doubtless, in the recollection of the more venerable of my auditors, when it was not impossible to learn all that chemists had to teach; but now that our "Hand Book" has grown so large that it would take a Briareus to carry it—and it requires a small army of abstractors to give the Chemical Society the substance of what is done abroad—we are compelled to become specialists in spite of ourselves. He who studies the general laws of Chemistry may well turn in despair from the ever-growing myriads of transformations among the compounds of carbon; we have agricultural, physiological, and technical chemists; one man builds up new substances, another new formulæ; while some love the rarer metals, and others find their whole soul engrossed by the phenyl compounds.

How is this necessity of specialisation to be reconciled with the necessity of general knowledge? By our forming a home for ourselves in some particular region, and becoming intimately conversant with every feature of the locality and their choicest associations, while at the same time we learn the general map of the country, so as to know the relative position and importance of our favourite resort, and to be able—when we desire it—to make excursions elsewhere.

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* Fifteen to a degree of longitude on the Equator.

To facilitate this is one of the great objects of the British Association. The different Sections are like different countries, and leaving the insular seclusion of our special studies, we can pass from one to the other, and gain the advantages of foreign travel.

From this chair I must of course regard Chemistry as the centre of the universe, and in speaking of other sections I must think of them only in their relation to ourselves. There is that rich and ancient country, Section A, which, according to the Annual Report, comprises several provinces—Mathematics, Astronomy, Optics, Heat, Electricity, and Meteorology.

Mathematics and Astronomy.—It was when the idea of exact weights and measures was projected into it that Alchemy was transmuted into Chemistry: as our science has become more refined in its methods its numerical laws have become more and more significant, and it may safely be predicted that the more closely it is allied with general physics, the greater will be the mathematical knowledge demanded of its votary. But till lately the Chemist and the Astronomer seemed far asunder as the heavens and the earth, and none could have foretold that we should now be analysing the atmospheres of the sun and stars, or throwing light on the chemical composition of planetary nebulæ and the heads of comets. There is in this, too, as in other things, a reciprocal benefit; for we are encouraged to hope that this celestial chemistry will reveal to us elements which have not yet been detected among the constituents of our globe.

Light, Heat, and Electricity.—How intimately are these associated with the chemical force, or rather how easily are these Protean forces transformed into one another! The rays of the sun coming upon our earth are like a chemist entering his laboratory; they start strange decompositions and combinations not only in the vegetable kingdom, but also among inorganic gases and salts; they are absorbed selectively by different bodies which they penetrate, or are refracted, dispersed, and polarised according to the chemical composition and structure of the substance. All this has been the subject recently of much scientific research; and I need scarcely remind you of the beautiful art of photography as one of the results of photo-chemistry, or of the benefits that have arisen from a study of circular polarisation, indices of refraction, and especially spectrum-analysis. In regard to the latter, however, I would remark that while the optical examination of the rays emitted by luminous vapours has yielded most brilliant results, there is another kind of spectrum-analysis—that of the rays absorbed by various terrestrial gases, liquids, and solids—which has already borne valuable fruit, and which, as it is far more extensively applicable than the other, may perhaps play a still more important part in the Chemistry of the future. The dispersion of the rays of the spectrum is certainly due to the chemical nature of the body through which they pass, but this is as yet almost unbroken ground waiting for an explorer. As to heat, it has ever been the tool of the chemist; and it would be difficult to over-estimate the significance of researches into the specific heat, or the melting- and boiling-points of elements and their compounds. The laws of chemical combination have been elucidated lately by thermo-chemical researches; it has been sought to establish a connection between the absorption or radiation of heat and the complexity of the chemical constitution of the active body; while the power of conducting heat, or of expanding under its influence, offers a promising field of inquiry. As to electrical science, one department of it—Galvanism—is strictly chemical: the electrolytic cell does our work; and indeed we claim half the electric telegraph, for while the needle may oscillate in Section A, the battery belongs to B.

Last in Section A comes Meteorology; and there are chemical questions concerning the constitution of the atmosphere, its changes, and the effect of its occasional constituents upon vegetable and animal life, which merit the deepest attention of the physiologist, philanthropist, and statesman.

If we turn to Section C, there is an outlying province belonging to us, namely, Mineralogy, which lies on the frontiers of Geology. A vast and very promising region is the origin and mode of formation of different minerals: this has attracted some explorers during the past year; but in order to investigate it properly the geologist and the chemist must travel hand in hand. Geology, in demanding of us the analysis of earths and ores, rocks and precious stones, repays us by bringing to our knowledge many a rare element and strange combination.

When we pass from C to D, that is, from the crust of the globe to the organised beings that inhabit and adorn it, we are introduced into new regions of research. When organic chemistry was young, Cuvier said of it, "Dans cette nouvelle magie, le chimistre n'a presque qu'à vouloir: tout peut se changer en tout peut l'extraire de tout"; and though we have now learnt much of the laws by which these magical transformations proceed, they far transcend the dreams of the French philosopher; there is yet no visible limit to the multitude of products to be derived from the vegetable and animal world, and their changes seem to afford boundless scope for chemical ingenuity. The benefit here is also reciprocal; for the physiologist enters by our aid into the wonderful laboratory of the living plant or animal, and learns to estimate the mode of action of different foods and medicines. There have lately been some good researches of this character; the difficulties are great, but the results to be achieved are worthy of any effort.

There may be little intercourse between us and the geographers in E, but we stand in no distant relationship with many of the subjects discussed in F. Economic science embraces the chemical arts from cookery upwards; such imperial questions as that of the national standards, or the patent laws, interest us greatly; the yield of our corn-fields is increased through our knowledge of the constituents of soils and manures, and upon many of the chemical manufactures depend in no small degree the commerce and the wealth of Britain.

In this most important branch of technical chemistry we need the skill of the mechanician; and this introduces us to Section G. One of the questions of the day will illustrate the connection between these varied departments of study. Statistics prove that the consumption of coal is now advancing, not at the gradual pace which recent calculations allowed, but at a rapidly accelerating speed, and they make the householder anxious about rising prices, and the political economist about the duration of our coal-fields. It is well known that there is a great waste of fuel throughout the country, as the maximum of heat produced by the combustion is very far from being ever utilised; and it will be for the combined wisdom of the chemist, physicist, and mechanician to devise means for reducing this lavish expenditure, or to indicate other available sources of power.

While this correlation of the natural sciences renders it desirable that the votary of one should have some general acquaintance with the rest, the correlation of all knowledge shows that no education can be complete which ignores the study of nature. A mind fed only on one particular kind of lore, however excellent that kind may be, must fail of proper nourishment. I am not going to say a word against philological studies; I am too fond of them myself for that; and I could wish that the modern languages were taught more, and the classic languages were taught better than they are at present. What I do contend for is, that chemistry (or some cognate branch of science) should have an honoured place in the education of every English lady and gentleman. I say purposely "an honoured place," for at present where chemistry is introduced we too often find the idea latent which was expressed by one principal of a lady's college, who told a friend of mine that he was to give the girls a course of pretty experiments, but she did not expect him to teach them anything; and we know that when boys repeat chemical experiments at home it is looked upon as an amusement, a philosophical one no doubt, but rather objectionable, inasmuch as they spoil their mother's towels and singe their own eyebrows.

Of course some knowledge of chemistry is indispensable for a large number of our manufacturers, and for the medical profession, while it is extremely valuable to the farmer, the miner, and the engineer. It will also be readily granted that information about the air we breathe, the water we drink, the food we live upon, the fuel we burn, and the various common objects we handle, must be of service to every man. But we are met by the advocates of the old system of education with the remark that the value of school-teaching does not depend so much upon the information given as upon the mental training. This I admit; though it seems to me that if the same training can be secured by two studies, the one of which (like the making of Latin verses) gives no information at all, and the other (like chemical analysis) imparts some useful knowledge, we should prefer the latter. But I hold that, as a means of educating the mental faculties, chemistry, faithfully taught, has in many respects the advantage over literary studies. There is superabundant scope for the exercise of the memory; the powers of observation are developed by it to a wonderful degree; the reasoning powers may be well disciplined on the philosophy of chemical change, or the application of the laws of Dalton, Mitscherlich, and Avogadro; while the imagination may be cultivated by the attempt to form a conception of the ultimate particles of matter, with their affinities and atomicities, as they act and react upon one another under the control of the physical forces.

Our Government insists on a certain standard of education for all who are allowed to teach in our elementary schools. In those schools which receive no State aid it is only public opinion which can insist that the teacher shall be duly qualified himself. Such bodies as the British Association form this public opinion, and will deserve well of their country if they demand that these masters and mistresses shall know something of the material universe in which they move, and be able to impart to every child such scientific knowledge as shall afford him an interesting subject for thought, give him useful information, and discipline his mental powers.

Among the many services rendered by the monthly reports of the progress of chemistry which the Chemical Society publishes, and the British Association helps to pay for, there is one which is rather salutary than pleasant. They bring prominently before our notice the fact that in the race of original research we are being distanced by foreign chemists I refer not to the quality of our work, about which opinions will probably differ, but to the quantity, which can be determined by very simple arithmetic. This is a matter of no small importance, not only for the honour of England, but still more for the advancement of science, and the welfare of man. From the Physical Chair of this Association last year, a note of warning was uttered in the following words, after a reference to the sad fate of Newton's successors, who allowed mathematical science almost to die out of the country:—"If the successors of Davy and Faraday pause to ponder even on *their* achievements, we shall soon be again in the same state of ignominious inferiority." The President of the Chemical Society, also in the last Anniversary Address, drew attention to the diminished activity of Chemical discovery, and to the lamentable fewness of original papers communicated. He traces this chiefly to "the non-recognition of experimental research by our universities," and suggests that in granting of science-degrees every candidate should be required, as in Germany, to prove his ability for original investigation.

Concurring in this, I would remark that other causes have also been assigned, and other suggestions have been made. There is the small recognition of original research even by our learned societies—at least such recognition as will come home to the understanding of the general public. It is true the Fellowship of the Royal Society is awarded mainly for original discoveries, and there are two or three medals to be disposed of annually; but these distinctions fall to the lot of the seniors in science, often men who are beyond the need of encouragement, and though they doubtless are serviceable as incentives, there is many a beginner in the honourable contest of discovery who is too modest even to hope for the blue ribbon of science. While the Victoria Cross is awarded to few, every soldier who has borne part in a victory expects his clasp, and so might every man who has won victories over the secrets of nature fairly look for some public recognition. It has been suggested, for instance, that the Royal Society, in addition to the F.R.S., might institute an Associateship, with the letters A.R.S., designed exclusively for those younger men who have shown zeal and ability in original research, but whose discoveries have not been sufficient to entitle them already to the Fellowship. It is suggested, too, that the Chemical Society might give some medal, or diploma, or some similar distinction, to those who contribute papers of sufficient merit.

But beyond this is the non-recognition of scientific research by society in general. We can scarcely expect the average enlightened Englishman to be anything but scared by a graphic formula, or a doubly sesquipedalian word containing two or three compound radicals; but he need not continue to talk of the four elements, or of acids being neutralised by sugar. But, indeed, the so-called educated classes in England are not only supremely ignorant of science, they have scarcely yet arrived at the first stage of improvement—the knowledge of their own ignorance. Then, again, there is the excessive preference of practical over theoretical discoveries; or rather, perhaps, the inability to appreciate anything but tangible results. Thus a new aniline compound is nothing unless it will dye a pretty colour; if we speak of the discovery of a new metal by the spectroscope, they simply ask—What is it useful for? and the rigorous determination of an atomic weight has for them no meaning nor interest nor beauty. The general appreciation of science must be of gradual growth; yet there are wealthy men who know its value, and who might well become the endowers of research. There are, indeed, at present funds available for the purpose—such as the Government grant, and the surplus funds of this association; but the money is given simply to cover actual outlay, and this, though very useful, scarcely meets the case of those young philosophers who have no balance at their bankers, and yet must live. Will not some of these wealthy men endow experimental scholarships, or professorships, in connection with our colleges, institutions, or learned societies? As an instance of the good that may be effected in this way, may be cited the Fullerian Professorships; and as a very recent example, worthy of all honour, may be mentioned the purpose of Mr. J. B. Lawes, not only to continue his elaborate experiments at Rothamsted throughout his lifetime, but to place his laboratory and experimental fields in trust, together with 100,000*l.*, so that investigations may be continued in the wider and more scientific questions which the progress of agriculture may suggest.

The Government of our country, through the Science and Art Department, renders good assistance to the teaching of science; and if the recommendations of the Royal Commission on Scientific Instruction and the Advancement of Science be adopted, the introduction of practical examinations for the obtaining of certificates for a superior grade of science master will certainly foster a spirit of research. It has been generally held that the promotion of research is within the legitimate scope of Government; and where, as in the case of Aristotle and Alexander, genius and industry have been sustained by princely munificence, the happiest results have ensued. Yet this question of Government aid is a delicate one: for genius, when put into swaddling clothes, is apt to be stifled by them; and were science to depend on political favour or imperial support, it would be a fatal calamity. Still I think it will be everywhere admitted that science might with propriety be subsidised from the public funds in cases where the results may be expected to confer a direct benefit upon the community, and where the inquiry, either from its expense, its tediousness, its uninteresting character, or the amount of co operation required, is not likely to be carried out by voluntary effort. The astronomical work which is paid for by Government bears upon navigation, and answers both these requirements; and it is easy to conceive of inquiries in our own science that might equally deserve the assistance of the State. Some of these might also more than repay the outlay, though perhaps the profit would not fall into next year's budget.

I believe that this diminution of original research, which we deplore, is partly due to a cause in which we rejoice—the recent extension of science-teaching. The professorships of chemistry are scarcely more numerous now than they were twenty years ago, while the calls upon the professor's time in conducting classes or looking over examination papers have greatly augmented. Thus some of the most capable men have been drawn away from the investigation of nature; and in order to afford them sufficient leisure for the purpose, means must be found to multiply the number of the professorships in our various colleges.

While the rudiments of science are being infused into our primary education, now happily becoming national, while physical science is gradually gaining a footing in our secondary and our large public schools, and while it is winning for itself an honoured place at our universities, it is to be hoped that many new investigators will arise, and that British chemists will not fall behind in the upward march of discovery, but will continue hand in hand with their continental brethren thus to serve their own and future generations.

SECTION C

GEOLOGICAL SECTION

OPENING ADDRESS BY THE PRESIDENT, R. A. C. GODWIN-AUSTEN, F.R.S.

THE Geological Section is fortunate in respect of this year's place of meeting of the British Association. The county of Sussex presents a wide range to the geological observer; there is the great fresh water Wealden series, next the entire Cretaceous group, then portions of the Nummulitic group, including the unique fossiliferous beds of Bracklesham. At Selsey is to be seen a remnant of a definite Tertiary period, of which at no other place in England is there any record. Lastly, the evidence as to local conditions during the Glacial period is peculiarly interesting. This rich field has not wanted for competent labourers, foremost amongst whom must be named Dr. Gideon Mantell, who in his day did so much by his zeal and knowledge to diffuse a taste for his favourite pursuit. There must also be added the names of Mr. Martin, of Pulborough, and Mr. Dixon, of Bognor.

It might perhaps be a fitting preliminary to the local communications which we may expect in the course of this meeting, should I here give a summary of what has been already done with reference to the geology of this S.E. of England; but to many who meet now in this section, very much of such a survey would be familiar. Instead of this I propose to call attention to what is the peculiar feature of our local geology, namely, its great Wealden formation, the product of that vast lake or sound, which at a time before a particle of the chalk hills of Sussex had been formed, covered an area larger than the whole of the south-east of this island. What I shall endeavour to put before you, a point not generally understood, is with reference to the place of formations akin to our Wealden, in the records of past time, and enable you to realise what were then the geographical conditions of the northern hemisphere, what the distribution and extent of other areas of fresh water, the equivalents of our Wealden.

Place of the Fresh- and Brackish-water formations on the Geological Scale

If a general view be taken of the successive physiographical conditions of bygone geological periods, it is seen in respect of each, such as those of the Palæozoic period, or of the Mesozoic such as for the Jurassic, Cretaceous, and Nummulitic, which all represent distinct periods of past time, and are all the products of purely marine conditions, that what is at present terrestrial surface, was at those times to a great extent covered by water, and that the great geological formations are merely old sea-beds.

When on a projection of the northern hemisphere, the known extent of each of these old seas is represented, as on the accompanying maps, it is also seen to how great an extent at those times the area of water exceeded what it has at present; at each of these great periods the northern hemisphere must have presented just such a preponderance of water which the southern hemisphere does at present; and it is further to be remarked how closely the area of one period of northern geological submergence corresponds with the others, as the Nummulitic with the Cretaceous, and the Cretaceous with the Jurassic. Whatever the course, there is to be seen in this a recurrence of like conditions at enormously long intervals of time.

If next the internal evidence to be derived from these Mesozoic formations be taken, it is to be seen, as is familiar to most geologists, that each, when most complete, presents a like order of change, from its older to its newer portions.

Over the mid-European area shallow-water accumulations, such as shingle and sand-zones (infra-Liassic), preceded the deeper water Lias-shales and limestones. Jurassic oolites follow upon these, indicating somewhat decreased depths for the Middle Jurassic series. Oscillations of surface mark this period; and with respect to its physiography, Mr. Darwin has given his opinion that the Malay Archipelago, with its numerous large islands separated by wide and shallow seas, probably represents the former state of Europe, when the Middle Jurassic beds were accumulating. Next follow deep-water depositions, when the widely spread Kimmeridge series was formed, ending upwards with the Portland beds.

The Cretaceous group, as it is exhibited here in the South of England, where its vertical thickness is very great, presents in its lowerbeds (Neocomian) a marine fauna which indicated to Edward Forbes a limited sea, with depths not exceeding eighteen fathoms. Sand-zones hundreds of feet in thickness overlie these. The argillaceous Gault, in its composition and fauna, is a deep-water deposit, followed by shallower-water sands (Upper Green Sand) indicating oscillating conditions as to depth of water. To which succeeds the widely-spread oceanic depositions of the white chalk. Here recurring conditions come about in like order as in the Jurassic series; and a corresponding illustration might be derived from the physical changes indicated in the course of the Nummulitic period.

In respect to none of these marine geological formations is there any indication whatever that one passed into, or was in continuous sequence with, another, either stratigraphically or geologically; on the contrary, wherever there is apparent continuity, either upwards or downwards, it is by change or transition from one set of conditions to another wholly different. The purely Marine Upper Silurian beds of the Welsh border are followed conformably by the Old Red Sandstone, which last is now universally accepted as a lacustrine formation, the place of which, in time, was intermediate between the middle Palæozoic group, and the Upper or Carboniferous, which commenced with the so-called "Devonian." The positions and extent of the "Old Red" lacustrine beds in all parts of the British Islands indicate, even at this day, to what extent Silurian sea-bed had become terrestrial surface, to which the lacustrine basins were subordinate.

In the contrary direction, and in our own area, the next group indicating widely spread marine conditions, that represented by the Devonian and Mountain limestone formations, sets in, as in North Devon, with shallow-water sands, and a marine fauna (Lower Devonian) in sequence in "Old Red" depositions, with fresh-water fishes and crustaceans. There is no continuity from "Old Red" into the earliest Devonian beds, any more than from uppermost Silurian into Lower "Old Red" (Phillips's Geology of Oxford, pp. 77—79).

The later Palæozoic ocean-floor, now one mountain limestone, in turn become terrestrial surface on which the Coal-measures were accumulated, and over which the abundant vegetation of that period established itself. The Coal-measures represent so much of the surface of their time, as from position favoured expanses of fresh and brackish waters, and of alterations from one set of conditions to the other.

Geologists are familiar with the amount of physical change which took place over the European area after the coal-growth period. The subsequent condition of surface which resulted is still distinctly traceable. The Perm-Trias period presents true Aralo-Caspian conditions, physically defined, subordinate to the same Continental area.

The marine Jurassic series next in sequence was succeeded by that period of terrestrial conditions to the more detailed physiography of which I here propose to call your attention. It may suffice on this occasion to state that at the end of the great Cretaceous period, the area of those seas, in our hemisphere down to depths at which the great chalk-floor had been deposited, became part of a continental land, on which the fresh-water formations of the times which preceded the marine Nummulitic were accumulated.

These evidences of successive physical conditions over the northern hemisphere indicate an order of recurrence of corresponding conditions, and, as already noticed, of a progress of change which, in the course of each period, came about in a corresponding order. Great periods, during which wide marine conditions prevailed, alternated with others of wide terrestrial surfaces. The marine periods, as we measure them by the products of the agents which seas and oceans call into action, must have been of vast duration. In like manner we may feel assured that the great fresh-water formations are not, as some geologists have supposed them, mere subordinate parts of the great marine groups, as our "Wealden" of the "Cretaceous," but rather true intermediate groups, of equal geological value with them in the estimate of past time.

The Wealden Formation

Mr. Martin proposed this designation for the assemblage of fresh-water depositions exhibited in the counties of Kent, Surrey, and Sussex, and which may be described generally as consisting of thick accumulations of sands and sandstones, for a lower or earlier part, surmounted by a great argillaceous deposit (Weald-clay). Mr. Webster suggested the propriety of uniting the Purbeck beds Hastings sands, and Weald clay into one group, the whole being mainly a consecutive fresh-water series. It must be understood, however, that there is not a definite line separating the Hastings sands from the Weald clay; all that is signified is that sands predominate for the lower, and clays for the upper portion of the Wealden depositions; but just as thick bands of clay occur in the lower series, so bands of sandstone occur in the upper.

The arrangement adopted by the Geological Survey, in descending order, is Weald clay, Tunbridge Wells sand, Wadhurst clay, Ashdown sands, Ashburnham beds, which in Sussex are the equivalent of the Purbeck beds of Dorsetshire.

The Lower Sands are well seen on the coast at Hastings, whence they took their name, and extend thence continuously to near Horsham, rising into the central ridge of the Wealden elevations of St. Leonard's, Tilgate, and Ashdown Forests On every side this tract is bounded by the Weald Clay, which extends to the base of the escarpment of the Lower Green Sand, beneath which it passes.

This surface of fresh-water strata, so defined, extends for seventy miles from E. to W., and has a breadth from N. to S. of thirty-five miles. Over the whole of this area the fresh-water depositions attain a great thickness; the lower sandy group may be taken at 820 feet, and the Weald Clay at 450 feet at least.

To realise the conditions under which these accumulations were formed, the now upraised central Sandstone ranges must be put back to their original horizontal position, and the whole series must be regarded as the infilling by fresh-water rivers of what was an area of depression, with reference to the terrestrial surface of the time. This Wealden formation can be traced far beyond the limits of the denudation of the S.E. counties. In a southerly direction it occurs in the Isle of Wight, with its two divisions of Weald clay and Lower sands. In this quarter the Weald clay is reduced to a thickness of 68 feet. In a westerly direction (Swanage Bay) the Wealden sands have a great thickness, and are surmounted by only a thinnish band of Weald clay or deep-water deposit, and both divisions decrease rapidly, in the extension of the formation across the Isle of Purbeck, and have not been recognised in the Isle of Portland, from which, if they even extended there, they must be denuded off.

In a northerly direction, several sections about Oxford, as from Shotover Hill to Great Hazeley, from Wheatley to Tetsworth, from Brill through Long Crenden to Thame, from Whitchurch to Aylesbury, extending from S.W. to N.E. for a breadth of thirty miles, show Purbeck beds, and fresh-water ferruginous sands passing beneath Cretaceous beds. It is obvious that the Wealden formation has been cut back in this quarter, and that originally it had a much greater extension. In this quarter, too, the ferruginous sands overlap the Purbeck beds, showing that the lake had here widened its area beyond the dimensions of the Purbeck lake.

From Oxford* to the Vale of Wardour is an interval of seventy miles, from over which the Portland Oolite has been removed, except at Swindon, at which place there are beds which are unmistakeably referable to the Purbeck group; and it is a fair inference that it is to this denudation that the absence of the lacustrine depositions is to be attributed, which everywhere on our area, and on much of that of Continental Europe which was adjacent, follow next upon the Portland stage. Such being the case, the smallest possible dimensions which can be assigned to the great Wealden lake, are that it extended from beyond Aylesbury to Portland for 120 miles, and from Portland to the Boulonnais for 200 miles.

From Rye to Portland the Wealden beds pass out of sight beneath the level of the English Channel. The valley of the Channel is the result of the disturbance which produced the E. and W. lines of the South of England, and was produced subsequently to the Nummulitic period.

Dr. Fitton remarks that the subdivisions of the Wealden formation, especially at its upper part, being in some measure arbitrary, it is difficult to determine to which of the three groups any outlying depositions ought to be referred. (Geol. Trans. vi., p. 323.)

Such a difficulty existed when corresponding portions of a formation were supposed to require an agreement in mineral character and composition; but it happened at all times, as now, with respect to the depositions within areas of water, whether of lakes or seas, that the beds which were strictly equivalent in respect of time, varied from place to place, from marginal shingle to submarginal sand-zones, and deeper and most distant argillaceous ar calcareous mud-beds. Considered in this way, the distant Oxford and Buckingham portions of the Wealden formation, are referable to the submarginal accumulations of the great lake, and may be synchronous with "Wealden clays." For the threefold division of the Wealden series into Purbeck beds, Hastings sands, and Weald clay, must therefore be substituted the more natural divisions of Lower Wealden for the Purbeck series, and Upper Wealden for the series as exhibited in the south-east of England may be of sand and sandstone or Weald clay. according to local conditions of depth.

There are indications that changes in the area surrounding the Wealden formation took place in the progress of that series; the lower and earlier sandy deposits indicate only inconsiderable depths of water. Yet the vertical thickness of the series may be estimated at nearly 2,000 feet; for that area at least progressive depression must have been going on, but not uninterruptedly. As regards the upper and lower divisions of the formation, the difference consists in the greater coarseness of the detritus of the upper, and in the evidence of strong currents settling in definite directions in an extension of the area and of an increased depth, so that at the later stage a central area of deep-water depositions may be defined as well as the directions in which such conditions thinned away. Great changes took place in the depth of the water of the lake, as indicated by the alternations of the drift-sand beds with deeper-water mud deposits, and in places by the conversion of lake-bed into land-surface, upon which plant-growths established themselves for considerable periods of time, and which were again submerged.

Such changes as these seem to imply change in the physical geography of the land region to which this great fresh-water area was subordinate—such, for instance, as would give rise to larger rivers, great influx of fresh waters, and stronger currents.

The successive conditions indicated by the great Wealden group, as a whole, are, for the first stage, that of an extensive shallow lake, or sound, at the sea-level of the time, the inflowing waters to which were largely charged with lime derived from the surface of Portland Oolite, from which they came. This is the Purbeck stage, which commenced with a long period of purely fresh-water conditions. Brackish-water conditions followed with a change of fauna. Mollusca, such as *Corbula*, *Cardium*, *Modiola*, *Rissoa*, appear, presenting—as was observed by the late Edward Forbes—the change of character which the Caspian-sea Molluscs have at present in adapting themselves to brackish-water.

During the Middle Purbeck series the alternations from fresh to brackish water conditions were frequent, and apparently of short duration, till finally it was closed as it commenced, by a thick set of purely fresh-water depositions.

The changes in the Purbeck series are readily accounted for by reference to areas of water such as occur on the American coast at present, and which may be salt or brackish, according to the extent to which the sea-waters are excluded by sand-bars from mixing with the fresh waters flowing from the land.

The S. and E. coast-line of our Wealden lake must be looked for beyond the area of our island.

Wealden Formations of the European Surface

The elliptical form of the Wealden elevation and denudation has its completion on the east in Picardy, across the English Channel. In the Boulonnais there occur ferruginous sands like those of Shotover, full of fresh-water shells (*Unio*) overlying Purbeck limestone, and passing beneath the Cretaceous formation, just as happens in this country. These Wealden beds are not now of any considerable thickness, having been reduced by the denudation of the district. They are so mixed up with pebble-beds in places as clearly to indicate a marginal line, which may safely be placed to the north of the Boulonnais denudations; for the Wealden depositions proper hardly rise to the level of the Palæozoic rocks of Marquise. The great fissures and pot-holes in the limestones there, which have been produced under subaerial conditions, and filled with sand, mould, and much vegetable matter, had been produced antecedently to the deposition of the Gault over that area.

The Wealden beds of the Boulonnais were formed beneath the waters of the same lake as our own. This fresh-water area had an extension southwards; thus M. D'Archiac refers the mottled clays beneath the iron sands and sandstones at Havre to the Wealden series of this country, so that the limits of our lake in that direction, or in the south, lay somewhere along the line of the English Channel.

Sixty miles to the south of the Boulonnais is a district known as the Pays de Bray, which is an elliptical valley of elevation and denudation, like our own Wealden on a small scale, extending from Beauvais to Neufchâtel, a distance of forty-five miles. In this denudation the lowest beds exposed belong to the marine Jurassic series (Portland Kimmeridge). Next above the Portland stone is a Wealden formation. "Les dépots regardés comme fluviatiles sont les plus voisins de létage Portlandien et forment le groupe inférieur du terrain Néocomien" (Graves, Oise, p. 55). The remains of the fishes, *Cyrenæ*, *Cyprides*, and ferns are such as occur in our Wealden.

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* *Vide* evidence as to range of Wealden deposits, Phillips' "Geology of Oxford."

The thickness of this fresh-water formation is inconsiderable compared with our Wealden. The separation of the fresh-water formation from the marine Portland is well defined; not so that betwixt the Wealden and Neocomian. Here, as in the Punfield section, the fresh-water and marine conditions seem to have alternated, and the manner in which this takes place suggests the supposition that the influx of a considerable body of fresh water from the land of the time took place not far from this place.

Neufchâtel is seventy miles south of Boulogne; the Wealden beds, as we have seen, indicate that the series extended southwards from Marquise; and it is no unreasonable supposition that the deposits of this Pays de Bray were formed under the waters of the same lake as were those of our own Wealden.

Such, then, were the dimensions of the Wealden Lake or Sound; it extended from parts of Buckingham, on the north, half across the English Channel on the south, a breadth of 160 miles. In the contrary direction it reached from Wiltshire into France, beyond Beauvais for 250 miles.

In another part of France, Depart de l'Aube, Mr. Cornuel has described a fluvio-lacustrine formation between the Jurassic and Cretaceous formations at Vassy, containing *Iguanodon*, several species of *Unio* and *Planorbes*. The lacustrine formation at Cimey is in a corresponding geological position.

In the Jura, Villers, Foncine-le-bas, the Portland beds are followed by hard bluish marls, calcareous marls, and gypsum, the whole very like our Purbeck series. These lacustrine formations are interesting, as they seem to show the existence of a chain of lakes stretching across France into Switzerland for 260 miles, with a general direction parallel to the axis of Artois, and thus connected as part of one great lake system with our Wealden.

In France, Depart. des Deux Charentes, some 350 miles due south of our Sussex coast, there occurs a great fresh-water formation in intermediate position between the Portland Oolite and what were then the lowest beds of the Cretaceous series. Like our own Wealden, this also is exhibited over a surface from which the Cretaceous strata have been denuded. This formation has engaged the attention of many French geologists, more particularly of M. Coquand, who has determined its age and purely lacustrine character, and who puts it as the equivalent of the Purbeck beds of England; in this he seems to be guided by the general likeness as to composition and the presence of *Physa Bristowi*, a well-known Purbeck species.

The sequence of events at this place was as follows:—Subsequently to the formation of the Portland Oolite the sea-bed became terrestrial surface, and subsequently again to that, a depression extending from Chateauneuf, near Angoulême, to beyond the Island of Oléron, became the site of a great fresh-water lake. From St. Jean d'Angely to Chateauneuf is a distance of thirty-five miles, and from Chateauneuf to Oléron, S.E. to N.W., is upwards of 100 miles; but then figures do not give the full dimensions of this fresh-water area, as its deposits have been reduced by denudation on the north, and passes beneath the Cretaceous series on the south. The original lake must have had a westerly extension seawards, and its area must have equalled that of Lake Ladoga.

The feeders of this lake are more easily accounted for than in the case of our own Wealden. Such a lake would necessarily have received all the streams descending from the western slopes of a terrestrial surface of very ancient date, namely, the granitic district of Central France.

In North Germany there is a well-exhibited Wealden formation, extending from Bentheim by Rheine, with a breadth from north to south of twelve miles. From Ibbenhüsen it reaches on the south side of the Triassic and Palæozoic axis of Osnaburg for many miles. It is everywhere in an intermediate position betwixt the Upper Jurassic and Lower Cretaceous formations. On the north of the axis it spreads for seventy miles to Minden, certainly as far as north as the Steinhuder Meer to near Hanover, and as far south as the Hils district. From west to east the ascertained extent of this lake is upwards of 120 miles.

At Bentheim the dark Wealden clays, with bands of limestone and spathic iron ore, with *Cyrenæ*, *Melaniæ*, &c., like those of Sussex here, are 400 metres thick, so that the real dimensions of this northern lake were very much greater than has been here given.

These large lacustrine areas imply that there was at that time a corresponding extent of terrestrial surface. And it may fairly be asked, what is the geological evidence of such a condition? There occur over parts of Belgium the remains of such a terrestrial condition of surface beneath the lower Cretaceous beds there (Tourtia) consisting of variegated sands and clays, with much diffused vegetable matter, and occasionally with beds of lignite; such surfaces can be traced along the line of the Belgian coal-field (Mons), and overlying parts of the Palæozoic series. These beds are not of sufficient dimensions to be termed lacustrine, but have all the characters of the deposits of ponds and marshes; and M. Dumont has properly referred them to the Wealden period. Such like evidence of terrestrial conditions recur over a wide European area; such are the subcretaceous beds of pisiform iron ore, of subaerial origin, and the wide area over which fresh-water sands with *Pterophyllum*, *Pecopteris*, *Cycadites*, &c., of our Wealden are met.

The break betwixt the marine Jurassic and Cretaceous formations is very distinct, physically and zoologically; and it may be fairly asked, in what way do the forms entombed in the products of the intercalated period of terrestrial surface conditions serve to throw any light on what took place during that long interval of time?

That the earliest Purbeck-Wealden fauna should have Jurassic relations, that is to say, that it must have synchronised with such, wherever that formation was being continued, is only what might be expected; for the whole of the bed of the Jurassic seas in the northern hemisphere was not converted into subærial surface at once. Midway in the course of the Purbeck-Wealden series there is evidence of the recurrence of marine conditions with Portlandian forms, such as *Ostra distorta* and *Hemicidaris purbeckensis*. It was on this ground that Prof. E Forbes suggested the propriety of placing the Purbeck series with the Jurassic in systematic grouping; for it showed that up to the time of the Middle Purbeck beds the marine fauna of the nearest seas was still Jurassic.

The considerable extent of land surface in the northern hemisphere during the whole of the marine Jurassic period, and the local conversion of any portions of such sea-bed into land, whether in the course of the deposition of the Lower Jurassic series (Stonesfield), or between the Lower and Middle (Brora, Staffin), or at the uppermost stage (Portland), would be merely the addition of so much more to the existing land.

The forms of life which would colonise such new surfaces would be such as migrated from the older adjacent lands; if any change should have taken place in the fauna or flora of such old land-surface, in the course of the production of the marine Jurassic series, it would be recorded in the forms entombed in the lacustrine formations of the several stages here alluded to.

The fossil-plants and fresh-water shells from Brora, Lock Staffin, and the Wealden seemed at first to certain well-known and competent naturalists to show that an identical set of forms ranged throughout. A minutely critical examination has since indicated shades of difference; yet it may be questioned whether such are greater than different localities in the same zoological province now present, allowance being made for differences of these old estuarine and lacustrine areas.

The relations of the land-surface forms of the Wealden formations of the European surface has been recognised by all naturalists as being Jurassic rather than Cretaceous. In this the Purbeck-Wealden group offers an exact counterpart zoologically and geologically, of the Permian-Trias group; just as the marine zoological relations of the Permian are Palæozoic, so those of the Purbeck are Jurassic; and when next after each of these, and after the wide spread of purely marine conditions over the northern hemisphere at each period, the marine fauna is seen to have undergone a complete change, in the one case Palæozoic forms go out, and for ever, to be succeeded by Mesozoic or Jurassic; in the other Jurassic forms go out and the Lower Cretaceous come in, and are those which interchange with the uppermost Wealden fauna at Punfield and the Pays de Bray.

Did time allow, I might call attention to the results of the labours of the distinguished palæontologists who have described the forms of life of the Wealden period, both of animals and plants. From them we know that crocodiles and chelonians, referable to many genera, abounded in the Wealden waters. These, with the cycadeæ of the land, sufficiently mark the temperature of that time as being much higher than it is here at present. With respect to the numerous large terrestrial Dinosaurs, it is observable that as yet they are nearly all peculiar to our Wealden lake. The relative level of this lake seems throughout to have been such as to have admitted of easy communication and interchange with the waters of the sea; and this condition may serve to account for some of the peculiarities which its fauna presents.

SECTION D

SUB SECTION ANTHROPOLOGY

OPENING ADDRESS BY THE PRESIDENT, COLONEL A. LANE FOX

AFTER some opening remarks, the author said :—"As one of those who for some years past have taken part in those practical measures which have been as yet only partially and feebly instrumental in promoting the union of the anthropological sciences, it occurs to me that the present occasion may be a fitting one for expressing some of the views which have suggested themselves to me in the course of my experience whilst so engaged. I propose, therefore, after considering briefly the existing phases of one or two of the more important questions with which anthropology has to deal, and saying a few words on the relative value of certain classes of evidence, to speak of the anomalies and misadjustments in what may be called the machinery of anthropological science, defects in the existing constitution of some of the societies which either are or ought to be included amongst the branches of our great subject. In the remarks which I shall offer upon this subject it is not my wish that any undue weight should attach to the particular suggestions which I may be called upon to make as in any way emanating from this chair. My object is rather to draw the attention of anthropologists to the urgent necessity which exists for better organisation than to propound any particular schemes of my own; indeed, so rapidly do our views change in the infancy of a science that I should be sorry to bind myself over to accept many of my own opinions a couple of years hence, for there is, perhaps, no branch of study to which we may more truly apply the dictum of Faraday that "the only man who ought really to be looked upon as contemptible is the man whose ideas are not in a constant state of transition."

Amongst the questions which anthropology has to deal with, that of the descent of man has been so elaborately treated, and at the same time popularised by Mr. Darwin, that it would be serving no useful purpose were I to allude to any of the arguments on which he has based his belief in the unbroken continuity of man's development from the lower forms of life. Nor is it necessary for one to discuss the question of the *monogenesis* or *polygenesis* of man. On this subject also Mr. Darwin has shown how unlikely it is that races so closely resembling each other, both physically and mentally, and interbreeding as they invariably do, should on the theory of development have originated independently in different localities. Neither are we now, I think, in a position to doubt that civilisation has been gradually and progressively developed, and that a very extended, though not by any means uniform, period of growth must have elapsed before we could arrive at the high state of culture which we now enjoy. The arguments of our sectional president, Sir John Lubbock, on this subject may, I think, be accepted generally as those of the best exponent of these views in our own time; such was the opinion, as we learn from various authorities, that was held by most of the ancient authors, and it tallies in all respects with the phenomena of progress now observable in the world around us, or which have been recorded in history. . . .

How far the first beings worthy of being called men may have possessed superior organic psychical powers to their predecessors, and whether the superior functions of the human mind were developed slowly or rapidly is a point on which it is more difficult to form an opinion. In contrasting the psychical differences between man and the lower animals, it is so invariably the practice, and indeed so impossible to avoid including in our estimate of the human intellect all that conscious education and unconscious infantile culture has added to the powers of the mind, that unless we were able to try the experiment of the Egyptian king, and send children to be brought up with animals apart from all intercourse with the human race, we could not place ourselves in a position to compare truly the innate capacities of the two, or to form any just estimate of the difficulties which primæval man, even supposing him to have possessed mental powers equal to our own, must have encountered in the first stages of human culture. It has been shown by Prof. Huxley and others that there is really no cerebral barrier between men and animals, nor does it appear beyond the pale of possibility that a slight increase in the vividness or permanence of the impressions of external objects upon the mind over that possessed by the brutes, might, by marking more clearly the sequence of events, be sufficient to imitate that faculty for improvement which is the special characteristic of man.

Be that as it may, there is, I believe, nothing in the constitution of our own minds which can lead us to doubt that the progress of our first parents must have been extremely slow, or that the slight improvement observable in the implements of the neolithic over those of the palæolithic age, did actually correspond to the continuous progression of human culture during enormous periods of time.

Now, if it is true that during the countless ages included in the palæolithic and neolithic periods, which we know to have been marked by great geological changes, by the union and separation of great continents, by great changes of climates, and by the migration of various classes of fauna into distant parts of the earth, the progress of mankind was as slow and gradual as we are warranted in supposing it to have been by the relics which have been left us, considering how short the period of history during which the rapid development of civilisation has taken place is, in comparison with the long periods of time of which we have been speaking, and that progress is always advancing at a rapidly increasing ratio, we need find no difficulty in supposing that where savages are now found in the employment of implements corresponding to those of the neolithic age, they present us with fairly correct pictures of neolithic culture, being really in point of time only a little behind us in the race of improvement. It is reasonable also to suppose that the use of such tools by savages, and the culture associated with them, was also like that of our neolithic parents inherited from lower conditions of life, and that being slow and continuous, it was sufficiently stable to enable us to trace connections between people in the same stage now widely separated, and between them and our own neolithic ancestors.

The most remarkable analogies are in reality found to exist between races in the same condition of progress, and it is to the study of these analogies, with the view of ascertaining their causes and histories, that the attention of anthropologists has of late been especially drawn, and on this subject I propose to make a few observations.

There are two ways in which it has been attempted to account for those analogous coincidences, one by the hypothesis of inheritance to which I have already referred, the other by the view of the independent origin of culture in distant centres, assimilated in consequence of the similitude of the condition under which it arose It is said that the wants of man being identical, and the means of supplying those wants by external nature being alike, like causes would produce like effects in many cases. There can be little doubt that many remarkable analogies have arisen in this manner, especially amongst the very variable myths, customs, religions, and even languages of savage races, and that it would be dangerous to assume connection to have existed except in cases where a continuous distribution of like arts can be traced. On the other hand, we should commit a grave error if we were to assume the hypothesis of independent origin, because no connection is found to exist at the present time, for we are as yet almost entirely ignorant of the archæology of savage and barbarous races. It is but fifteen years since we began to study the prehistoric archæology of our own race, which has already carried us so far on the road towards connecting us with savages; and can we say what further connections may be brought to light when the river drifts of such rivers as the Niger or the Amazons come to be studied. Nor can it fairly be said that the wants of mankind are alike in all cases; for if we adopt the principle of evolution, it is evident that the wants of man must have varied in each successive stage of progress, diminished culture being associated with reduced wants, thus carrying us back to a condition of man, in which, being analogous to the brutes, he could scarcely be said to have any wants at all of an intellectual or progressive character.

It would be an error to apply either of these principles exclusively to the interpretation of the phenomena of civilisation. In considering the origin of species, we are under the necessity of allying ourselves either on the side of the *monogenists* or that of the *polygenists*, but in speaking of the origin of culture, both principles may be, and undoubtedly are, applicable; there is in fact no royal road to knowledge on this subject by the application of general principles; the history of each art, custom, or institution, must be diligently worked out by itself, availing ourselves of the clue afforded by race as only the most probable channel of communication and development. We may be certain however that in all cases culture was continuously and slowly developed. . . .

There is but one existing race the habits of which are sufficiently well known, which can be said to present in any great degree the characteristics of a primæval people, and that is, the Australians. As I have elsewhere noticed, all the weapons and tools of the Australians, whatever the uses to which they are applied, are closely allied to each other in form. The spear, the club, the malga, the boomerang, and the heileman, or rudimentary shield, all pass into each other by sub-varieties and connecting links, and all consist of the but slightly modified natural forms of the stems of trees, and other natural productions. The Australian in his arts corresponds the most closely of any people now living to those of the palæolithic age. His stone axe is sometimes held in the hand when used, and like the palæolithic man, he has not yet conceived the idea of boring a hole through it for the insertion of a handle. In some cases he cannot without instruction even understand the use of such a hole when he sees it in the axes of European manufacture. A most remarkable instance of this was brought to my notice not long ago by Mr. Grimaldi, who found on the site of a deserted native camping-ground, a European axe having a hole for the handle, which the natives, unable to conceive the use of this part, had filled up with gum, and hafted by means of a withy bent round the outsides of the hole, in accordance with their traditional custom. In the temporary museum established here during the meeting of the Association, you will see a case containing knives of stone, glass, and iron, all of exactly the same form, and hafted, if one may use such a term for the attempt to form a handle, precisely in the same manner; showing with what tenacity these people retain their ancient forms, even after they have been supplied with European materials.

Now it has been shown that in some cases—and here I especially refer to the account lately published by Mrs. Millett, of the Native School, established under conditions only partially favourable to its success, in the interior of Western Australia [*]—The Australians are found to be not only capable, but even quick in receiving instruction. It is evident, therefore, that we should be wrong if we were to attribute the extraordinary retardation of culture on the Australian continent to racial incapacity *alone*; racial incapacity is one item, but not the only item to be considered in studying the development of culture.

The earliest inhabitants of the globe as they spread themselves over the earth, would carry with them the rudiments of culture which they possessed, and we should naturally expect to find that the most primitive arts were, in the first instance, the most widely disseminated. Amongst the primæval weapons of the Australians I have traced the boomerang, and the rudimentary parrying shield—which latter is especially a primitive implement—to the Dravidian races of the Indian peninsular and to the ancient Egyptians, and although this is not a circumstance to be relied upon by itself, it is worthy of careful attention in connection with the circumstance that these races have all been traced by Prof. Huxley to the Australoid stock, and that a connection between the Australian and Dravidian languages has been stated to exist by Mr. Morris, the Rev. R. Caldwell, Dr. Bleek, and others.[†] And here I must ask for one moment to repeat the reply which I have elsewhere given to the objection which has been made to my including these weapons under the same class, "that the Dravidian boomerang does not return like the Australian weapon." The return flight is not a matter of such primary importance as to constitute a generic difference, if I may use the expression, the utility of the return flight has been greatly exaggerated; it is owing simply to the comparative thinness and lightness of the Australian weapon. All who have witnessed its employment by the natives, concur in saying that it has a random range in its return flight. Any one who will take the trouble to practise with the different forms of this weapon, will perceive that the essential principle of the boomerang, call it by whatever name you please, consists in its bent and flat form, by means of which it can be thrown with a rotatory movement, thereby increasing the *range and flatness of the trajectory*. I have practised with the boomerangs of different nations. I made a *fac simile* of the Egyptian boomerang in the British Museum, and practised with it for some time upon Wormwood Scrubs, and I found that in time I could increase the range from fifty to one hundred paces, which is much farther than I could throw an ordinary stick of the same size with accuracy. I also succeeded in at last obtaining a slight return of flight; in fact it flies better than many Australian boomerangs, for they vary considerably in size, weight, and form, and many will not return when thrown. The efficacy of the boomerang consists entirely in the *rotation*, by means of which it sails up to a bird upon the wing and knocks it down with its rotating arms; very few of them have any twist in their construction. The stories about hitting an object with accuracy behind the thrower are nursery tales; but a boomerang, when thrown over a river or swamp will return and be saved. . . . . To deny the affinity of the Australian and Dravidian or Egyptian boomerang on account of the absence of a return flight would be the same as denying the affinity of two languages whose grammatical construction was the same because of their differing materially in their vocabularies.

(*To be continued.*)

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Kinetic Energy

PROFESSOR EVERETT asks from what source is the gain of Kinetic energy in water which has flowed from higher to lower latitudes derived? I answer, undoubtedly from the earth's rotation. If so, it will be asked, what becomes of the Kinetic Energy which disappears when water flows from a lower to a higher latitude? Mr. Ferrel, a physicist of high authority in all questions relating to the earth's rotation, says that it is all consumed in friction. "If a free body on the earth's surface," says Mr. Ferrel, "should be moved from a lower to a higher latitude without friction by a force in the direction of the meridian, it would acquire a certain amount of relative eastward velocity, which would be the same whether the body moved toward the pole with a very slow uniform velocity arising from a single impulse, or whether it moved with a continual accelerated velocity down a gradient by the force of gravity. If a particle of atmosphere or of the ocean is moved in the same way by a similar force, and does not acquire the same amount of relative eastward velocity, *the difference between the velocities in the two cases is the true measure of the effect of friction.*" (NATURE, June 13).

In my last two letters on the subject, I have inadvertently made a similar statement. But as regards the amount of energy lost being the measure of the effect of the friction, we are, I fear, evidently both wrong. A considerable amount of the 9,025 foot-pounds of energy would be consumed, not in friction, but in work of rotation. But let it be observed that so far as the argument under discusssion is concerned, it is a matter of perfect indifference in what way the energy is consumed. The point which Prof. Everett, Mr. Ferrel, and all those who defend Dr. Carpenter's theory has to explain is this, *viz.*, How is it that six foot-pounds of energy can carry a pound of water from the equator to latitude 60°, while during the passage of the pound of water not less than 9,925 foot-pounds of energy is consumed in overcoming the resistance to its eastward motion? How is it that in a fluid, in which the molecular resistance to motion is equal in all directions, a body manages to lose 1,500 times more energy in moving in one direction than it does in another, and yet the velocity of motion is the same in both directions? Then if this cannot be explained, how is the gravitation theory of oceanic circulation to be maintained?

JAMES CROLL

Edinburgh, August 9

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ERRATA.—Vol. vi. p. 273, col. 1, line 31, for "Lenou's" read "Lesson's"; line 45, for "special" read "spiral", line 57, for "fold" read "folds"; col. 2, line 6, for "Edentata" read "Edentate."

THE BRITISH ASSOCIATION.—*Authors of papers are requested to favour the Editor of* NATURE *with copies or abstracts of their communications as soon as possible, addressed to him at the Post Office in the Reception Room, as by these means alone can an accurate and early notice be insured. The Editor appeals to men of science to aid him in his attempt to give an account of the results of their investigations to their brethren throughout the world.*

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[*] "An Australian Parsonage, or the Settler and the Savage," by Mrs. E. Millet, chap. vii.

[†] Journal of the Anthropological Institute, No 1, vol. i., July 1871.

THURSDAY, AUGUST 22, 1872

AIR AND RAIN

Air and Rain. By R. A. Smith, Ph.D., F.R.S., General Inspector of Alkali Works. (Longmans, 1872.)

THIS work contains the germs of a system of chemical climatology. It indicates a plan of testing the purity of the atmosphere of localities with regard to certain constituents of organic origin—the *débris* of living things—by washing the air, and determining the character and amount of the substances in solution by certain microchemical methods. By the systematic repetition of these testings, the possibility is foreshadowed that we may be enabled to classify such atmospheres, and actually to assign to them quantitative sanitary values. It thus points out how we may be able to estimate the difference between the vitiated air of the town and the pure air of the country. Our senses and experience tell us plainly of the existence of such differences; but chemistry has been hitherto powerless to detect them. "It seemed to many as if the eye had obtained a mysterious power of seeing what was scarcely capable of being proved to be within the domain of substance, and the smell had a power of observing what was more an influence than a positive thing." Cavendish, nearly a century ago, asserted that chemical experiments could not distinguish the air of London from the air of the country; and in spite of the labours of Bunsen and Regnault, Frankland and Williamson, which have rendered gasometry more susceptible of refinement and accuracy than any other branch of chemical analysis, this assertion seems as true of to-day as it was of the time when uttered. Hitherto chemists, in judging of the quality of the air of any locality, have been obliged to content themselves with determining the proportion of oxygen and carbonic acid which it contains, in conformity with the practice of their ancestors of a century back. Gradually, however, they have been forced to the conclusion that such determinations have very little positive value in enabling them to assign a value to the sanitary condition of an atmosphere—that oxygen was no panacea, nor carbonic acid as deadly as strychnine; and thus we have been thrown back upon our unaided senses to distinguish between the good and the evil. Supposing that some Martin Chuzzlewit, going out to another Eden, required information respecting the sanitary condition of the settlement, the chemist could tell him something concerning the water he might have to drink, but he would be utterly unable to enlighten him respecting the air he would be compelled to breathe. Some such considerations prompted the inquiries which have resulted in this book. Dalton's assertion that he could not distinguish the air of Manchester from that of Helvellyn, or generally the air which depresses from that which cheers and invigorates, seems to have forcibly impressed the author. For upwards of forty years he has laboured to remove the stigma on chemical analysis, and in this volume he concentrates his thoughts and experimental results. "It was with the desire," he says, "of clearing the mystery of air to some extent that I have devoted so much of my time to the subject; and now I feel that, whilst I have succeeded in doing much of that which I intended to do, I have not got beyond the limits which earlier observers attained by the mere fineness of unaided sense, and by sound reasoning without experiment. Still I hope I shall be found to have put their suspicions into plainer language, proved that which they only imagined, and given in detail that which they only in a general, and, we may add, in a vague manner, had attained."

Dr. Smith first sets out by defining the composition of a normal atmosphere, as deduced from the many analyses which have been published, and from numerous supplementary determinations of his own made on air collected in various parts of Great Britain and on the Continent. In the outset he insists on the value of minuteness in reading the figures; differences which in the eyes of most chemists are of little value, are to him full of meaning. Every deviation from the standard of purity is to be rigidly criticised. Thus, the difference between 20·980 and 20·999 in the percentage amount of oxygen means a difference of 190 parts in a million. If this consisted of organic matter, or the gases of putrefaction, it might become of the gravest consequence. Certainly 190 parts of putrefying matter in 1,000,000 parts of water—equal to 13·3 grains per gallon—would be considered as an enormous quantity. But, comparatively speaking, we drink only a small quantity of water, and the whole 13 grains would not be swallowed in a single day; whereas we draw through our lungs nearly a couple of thousand gallons of air daily. But, indeed, differences much greater than this are found to exist. Thus, the air of a theatre sometimes contains as little as 20·7 per cent. of oxygen, and even this is by no means an exceptionally small quantity for such a place; and yet this amounts to a deviation of 3,000 parts in a million from the standard of purity.

In speaking of the proportion of carbonic acid in the air, the author bases certain considerations (p. 11) upon the assumption that this gas is washed out by falling rain. But is this supposition exactly confirmed by experiment? Saussure, it is true, thought that he could detect a difference in the amount of carbonic acid between the air over the Lake of Geneva and that over the land; but such differences have not been found by other experimenters. Sea air contains about three volumes of the gas in 10,000 volumes; whilst the air of the land contains only four volumes. But this difference is due more to the influence of the land than to any absorptive action exerted by the sea. Indeed, from a consideration of the laws of gaseous absorption, it can be shown that the pressure exerted by the relatively small quantity of carbonic acid present in the air is unable to bring about any perceptible variation in its amount over sea and land.

Having fixed on his standard of purity, Dr. Smith proceeds to examine vitiated air and to trace its effects on breathing. For this purpose he used an air-tight chamber in which one or more persons could be seated; and from time to time he collected and analysed samples of the enclosed air, and compared the analytical numbers with the sensations experienced and noted at the moment of collection. The details of these experiments are of great interest, and merit careful study. That they were not unattended with danger is evident from the experience of a young lady "who was extremely fond of pure air," but who in the cause of science "was anxious to be in the chamber when the candles went out." At such a time there would be about 19 per cent. of oxygen and 2·1 per cent. of carbonic acid in the chamber. No person had been in the chamber previously. "She stood five minutes perfectly well, and making light of the difficulty; but suddenly became white, and could not come out without help. She was remarkably healthy, never was ill, and was troubled with no fear of the air in which she stood."

From the air to the rain which falls through it, is but a single step; for if, as our author says, there is life and death in the air, we must believe the same of the rain, which collects the solids and liquids, gases and vapours which float about in the atmosphere. These ingredients of rain water can, indeed, be shown by chemical analysis; and by the microscope distinctions may be drawn between the air of various localities, without putting the health to the test. The author proceeds to describe his methods of testing rain water; but as the details of the scheme are mainly of interest to chemists, we must refer any curious readers to the book itself. Much of the work herein detailed was done years ago; long, indeed, before Pasteur had enlightened us as to the great reservoir of life which exists in our atmosphere. In 1852 Dr. Smith showed how complicated a fluid rain is. However carefully collected, albuminous bodies, the remains of living creatures, and minute animalculæ, may invariably be detected in it. "These creatures," says Dr. Smith—anticipating Dr. Frankland's aphorism, "Ohne Phosphor gar kein Leben"—"are sufficient of themselves to show the existence of phosphates, whilst sulphates and lime may be readily obtained. In examining the Thames water I often found that the readiest way of collecting phosphates and magnesia was to wait for the animalcules to do it."

Through the kindness of a number of gentlemen, Dr. Smith was enabled to make numerous collections of rain water from as far north as the Hebrides and as far west as Valentia. The results of the samples of the analysis may be thus briefly summarised:—The rain over the sea contains chiefly common salt, which crystallises clearly; but it also contains sulphates, and in larger proportion to the chlorides than is found in sea water. The amount of these sulphates increases inland before large towns are reached. They are to be regarded as the measure of the products of decomposition, the sulphuretted hydrogen from putrifying organic compounds becoming oxidised in the atmosphere. Within certain limitations, they may be taken as an index of the amount of sewage in the air. We accordingly find in the large towns the amount of the sulphates is greatly increased, owing to the combustion of the sulphur in coal, as well as the decomposition of organic matter contained in protein substances. When the sulphuric acid increases more rapidly than the ammonia, the rain-water becomes acid, and when the amount of this free acid reaches two or three grains in a gallon, or forty parts in a million, there is no hope for vegetation in a climate such as we have in the northern parts of this country. These free acids are not found with certainty where combustion or manufactures are not the cause. The amount of ammoniacal salts in the rain water increases with the number of towns in the district. This ammonia comes partly from the coal, and partly from the decomposition of albuminoid substances, which, indeed, may also be detected in the rain water. It is very interesting to compare the relative purity of the atmospheres of our cities and large towns, as determined by this method of air-washing. Upon the whole, that of London appears to be the best, and that of Glasgow decidedly the worst. Calling the amount of sulphuric acid in sea air 100, the average amount in that of London is 352, and in that of Manchester 513. In Glasgow the amount of ammonia is 150, in London it is 115; the amount of albuminoid ammonia in London air is only 109, whereas in Glasgow it is more than twice that amount, viz. 221. These analyses show unmistakably in what the evil of overcrowding consists; and it is with this subject on which he is thus led to speak that Dr. Smith closes his book. We commend his remarks to our City Improvement officials:—"Let those courts, alleys, and streets, which show the greatest mortality and the worst air, be destroyed or improved, without foolish mercy. There is a want of willingness to pull down dangerous property, but a readiness to rush forward to save the life of the greatest criminals. Reason is out of the question in the matter. We are misled by an uneducated feeling. We like to save property, forgetting that deadly weapons and poisons are subject to peculiar laws, and their indiscriminate use is forbidden to the nation. And houses that produce death are not property; as well might a man claim his debts as such. If a man sells unwholesome meat the law interferes; if he sells the use of a room with fever in it, the nation seems not to complain. Officers of health point out such places, but the public still refuse to destroy them, and great numbers are slain annually by legal methods, while strict methods are taken to prevent a few annually being killed by arsenic—a death more agreeable than the lingering misery in the lower parts of our crowded towns. I know that the lowest classes living in poisoned houses die from other causes than bad air; but I am speaking of air at present, and that is one of the causes. The time must come—and the sooner the better—when it shall be enacted that no land shall contain more people per acre than we know by experience in several places can live healthily thereon. The same thing must be said regarding houses, although these are more difficult for governments to deal with, because of the degradation of some of the population. Still the limitation must be attained, and for that we must strive."

T. E. THORPE

THE IRON AND STEEL INSTITUTE

THE Fourth Annual Provincial Meeting of the Iron and Steel Institute was held recently (August 6th to 9th inclusive) in Glasgow, under the presidency of Mr. Henry Bessemer, and it has been, if it were possible, even more successful than any of the previous reunions, furnishing thereby the best proof that such an association actually was a desideratum, and of the hearty co-operation which its establishment has called forth, from all interested directly or indirectly in the development of the Iron and Steel manufactures of Great Britain.

Since the three previous meetings were all held in the iron districts south of the Tweed, it is the more gratifying on this occasion to find that the first meeting of the Institution in Scotland should have turned out so eminently successful, and so marked by the hearty welcome with which Scotch ironmasters have received their Southern competitors in the trade; and it cannot but be observed that since the foundation of the Institute, a very different spirit has infused itself among the members of the iron and steel trades in general, as they no longer keep themselves jealously aloof from one another, but, on the contrary, are now pleased to meet, and in a spirit of generous rivalry to interchange their ideas, thereby to some extent, at least, tacitly acknowledging that the advancement of industry of the country at large, so far from being prejudicial to, is in reality ultimately connected with, the interests of each individual manufacturer also. It is the recognition of this principle which has enlisted the sympathies of those engaged in the iron and steel industries, and has contributed so much to the success of the Institute, which at this moment, including those candidates nominated at the present meeting, numbers no less than 602 members—a surprising result when it is remembered that the Association is now only in the fourth year of its existence.

The Glasgow meeting of the Institute commenced on Tuesday, August 6th, when its members assembled in the Corporation Galleries, Sauchiehall Street, which had most liberally been placed at the disposal of the Council of the Institute by the Lord Provost and Magistrates of the City; the proceedings being prefaced by a short introductory address from the President (Mr. Bessemer), together with a few words of welcome from the Lord Provost of Glasgow.

The Secretary then announced that the Council had recommended Mr. Isaac Lothian Bell, of Newcastle, as president-elect, and nominated Mr. Edward Williams, of Middlesborough, as vice-president in the place of Mr. Bell, as also Messrs. W. S. Roden, C. W. Siemens, H. Sharpe, W. Nielson, and J Hunter to the vacancies caused by the retirement of the other members of the Council by rotation, which recommendations were unanimously adopted by the meeting.

The Foreign Secretary read out the names of those gentlemen connected with the foreign iron trade then present at the meeting, amongst whom were representatives of France, Belgium, Germany, Sweden, and the United States of America.

The election of members was then proceeded with, after which an extremely interesting paper, "On the Coal and Ironstone Strata of the West of Scotland," was read by Mr. James Geikie, of the Geological Survey, in which a general sketch of the geology of the district, with special reference to the occurrence and nature of the coal and ironstone deposits, was given in a concise yet extremely lucid communication, the delivery and discussion of which occupied the remaining available time of this day's meeting. Visits were subsequently made by the members to some of the neighbouring works, and more particularly to the Blochairn Iron Works, where considerable attention was paid to Mr. Graham Stevenson's new mode of reversing rolling mills then in operation.

On the following day (Wednesday) the meeting commenced at 10.30 A.M. by the reading of a paper by Mr. J. F. Mayer, of Glasgow, "On the Rise and Progress of the Iron Manufacture of Scotland," which was an historical sketch of the subject, commencing from the year 1760 when iron was first smelted at Carron, near Falkirk, and continuing it down to the present time, when the Scotch iron manufacture occupies so prominent a position in the British iron trade. Attention was specially directed to such improvements in the manufacture of iron as had originated in this part of Scotland, amongst which were mentioned the use of raw coal instead of coke in iron smelting by Condie at Govan, the employment of hot blast by Nielson, and the utilisation of the black band ironstone by Daniel Mushet, discoveries which, it may be said, were the making of the Scotch iron trade.

The next two communications related to the different systems for reversing the rolls in rolling wrought iron; the first of these by Mr. J. D. Napier, "On Napier's differential gear for reversing rolling mills," illustrated by models, described the application at the Codnor Park Iron Works in Derbyshire of a differential clutch, identical in principle with the differential breaks used by him in the windlasses of ships; whilst the second, by Mr. Graham Stevenson, "On Reversing Rolling Mills," advocated the employment of his conical clutch, which had been inspected the previous afternoon at the Blochairn Iron Works. These two papers were discussed together, and gave rise to a very animated debate, in the course of which much valuable information was elicited from the observations made by members practically acquainted with the subject. The balance of opinion appeared, however, to be in favour of Mr. Napier's differential clutch, the extreme simplicity of which appeared to give it advantages over any hitherto applied form of reversing gear, not excluding the conical clutch of Mr. Stevenson, which, nevertheless, was admitted to work very satisfactorily.

The meeting then broke up, most of the members proceeding by a special train to inspect the Iron Works at Gortsherrie, Coatbridge, Summerlee, and Monkland. Amongst the novelties examined on this excursion may be mentioned the new coal-cutting machine, invented by Messrs. Miller and Anderson of Coatbridge, then at work in the No. 3 pit, Gortsherrie, belonging to Messrs. William Baird and Co.

This machine is stated to cut 350 ft. of coal per shift of 8 hours, yielding 75 tons, or equal to the united turn out of 40 men, whilst it only requires two to attend to it, being driven by compressed air at a pressure of 45 lbs. to the square inch, brought in cast iron pipes some 300 fathoms from the shaft. As six additional machines are in course of construction for Mr. Baird, it is evidently regarded as a success; and it is hoped that in these days of incessant strikes it may prove capable of doing all it is represented to do, and thus be the means of checking in some degree the exorbitant demands of the colliers.

At the North British Iron Works, at Coatbridge, M. Dormoy's patent puddling furnace, with revolving rabble, driven by steam power, was shown in full operation, and appeared to elicit the very general expression of opinion that the invention, even if ingenious and practical, had come out too late in the day. Now that the more perfect system of rotary puddling of Mr. Danks has proved successful in dispensing with the labour of the puddler altogether, it is not likely that the ironmaster of the future will be content with a system which at best is only an improved manual process for puddling iron.

At the Monkland Iron and Steel Works the interest of the visitors was concentrated in inspecting the two blast furnaces constructed on Ferrie's patent coking principle. These furnaces have a height of no less than 90 ft., and are so arranged that, when fed at the top with the raw coal and iron ore mixed together, the coal, as it descends, becomes coked in the upper portion of the furnace, before it reaches the smelting region of the furnace lower down. Great economy in fuel is claimed for this arrangement; it being considered that the heat and combustible gases driven off and lost, when the coal is previously coked as at present in separate coking ovens or heaps, are utilised entirely in the Ferrie blast furnace.

The favourable opinion expressed by several of the members of the Institute, will, it is understood, lead to the erection of several of these furnaces, both in the district and probably in England also.

The meeting on Thursday commenced at 10.30 A.M., and was opened by the reading of a paper by Dr. A. K. Irvine, of Glasgow, "On a new miner's safety lamp," in which the author described an invention of a most ingenious character, which is likely to prove of great service in coal mines troubled with explosive gases, since, besides serving the purpose of an ordinary safety lamp, it sounds a note of warning to the workman the moment the air around becomes so charged with firedamp as to be dangerous or explosive. The principle of the lamp is based on the fact that when a mixture of any inflammable gas or vapour, with air in explosive proportions, is lighted on the surface of wire gauze, having meshes sufficiently small to prevent the passage of flame, and a suitable tube or chimney is placed above, so as to prevent admission to the chimney except through the wire gauze, a musical sound is produced varying in pitch with the size of the flame and dimensions of the chimney.

A number of interesting experiments were exhibited to illustrate this principle, and various miner's lamps as constructed were exhibited and tested in mixtures of air with ordinary coal gas, when they at once indicated the danger as soon as the atmosphere by which they were surrounded contained sufficient gas to be dangerous, by emitting a strong clear sound like that of a horn, which could be heard at a considerable distance. Another form of this lamp was also shown, intended to be employed as a stationary warning apparatus or alarum after being placed in any part of the mine considered likely to ensure the safety of the workmen, so that it might sound the danger signal before the air around it was so far charged with fire-damp as to become explosive. The novelty and importance of such an invention were apparent to an audience of practical men; and besides passing a cordial vote of thanks to the inventor, arrangements were made for at once fully testing its merits by its practical employment in some English collieries noted for fire-damp.

After an interesting paper by Mr. D. Rowan, of Glasgow, "On the rise and progress of the iron shipbuilding trade in Scotland," which, however, was of a purely historical and statistical nature; the next communication was made by Mr. Lauth, of Pittsburg, United States, "On Lauth's system of rolling iron by three high rolls." The improvements proposed in this system of rolling, which is in itself very old, consisted in making the central roll of less diameter than the two others, which are of ordinary size, and in having it fixed, whilst the two others are adjustable by screws. In the hard rolls the bottom roll alone is driven, both the middle and top roll being carried round by friction. All expansion or contraction is prevented by a stream of water constantly kept running on to the roll; and great rapidity in rolling, as well as economy in labour, is claimed for this system. In the discussion which followed, the general opinion appeared to be that, although such rolls were well adapted for plates, in this country they were less adapted for rail rolling, owing to the greater difficulty in adjusting the grooves so as to turn out rails as correct in section as was insisted upon by our and most of the Continental engineers, but not in the United States; also because the necessity for three rails would still further augment the immense stock of rolls requisite to suit the multiplicity of sections required in the English trade, as well as increase the labour and time required in changing the rolls.

The next paper was by Mr. A. Spencer, of West Hartlepool, "On further improvements in Spencer's Rotary Puddling Furnace" a model of the furnace in its present form being exhibited, and its construction, mode of fettling, and working, fully entered into by the author; after which, owing to time not permitting, a lengthy paper by Mr. J. Guildford Smith, of Philadelphia, "On the Westward development of the Iron Manufacture of the United States," was taken as read; and after passing votes of thanks to the Lord Provost and civic authorities of Glasgow, the Council of the Philosophical Society, the Committee of the Royal Exchange, the local Committee, and the President of the Institute, the proceedings of the meeting were brought to a close.

In the afternoon an excursion was made by a special train to the Coltness and Mossend Ironworks; the members of the Institute being entertained on their return at a banquet given in the Corporation Galleries by the local Committee of the Institute.

An interesting feature in connection with the meeting was the arrangement of a temporary museum in the Corporation Galleries containing models, specimens, and objects of all kinds bearing more or less directly on the Iron and Steel manufacture, many of the articles exhibited being of great interest.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Solar Outbursts and Magnetic Storms

In the French *Comptes Rendus* of August 4, which has lately reached this country, is an account by Father Secchi of a remarkable outburst from the sun's limb witnessed by him on July 7, which lasted from $3^h\ 30^m$ to $6^h\ 50^m$ (Roman time, I presume), or nearly $2^h\ 40^m$ to $6^h\ 0^m$ (Greenwich time).

A magnetic storm commenced at Greenwich at $5^h\ 0^m$ precisely on the same day. Its indications began at that time with unusual suddenness and strength, on all the magnetic indicators, namely the declination-needle, the horizontal force magnetometer, the vertical force magnetometer, the earth-current wire, in an approximate N.E. and S.W. direction, and on the earth-current wire in an approximate N.W and S E. direction The disturbance lasted, gradually diminishing, to the evening of July 9. During a part of the time it was accompanied with aurora.

I do not venture upon the question whether there really was any connection between the solar outburst and the terrestrial magnetic storm, but I will remark that, if there was such connection, the transmission of the influence from the sun to the earth must have occupied $2^h\ 20^m$; or a longer time if Father Secchi did not see the real beginning of the outburst. This, if established, would be an important cosmical fact; and, at any rate, the notification of this apparent retardation may direct the attention of observers of similar phenomena in future to a new element in their interpretation. G. B. AIRY

Royal Observatory, Greenwich, August 14

Ocean Circulation

ALTHOUGH no mathematician, and only an amateur in physics, it appears to me that the difficulties and objections of Mr. Croll on this subject may be obviated, and the whole question elucidated by a reference to the admitted facts, and a common sense interpretation of them. And first, as to the fact that the surface water of the Atlantic Ocean, in moving northwards from the equator to 60° lat., has almost wholly lost the easterly motion of rotation it should have brought with it. This loss is imputed by Mr. Croll to friction only, and he argues that the much lower velocity of the northward current must, therefore, be wholly neutralised by friction. This is his main argument, which he has repeatedly adduced, and to which he has hitherto received no reply. But, although his reasoning might be admitted if the conditions affecting the two motions were the same, it seems to me to be quite inapplicable to the present case. If, in the temperate zone, the ocean extended uninterruptedly in an east and west direction round the globe, it would no doubt retain a considerable portion of the equatorial eastern motion, and whatever deficiency existed might fairly be imputed to friction. But the Atlantic is actually like a huge lake, with continuous eastern and western shores, and the water which flows northwards along the eastern shore is prevented from moving eastwards, not by friction against water or even against the shore, but by having to perform work in lifting or heaping up the water against the shore, just as the water of a pond or lake is heaped up on the leeward side by a strong wind. As the direction of the motion of the water will, however, by the hypothesis, be oblique or somewhat north of east, some of the motion will be diverted northwards along the eastern shore, and thus tend to increase the northerly flow. The 9,925 pounds of energy (according to Mr. Croll) are not therefore consumed in overcoming the frictional resistance to eastward motion, but for the most part in doing the actual work of overcoming gravitation and holding up the waters at a higher level, and the theoretical amount of this rise can, no doubt, be easily calculated for us by Mr. Croll.

The case of the water moving northward is very different.

There is a clear passage into the polar area, and probably up to and beyond the pole; and within this area there is a continual diminution of bulk of the entering water as it becomes cooled, as well as a continual subsidence of the surface water, producing a partial depression to be constantly filled by water from the south. Experiment proves that if at one end of a vessel of warm water ice is applied at the surface, the cooled water instantly sinks, and its place is taken, not by water rising upwards from below, but by a horizontal movement of the surface gradually propagated to the other end of the vessel, while the descending cold water creeps along the bottom, and gradually acquiring a higher temperature, rises and completes the circuit. It is somewhat difficult to conceive, theoretically, how such a circulation can commence, because the cooled atoms of water must displace others before they can descend, and these again must displace others, and so on over the whole mass. The amount of energy due to the superior weight of the first-cooled atoms may appear inadequate to perform so much work, but nevertheless circulation does commence and indefinitely continues so long as a difference of temperature of the two ends of the vessel is kept up. The extreme mobility of the particles of water, and the almost total absence of friction between them, seems to be influential in producing this result; and it is not probable that any minute difference of level that may be caused on the surface of the water by difference of temperature has anything to do with the motion; and I cannot help thinking that the supposed six-feet incline from the equator to lat. 60° is, if it exists, by no means an effective cause of the oceanic circulation.

ALFRED R. WALLACE

I THINK the root of Mr. Croll's difficulty (see NATURE, p 324) is to be found in his overlooking the possibility of energy becoming potential in the distribution of oceanic water.

Water running in any direction in the northern hemisphere tends to swerve to its own right, and if this tendency is checked (as it is in fact by the presence of continents) its layers of equal density will be tilted up on the right, the limit of tilt being the angle whose tangent is the quotient of the tendency to swerve by the force of gravity. This consideration is, I think, sufficient to deprive Mr. Croll's argument of one of the two legs on which it stood.

Mr. Ferrel's argument from the tides is quite conclusive in showing that the forces arising from difference of temperature are of sufficient magnitude to keep up an oceanic circulation. Thus the other leg of Mr. Croll's argument is gone.

Mr. Croll may well retract his previous assertion that the difference of kinetic energy is consumed in friction, for he was in a fair way to bring the earth to a standstill.

Brighton, August 20 J. D. EVERETT

Spectrum of Aurora

A FINE aurora was seen at Bedford on Thursday night between midnight and one o'clock. It was brightest under the Polar Star near the horizon, where the colour was a pale green; whilst overhead the hue often changed to a rosy red. On directing a spectroscope at the most brilliant part, a bright green line (W.L. 557) was very distinct, and two or three faint nebulous bands more refrangible were visible; but the red line was not to be seen, though carefully looked for on the rosy parts of the aurora. Objects around were faintly illuminated as if by a young moon. At one time two very faint pale green streamers were seen stretching from the north to a little east of the zenith.

Blackheath, August 11 J. P. MACLEAR

The Method of Least Squares

As the wording of Prof. Hall's letter in NATURE for July 25 might imply that he was calling attention to evidence that would change the opinion expressed in my letter, it seems to me worth while to state that at the time of writing that letter I was acquainted with the passages in question, and to repeat my assertion that with reference to the *method of least squares* I should not regard the neglect of Lagrange's memoir as an omission. Also in spite of Encke's and Prof. Hall's remarks, I think it has received as much attention as, viewed practically, its importance entitled it to.

With regard to the principle of the Arithmetic Mean, I may add that I have devoted the greater part of a tolerably long memoir to its consideration, and feel sure that no remarks on the subject contained in a few lines could be rendered even intelligible.

J. W. L. GLAISHER

Blackheath, August 11

NOTES

WE are informed that M. Faye will in all probability be M. Delaunay's successor as Director of the Observatory at Paris. In the meantime M. Matthieu supplies his place *pro tem.*

THE French Academy has elected two foreign correspondents in the section of botany—M. Planchon in the place of M. Lecoq, and M. Weddel in the place of Prof. Mohl.

THE American Association for the Advancement of Science was to commence its sittings yesterday at Dubuque, Iowa. Prof. J. Lawrence Smith, of Louisville, had been elected President, and Prof. Alexander Winchell, of Ann Arbor, Vice-President. It was announced that the citizens of Dubuque had determined that all members attending the meeting should be entertained at their private residences free of charge during the session; and their travelling expenses would also probably be remitted by the various railroad and steamboat lines. A very successful meeting was anticipated.

AT the recent combined First B.A., First B.Sc., and Preliminary Scientific (M B.) Examinations of the University of London, Mr. J. M. Lightwood, of Trinity Hall, Cambridge, obtained the Exhibition in Mathematics and Philosophy; Mr. R. E. Carrington, of Guy's Hospital, the Exhibitions in Chemistry and in Zoology; and Mr. J. C. Saunders, of Downing College, Cambridge, the Exhibition in Botany.

WE regret to announce the death of Mr. Frederick Carpenter Skey, C.B., F.R.S., which took place on Thursday last at his residence, Mount Street, Grosvenor Square. Mr. Skey was in his 73rd year. He was in early life a pupil of the celebrated Dr. Abernethy, to whom he was articled in 1816 by the Royal College of Surgeons. About 1826 he was appointed Demonstrator of Anatomy at St. Bartholomew's Hospital. Subsequently he founded the Aldersgate School of Medicine, which became one of the largest in London. From that time to his death Mr. Skey enjoyed the reputation of being in the first rank of London surgeons. His writings on medical subjects were numerous and important, and on subjects connected with sanitary science his communications to the public journals were frequent.

THE following is the list of candidates who have been successful in obtaining Royal exhibitions of 50*l.* per annum each for three years in the Science and Art Department, and free admission to the course of instruction at the following institutions:—To the Royal School of Mines, Jermyn Street—William Carter, Ambrose R. Willis, and Alexander Gibson. To the Royal College of Science, Dublin—Arthur G. Meeze, Denis Coyle, and Ernest H. Cook.

A NATURAL History Society has been formed at Madrid called "La Sociedad Española de Historia Natural," under the presidency of Don Miguel Colmeiro. The first part of its publication has reached this country, and contains the regulations of the Society, an account of the meetings held by it up to this time, and papers by Poey on Ichthyology, by Colmeiro on the Fumitories of Spain and Portugal, by Espada on the Volcano of Ansango, by Solano on a Meteoric Stone, by Espada on New America Batrachians, and by Perez Arcas on New Reptiles and Insects of the Spanish Fauna. It is extremely well printed, and is illustrated by three capital plates. The subscription to the society is 12*s.* 6*d.* Anyone desiring to become a member may address himself to Don Serafin de Uhagon, Sordo 27, Madrid, the treasurer, or to any other member.

PRIZES for Collections of Economic Entomology are offered for competition in 1873, and the following rules relating thereto have been issued by the Royal Horticultural Society:—10*l.* for a collection of British insects injurious to some one order of plant used for food—as Cruciferæ, Leguminosæ, or corn. The order may be selected by the competitor. 3*l.* for a miscellaneous collection of British insects injurious to plants used as food. 5*l.* for a collection of British beetles injurious to timber and fruit trees either growing or felled. 2*l.* for a collection of British insects injurious to some one timber or fruit tree. The insects are to be exhibited in their various stages of development, accompanied by specimens, models, or drawings of the injuries caused by them. The collections are to be sent in addressed to James Richards, Assistant-Secretary, Royal Horticultural Society, South Kensington, on or before November 1, 1873.

THE threatened destruction of the "Cursus," at Stonehenge, by the ploughing up of the land, is attracting so much public attention, that we may hope it will be in time to arrest this piece of Vandalism.

THE largest and most important of the fragments of the carved column dug up by Mr. Wood at a depth of 23 feet on the supposed site of the Temple of Diana at Ephesus, has been set up in the Græco-Roman room at the British Museum. It measures about 6 feet in height and 18½ feet in circumference, and it is supposed to have formed a portion of the first drum of one of the thirty-six Ionic carved columns which, with ninety-one others, supported and adorned the structure. Portions of the base and capital of the column were also found close by. On the side of the drum, which has sustained comparatively slight injury, there are five figures of considerable beauty, but all more or less mutilated. Of only two of these can the identity be determined—namely, the figures of Mercury and Victory. The former is perfect, with the exception of the face, which is slightly mutilated, and is regarded by competent judges as a work of considerable merit.

DR. HAYDEN, in charge of the Geological Survey of the Territories, having completed his preliminary arrangements at Ogden, has separated his forces into two divisions, one of which was to proceed to Fort Hall, with waggons and a suitable outfit, to be changed into a pack train at Fort Hall, and thence to travel up the Snake Valley, under the direction of Mr. Stevenson; the other division, under the doctor's own charge, was to start soon for Fort Ellis, and expected to be at work there by the 1st of July. Among other interesting observations already made by Dr. Hayden's expedition, was the occurrence of invertebrate animal life in great abundance in the Great Salt Lake. This fact is not entirely new, as the existence of dipterous larvæ in these waters has already been recorded by Captain Stansbury and others.

PROF. DAVIDSON, of the United States Coast Survey, has recently, before the Academy of Sciences of San Francisco, contested the theory of Mr. Octave Pavy in regard to polar currents and the topography of the polar regions. In his paper the professor maintains, in opposition to the views of Mr. Pavy, that the currents flow eastward through the straits north of the American continent, and that the current through Behring Straits is local, and unimportant in its effects as regards the polar basin; that Wrangell's Land is not a region continuous to a great distance toward the pole, as contended by Mr. Pavy, but a small island or cluster of islands.

THE BRITISH ASSOCIATION MEETING AT BRIGHTON

BRIGHTON, *Tuesday Morning*

THERE is a general agreement that the Brighton meeting of the British Association is a brilliant one; though we fear that this phrase, as used among Brightonians, refers rather to the fashionable character of the audiences at the various sections than to the scientific value of the papers read. Not that this latter has been below the average; but so far at least there has been no one paper or discussion which has placed a distinctive character on the meeting of 1872 as marking an epoch in scientific thought. We miss, also (no doubt owing to the remoteness of the locality from the great intellectual foci of the north), the familiar faces of many who are wont to add life and interest to our meetings; while the London *savans* do not appear to muster in greater force than when the meeting is held 300 miles from the metropolis. The spacious dome of the Pavilion was crowded to its utmost capacity on Wednesday evening, to hear Dr. Carpenter's opening address; and so admirable are the acoustic properties of the building, that each word was distinctly heard in every corner, if we may use the term in describing a circular room. Among the distinguished visitors, the curiosity of the audience was about equally divided between Mr. Stanley and the ex-Emperor of the French, both of whom occupied seats on the platform. The total number of tickets issued up to Wednesday evening was 2,140, or only 400 short of the Edinburgh total. Although the rooms in which the various sections are held are scattered, the distances are not great, and the splendid weather makes it easy to get from one to another. The localities selected give rise to some singular incongruities; as when wandering into the Friends' Meeting House, where Section G finds its local habitation, we heard a paper read from the "Ministers' Gallery," "On the progress of invention in breech-loading small arms during the past twenty years."

At the meeting of the General Committee previously held, the Report of the Council was read, in which the following are the more important features:—

The Council announce that a vacancy has occurred in the number of the trustees in consequence of the death of Sir Roderick Murchison, and take this opportunity of expressing their regret at the great loss which science has sustained by his death. He worked long, earnestly, and with eminent success in the sciences of geology and geography, and was at all times a steady patron of rising scientific men in all branches of science. He was a member and strenuous supporter of this Association at its first formation in 1831, and continued until the close of his life a very constant attendant at its meetings and a firm promoter of its interests. The Council recommend that Sir John Lubbock, Bart., be selected to fill the vacancy.

The next subject referred to is the appointment of the Committee to promote observations on the Total Solar Eclipse of December last, from which a Report will be read in due course. The results will be published by the Association to form part of a series uniform with the contemplated reports of the Royal Astronomical Society of the observations of the eclipses of 1860 and 1870.

There were five other resolutions referred to the Council for consideration or action, upon which the proceedings of the Council have been as follows:—

First resolution:

"That the President and Council of the British Association be authorised to co-operate with the President and Council of the Royal Society, in whatever way may seem to them best, for the promotion of a circumnavigation expedition, specially fitted out to carry the physical and biological exploration of the deep sea into all the great oceanic areas."

A copy of this resolution was forwarded to the Royal Society, and a committee was appointed, consisting of the president and officers of the Association, Dr. Carpenter, Prof. Huxley, Mr. Gwyn Jeffreys, Mr. C. W. Siemens, and Prof. Williamson, and authorised to co-operate with the Committee of the Royal Society in carrying out the objects referred to in the resolution The expedition has been organised, the ship *Challenger* is being fitted out at Sheerness, Capt. Nares has been appointed to the command, and Prof. Wyville Thomson (who has obtained three years' leave of absence from the University of Edinburgh) is appointed to the scientific charge, with an adequate staff under him. It is hoped that the expedition will sail about the end of November.

Second resolution:

1. "That it is desirable that the British Association apply to the Treasury for funds to enable the tidal committee to make observations and to continue their calculations.

2. "That it is desirable that the British Association should urge upon the Government of India the importance, for navigation and other practical purposes and for science, of making accurate and continued observations on the tides at several points on the coast of India."

With the result of these applications we have already informed our readers, and discussed the bearings on the future of Science of the attitude of the Government.*

Third resolution:—

"That the Council of the Association be requested to take such steps as to them may seem most expedient in support of a proposal, made by Dr. Buys Ballot, to establish a telegraphic meteorological station at the Azores."

The Council appointed a Committee of their own body to report upon this proposal. The Committee, after due deliberation, reported that while sympathising with the proposal made by Dr. Buys Ballot, they cannot recommend a grant of money to be made by the Association for carrying it out. In this recommendation the Council concur.

Fourth resolution:—

"That the Council be requested to take into consideration the desirability of the publication of a periodic record of advances made in the various branches of sciences represented by the British Association."

The Council, after a careful consideration of this proposal, are not prepared to recommend the Association to undertake the publication of a periodic record of advances made in the various branches of science represented by the sections of the British Association. They are of opinion that in so vast an undertaking special societies should be invited to prepare such records, the action of the Association being limited to occasional grants in aid. They are of opinion, however, that the Association would do well to promote the more frequent publication in their proceedings of critical reports on various branches of science, of the same nature as those which have already rendered previous volumes so valuable to investigators.

Fifth resolution:—

"That the Council of this Association be requested to take such steps as may appear to them desirable with reference to the arrangements now in contemplation to establish 'leaving examinations,' and to report to the Association on the present position of science-teaching in the public and first grade schools.

"That the Council be requested to take such steps as they deem wisest in order to promote the introduction of scientific instruction into the elementary schools throughout the country."

A Committee, consisting of the President and the General Officers, Mr. G. Busk, Mr. Debus, Dr. Duncan, Mr. Fitch, Prof. M. Foster, Mr. F. Galton, Dr. Hirst, Prof. Huxley, Sir John Lubbock, Bart., Sir J. Paget, Bart., Rev. Prof. Price, Prof. H. J. S. Smith, Prof. Stokes, Prof. Tyndall, and Prof. Williamson, was appointed to consider the first of these resolutions, and to report on them to the Council.

In accordance with the recommendation of this Committee the Council adopted the following resolution:—

"That having had under consideration the requests which the Committee of Masters of Schools have made to the Universities of Oxford and Cambridge upon points in which the Education of the Universities and Schools came into contact, the Council of the British Association recommend that arithmetic, and either elementary physics or chemistry experimentally treated, be introduced into the leaving examinations as compulsory subjects.

"That the Head Masters of Public Schools be requested to furnish the Council with information about the present position of Science-teaching in their Schools."

The Council have communicated thereon with the Universities of Oxford and Cambridge, but at present no decision respecting "leaving examinations" has been arrived at in these Universities.

In accordance with the terms of the resolutions passed by the General Committee last year, appointing a Committee on science lectures and organisation, the action proposed to be taken by this Committee in the following resolutions was referred to the Council and sanctioned.

"1. That a Sub-Committee, consisting of Dr. Carpenter, Prof. Williamson, Prof. W. G. Adams, Dr. Hirst, Mr. Geo. Griffith, Dr. Michael Foster, and Prof. Roscoe, be appointed for one year for the purpose of preparing a list of Lecturers for the consideration of this Committee, and of communicating with the various towns with the view of establishing a system of Science Lectures throughout the country.

"2. That the names of the proposed Lecturers be selected (with their consent) from amongst the Members of the General Committee of the Association, or from amongst the Graduates of any University in the United Kingdom; and that the subjects upon which the Lectures be delivered shall be such as are included in one or other of the Sections of the Association."

The Committee have drawn up a Report, dealing generally with the subject of their inquiry, which the Council recommend should be referred to the Committee of Recommendations.

The Council have had under consideration the question of enabling Members, who are unable to be present at the Meetings, to obtain the Journal and other printed papers, and they have adopted a Regulation as follows:—

"The Journal, President's Address, and other Printed Papers, issued by the Association during the Annual Meeting, will be forwarded daily to Members and others on application and prepayment of 2*s.* 6*d.* to the Clerk of the Association, on or before the first day of the meeting."

The Council have added the following names of gentlemen, present at the last meeting of the Association, to the list of Corresponding Members, viz.:—His Imperial Majesty the Emperor of the Brazils, Prof. Dr. Colding, Dr. Gussfeldt, Dr. Luroth, Dr. Lutken, Dr. Joseph Szabó.

The following resolution was then agreed to:—"That the members in the following list be constituted, with the president and vice-presidents of the meeting, the past presidents of former years, the trustees, the general and assistant-general secretaries, a Committee of Recommendations, viz.:—Mr. Gassiot, Prof. Henry Smith, Colonel Strange, Prof. Williamson, Mr. Abel, Prof. Martin Duncan, Dr. Burdon Sanderson, Colonel Lane Fox, Prof. Michael Foster, Sir Walter Elliot, Prof. Wyville Thomson, Sir Henry Rawlinson, Mr. Newmarch, Mr. J. G. Fitch, Prof. Hawkins, Mr. Siemens, Mr. Hawkshaw."

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* See NATURE, vol. vi., p. 157.

The incident of greatest interest in Thursday's meetings was when, at the close of Sir John Lubbock's presidential address to Section D, he alluded to the unworthy treatment which Dr. Hooker had received at the hands of a department of the Government, the name of the distinguished Director of Kew bringing down a perfect storm of applause, which was repeated when Dr. Carpenter, in proposing a vote of thanks to Sir John, spoke of the "low and degrading view of science" entertained by a member of Her Majesty's Government. The Section adopted the following resolution:—"That this section would view with regret any change introduced into the botanical establishment at Kew which would tend to affect its completeness, or to impair its scientific character; and that the attention of the Council be called to the subject, with a request that they will take any steps they may deem desirable." This resolution was carried to the Committee of Recommendations, where, however, we regret to say, it halted, and was not allowed to proceed.

The *soirée* on Thursday evening was a highly successful one, and well brought out the capabilities of Brighton and the Brightonians for managing entertainments of this kind. The arrangements, indeed, may be said to have been perfect; and there was a veritable *embarras de richesse* of objects of interest. The magnificent array of microscopes, the splendid Willett collection of the fossils of Sussex, the collection of living flowering plants of the county formed by the Brighton and Sussex Natural History Society, and the pictures in the new museum gallery, were alike objects of attraction. It was universally admitted to be one of the most brilliant gatherings ever held in Brighton.

On Friday Section E was of course the one object of popular attraction. The scenic, not to say dramatic, character of the narrative by which Mr. Stanley preceded the reading of his paper produced a great effect, though probably the discussion which followed was hardly so exciting as the popular expectation had calculated on. Sir Henry Rawlinson's graceful acknowledgment, as President of the Geographical Society, of the sense entertained of Mr. Stanley's energy, and of the substantial value of his services to Dr. Livingstone, met with a hearty response. With regard to the general upshot of the "Livingstone Day," it may be said that while Mr. Stanley's narrative itself carried conviction to the minds of some who were previously sceptical, the manner in which it was delivered was unfortunate in the extreme. In the lamentable episode at the dinner of the Medico-Chirurgical Society on Saturday, when Mr. Stanley abruptly left in consequence of a discourtesy offered him by some of the members, there was undoubtedly fault on both sides, though nothing can justify the want of courtesy exhibited towards a stranger and a guest.

Not more than fifty individuals were collected in the room of Section A, at the Albion Hotel, to hear the most important proposition made at this meeting, by Lieut.-Col. Strange, in the form of a paper entitled, "On the duty of the British Association with respect to the distribution of its funds." The view of the distinguished author of the paper was that which has already been advocated in our columns,* that the Association ought not to grant money in aid of objects which it is the duty of the State to undertake. Such a course, he maintains, only encourages the Government in its present Philistine attitude towards Science; while if a contrary course were pursued, though Science might for a time suffer, a sounder and truer policy would ultimately be forced. In the discussion which followed the reading of the paper, the chief objection started to the proposal was that it will be necessary first of all to raise up a race of statesmen who have received a more complete scientific training, and will consequently have minds more open to the value of scientific research. This argument was forcibly advocated by Prof. H. Smith; to whom Col. Strange replied that, if we are to wait for this, we must be content to be a whole generation behind France and Germany, which countries are both keenly alive to the necessity of the promotion of scientific inquiry.

Saturday was, as usual, a kind of half-day, the brilliant weather inducing all who could possibly avail themselves of the various attractions in the form of excursions, scientific or picturesque, in the neighbourhood.

On Monday Section F was crowded to hear Miss Lydia Becker read her paper "On the attendance and education of girls in the elementary schools of Manchester." The object of the paper was to enforce the necessity for giving equal advantages to girls as to boys in the matter of education; and it was listened to with marked attention, the discussion which followed exciting also great interest. Considerable astonishment was caused by the statement that even the last Revised Code enforced a higher standard for male than for female pupil-teachers. On this the President of the Section, Prof. Fawcett, commented severely, and strongly urged the justice of allowing women to exercise the highest gifts of their nature as freely as men. The discussion proceeding this morning in the same section as Miss Shireff's paper on Female Education, embraces the higher rather than the primary department of the subject, and will be the great feature of to-day's proceedings.

At the meeting of the General Committee held yesterday letters of invitation for the meeting of 1874 were read from Belfast, Glasgow, Bristol, and Bath.

Mr. De La Rue proposed that Belfast should be selected for 1874. Mr. Pengelly seconded, and the resolution was carried unanimously.

Prof. Williamson proposed that Dr. James P. Joule, LL.D., D.C.L., F.R.S., be appointed president-elect of the Association for the meeting at Bradford, which was seconded by Prof. Rankine, supported by Sir W. Thomson, and carried by acclamation.

The next meeting was fixed for September 19, 1873.

The Earl of Rosse, Lord Houghton, Right Hon. W. E. Forster, M.P., Mayor of Bradford, Mr. Gassiot, D.C.L., F.R.S., Prof. Phillips, D.C.L., F.R.S., Mr. T. Hawkshaw, F.R.S., were requested to accept the office of vice-presidents-elect of the Association.

The following alterations were made in the list of the ordinary members of the Council:—Mr. De La Rue, Mr. W. H. Flower, Sir Henry Rawlinson, and Mr. Sclater, were substituted for Prof. Foster, Mr. Gassiot, Mr. Simon, and Mr. Wallace.

Dr. Michael Foster was appointed one of the general secretaries, in the place of Dr. T. Thomson, and Mr. John Ball, F.R.S., Colonel A. Lane Fox, F.G.S., F.S.A., and Mr. Gwyn Jeffreys, F.R.S., were appointed auditors. The other officers were re-elected.

The lecture in the evening by Prof. Clifford, "On the Aims and Instruments of Scientific Thought," was undoubtedly the great intellectual treat of the meeting. It is impossible to give in a few words any idea of the lecture, which we hope to reprint at length. Suffice it to say that it presented some of the most abstruse problems which can form the subject of scientific thought, in a manner so lucid and sparkling as to enchain the audience in rapt attention, notwithstanding that it was unillustrated by a single experiment, like the very beautiful ones exhibited at Mr. Spottiswoode's admirable lecture to working men delivered the previous Saturday evening, of which a report will be found in our columns.

The number of distinguished foreigners attending the meeting is considerably larger than was anticipated. Among those not already named may be mentioned Dr. Hilyard, of the U.S. Coast Survey, Prof. Semper, of Wurtemberg, Prof. Gervais, of Paris, Prof. Gaudrey, of Paris, M. Georges Pouchet, Dr. Anton Dohrn, Prof. Richter, of St. Petersburg, &c.

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* See Nature Vol vi. p 297.

MR. SPOTTISWOODE'S LECTURE TO WORKING MEN ON SUNLIGHT, SEA, AND SKY

THERE are many ways in which men have looked at life; the higher kind of life, that ideal which each of us forms in his own mind to which we each hope that we are always tending. But all these various ideas may for the most part be grouped under two heads: the Ideal of Rest, and the Ideal of Work. "Rest, rest!" said a brave old German worker, "shall I not have Eternity to rest in?" That represents one view. "Work, work!" said another, "must I not work now, that I may the better work in Eternal Life?" That represents the other. But without entering upon the somewhat transcendental question of a future life, these ideas and aspirations have a meaning and reality even in the life which we now live. How do we hope to spend the leisure which old age may some day bring? Or, nearer still, when the day's work is done, and the day itself is not quite spent; or when such holiday as may befall each of us comes round, how do we hope to spend the time? Do we long for mere rest, for that

> land
> In which it seemed always afternoon.

Do we desire to sit us

> down upon the yellow sand
> Between the sun and moon upon the shore,

and sing with the lotus eaters

> All things have rest, why should we toil alone,
> Nor steep our brows in slumber's holy balm,
> Nor hearken what the inner spirit sings.
> There is no joy but calm.

Or do we rather with Ulysses say,

> How dull to pause, to make an end
> To rust unburnished, not to shine in use!
> As tho' to breathe were life Life piled on life
> Were all too little, and of one to me
> Too little remains, but every hour is saved
> From that eternal silence, something more,
> A bringer of new things, and vile it were
> For some [few] suns to store and hoard myself,
> And this gray spirit yearning in desire
> To follow knowledge like a sinking star
> Beyond the utmost bounds of human thought

To which of these two ideals I myself lean has perhaps already betrayed itself; and that being so, I shall venture to consider your presence here a proof that, for this evening at least, you side with me, and that you are willing to spend an hour of your leisure in an intellectual effort to see a little deeper into those phenomena which Nature in this place and at this season displays with such profusion and splendour.

But at the outset I must warn you that we are met by a difficulty, for the surmounting of which you must rely upon yourselves rather than upon me. It is this: the phenomena to which I propose to draw your attention, although taking place nearly every day, and all day long, and in almost every direction, are veiled from our eyes; and it is only by the use of special appliances to aid our eyes that they can be made visible. It will be my business to supply these appliances, and, reproducing on such scale as may be possible within these four walls the optical processes which are going on in the sea and sky outside, to exhibit the hidden phenomena of which I am speaking. But it must be your part to transport yourselves mentally from the mechanism of the lecture-room to the operations of Nature, and by a "scientific use of the imagination" (to adopt what has now become a household word at these meetings) to connect the one with the other.

Now the main point in question is this: that light, when subjected to the very ordinary processes of reflexion from smooth surfaces, such as a window, a mahogany table, or the sea itself, or when scattered to us from the deep clear sky, undergoes in many cases some very peculiar changes, the character and causes of which we have come here to investigate. The principal appliance which will be used to detect the existence of such changes, as well as to examine their nature, consists of this piece of Iceland spar, called—from the man who first constructed a compound block of the kind—a Nicol's prism, and this plate of quartz or rock crystal; both of which, as you will observe when the light passes through them, are clear, transparent, and colourless, and both of which transmit the direct light from the electric lamp with equal facility, however they may be turned round about the beam of light as an axis.

If, however, instead of allowing the beam to fall directly upon the Nicol, we first cause it to be reflected from this plate of glass, we shall find that the process of reflexion has put the light into a new condition. The light is no longer indifferent to the rotation of the Nicol; in one position of the Nicol the light passes as before, but as the instrument is turned round the light gradually fades, and when it is turned through a right angle the light is extinguished. Beyond this position the light reappears, and the same changes of fading and revival are observed in the light for every right angle through which the instrument is turned.

But these phenomena are susceptible of a very beautiful modification by the interposition of this plate of quartz between the reflecting surface and the Nicol. The changes in the light are no longer mere alterations of brightness, but exhibit a succession of colours resembling in their main features those of the rainbow or spectrum.

The peculiar condition to which light must be brought in order that these phenomena may be produced is called polarisation; and although an explanation of its nature must be reserved until later, I beg you to notice that it is effected in this instance by reflexion from a plate of glass A similar effect is produced if light be reflected from many other substances, such as the leaves of trees, particularly ivy, mahogany furniture, windows, shutters, and often roofs of houses, oil paintings, &c., and last, but not least, the surface of water. In each of these cases the alternations of light and darkness are most strongly marked, and the colours (if a quartz plate be used) are most vivid, or, in technical language, the polarisation is most complete, when the light is reflected from each substance at a particular angle. In proportion as the inclination of the light deviates from this angle the colours become fainter, until, when it deviates very greatly, all trace of polarisation at last disappears Without occupying the time necessary to shift our apparatus so as to exhibit this with the glass plate, we may alter the reflecting surface from glass to water, and by projecting on the screen the beautiful phenomena of liquid waves, make visible the different degrees of polarisation produced at the variously inclined portions of the surfaces of those waves. A tea-tray will serve as well as anything else to form our little sea, and a periodic tap at one corner will cause ripple enough for our present purpose. The waves now appear bright on the screen, and although brighter in some parts than in others, they are nowhere entirely dark. But on turning round the Nicol the contrast of light and darkness becomes much stronger than before. Here and there the light is absolutely extinguished; in these parts the polarisation is complete, in others incomplete in various degrees. And if the quartz plate be again introduced we have the beautiful phenomena of iris-coloured rings playing over the surface of our miniature sea.

Now, that which you see here produced by our lamp and teatray, you may see any day under the bright sky of this southern coast. By using an apparatus such as we have here, or a simpler one which I will immediately describe, you may bring out for yourselves these phenomena of colour, and thereby detect the profusion of polarisation which Nature sheds around us. But before describing it, there is one peculiar feature of all these experiments which must be noticed—namely, that the same results would be produced if we changed the positions of the lamp and the screen. The light which is now polarised by the glass or the water, and examined by the Nicol, might equally well be polarised by the Nicol and examined by the glass or the water. And, therefore, if we find that any contrivance will serve for the one purpose, we may conclude that it will serve equally well for the other.

And now a word about that simpler apparatus. When light falls upon a transparent substance, part is reflected, part transmitted. If, therefore, the reflected part is polarised (and you have already seen that this is sometimes the case), it is not surprising that the transmitted part should be so also. And further, if the polarisation by a single reflexion or transmission is incomplete, it will become more and more complete by a repetition of the processes. This being so, if we take a pile of glass plates—say half-a-dozen, more or less, the thinner the better—and hold them obliquely before our eye at an angle of about 30° (say one-third of a right angle) to the direction in which we are looking, we shall have all that is necessary to detect the presence of polarisation; and if, further, we hold a piece of talc or mica, such as is commonly used as a cover to the globes of gas-burners, beyond the pile of plates, colour will be produced in the same general manner as with the quartz, although with some essential difference in detail.

Suppose that we now turn our attention from the sea to the sky, and that on a clear bright day we sweep the heavens with our apparatus, or polariscope, as it is called, we shall find traces of polarisation colours brought out in a great many directions. But if we observe more closely we shall find that the most marked effects are produced in directions at right angles to that of the sun, when, in fact, we are looking across the direction of the solar beams. Thus, if the sun were just rising in the east or setting in the west, the line of most vivid effect would lie on a circle traced over the heavens from north to south. If the sun were in the zenith, or immediately overhead, the most vivid effects would be found round the horizon; while at intermediate hours the circle would shift round at the same rate as the clock, so as always to retain its direction at right angles to that of the sun.

Now, what is it that can produce this effect—or what even produces the light from all parts of a clear sky? The firmament is not a solid sphere or canopy, as was once supposed; it is clear, pure space, with no contents, save a few miles of the atmosphere of our earth, and beyond that the impalpable fluid or ether, as it is called, which is supposed to pervade all space, and to transmit light from the further limits of the stellar universe. But, apart from this ether, which is certainly inoperative to produce the sky appearance as we see it, a very simple experiment will suffice to show that a diffusion, or, as it has been better called, a scattering of light, is due to the presence of small particles in the air. If a beam from the electric lamp, or from the sun if we had it, be allowed to pass the room, its track becomes visible, as is well known by its reflexion from the motes or floating bodies, in fact by the dust in the air. But if we clear the air of dust, as I now do by burning it with a spirit lamp placed underneath, the beam disappears from the parts so cleared, and the space becomes dark. If, therefore, the air were absolutely pure and devoid of matter foreign to it, the azure of the sky would be no longer seen, and the heavens would appear black; the illumination of objects would be strong and glaring on the one side, and on the other their shadows would be deep, and unrelieved by the diffused light to which we are accustomed.

Now, setting aside the dust, of which we may hope that there is but little on the downs behind your town, or out to sea in front, there are always minute particles of water floating in the atmosphere. These vary in size from the great rain drops which fall to earth on a sultry day, through the intermediate forms of mist and of fine fleecy cloud, to the absolutely invisible minuteness of pure aqueous vapour which is present in the brightest of skies. It is these particles which scatter the solar rays, and suffuse the heavens with light. And it is a curious fact, established by Prof. Tyndall while operating with minute traces of gaseous vapours (which I can only notice in passing, because it belongs only in part to our present subject), that while coarse particles scatter rays of every colour equally—in other words, scatter white light—finer particles scatter fewer rays from the red end of the spectrum, while the finest scatter only those from the blue end. And in accordance with this law, clouds are white, clear sky is blue.

But beside this fact, viz., that light scattered laterally from fine particles is blue, the same philosopher perceived that light so scattered is polarised; and by that observation he again connected the celestial phenomena described above with laboratory experiments.

By a slight modification of his experiment, due to Prof. Stokes, I hope to make this visible to the audience. It will probably be in your recollection that when polarised light passed through a Nicol, its intensity is unaltered when the Nicol is in one position, but it is destroyed when it is in another at right angles to the first. I now pass the beam from the electric lamp through a tube of water containing a few drops of mastic dissolved in alcohol. The mixture so formed holds fine particles of mastic in a state of suspension; these scatter the light laterally, so as to be visible, I hope, to the entire audience. And if we were to examine with a Nicol this scattered light, we should find the phenomena of polarisation. But, better still, we can cause the light to pass through the Nicol before being scattered, and produce the same effect, not only upon the particular part to which our eye is directed, but upon the whole body of scattered light. As the Nicol is turned, the light seen laterally begins to fade; and when the instrument has been turned through a right angle, the only parts remaining visible are those which are reflected from the larger impurities floating in the water independently of the mastic. An effect still more beautiful, and at the same time more instructive, can be produced by interposing, as was done in the case of reflexion, a plate of quartz between the Nicol and the medium which causes polarisation. The whole beam is now suffused with colour, the tint of which changes, as did the tints on the waves, while the Nicol is turned round. And not only so, but while the Nicol remains at rest, the tints are to be seen scattered in a regular and definite order in different directions about the sides of the beam. This may be shown by reflecting from a looking-glass a side of the beam not visible directly, and by comparing the tint seen by reflexion with that seen direct. But this radial distribution of colours may also be shown in a more striking manner, by putting together two half-plates of quartz of the kinds which have the property of distributing the colours in opposite orders, and by observing the result along the line of junction. The compound plate here used is known by the name of a biquartz, and affords one of the most delicate tests of the presence of polarised light. In this case, when the Nicol is turned round, the colours of the two halves follow one another in opposite orders; and as each series is completed twice in a revolution of the Nicol, the halves of the quartz will be of the same colour four times in a revolution—twice of one colour and twice of its complementary.

The colours which we have here seen are those which would be observed, as before remarked, upon examining a clear sky in a position at right angles to that of the sun; and the exact tint visible will depend upon the position in which we hold the Nicol, as well as upon that of the sun. Suppose, therefore, we direct our apparatus to that part of the sky which is all day long at right angles to the sun, that is, to the region about the north pole of the heavens (accurately to the north pole at the vernal and autumnal equinox); then, if on the one hand we turn the Nicol round, say in a direction opposite to that of the sun's motion, the colours will change in a definite order; if, on the other, we hold it fixed, and allow the sun to move round, the colours will change in a similar manner. And thus, in the latter case, we might conclude the position of the sun, or, in other words, the time of day, by the colours so shown. This is the principle of Sir Charles Wheatstone's polar clock; one of the few practical applications which this branch of polarisation has yet found. The action of such a clock may be thus roughly shown. There is now projected upon the screen a dial plate, in which the hours are arranged in their usual order, but are crowded together into half their usual space, viz., twelve hours occupy half instead of the entire circle. The inner part of the disc is covered with a plate of selenite (mica would serve the purpose equally well), which is capable of revolving about its centre, and which, as you see, in a particular position shows colour more strongly than in any other. An hour hand is roughly drawn upon the plate. The apparatus here used is furnished with two Nicol's prisms, the hinder one of which imitates the polarising effect of the sun, while that in front is the instrument with which we should examine the north pole of the sky. The whole is now so arranged that when the plate shows brightest colour the hand points to XII., say noon. As the back Nicol is turned round, say as the sun begins to sink, the colour fades; and when the plate is turned so as to restore the colour, the hand points to I. Similarly, as the back Nicol is turned gradually further, representing the passage of the sun westward during the afternoon, the position of the plate giving the strongest colour, as indicated by the hand, corresponds to the successive hours of the dial; and when the Nicol has been turned through 90°, that is, when the sun has reached the horizon, the hand has moved from XII. to VI. In this way, as its inventor has remarked, a dial may be constructed which will work equally well in sunshine or in shade, or even when the sun itself is overcast, provided only that there be a patch of clear sky to the north.

Up to this point we have reproduced in an experimental fashion the general every-day phenomena, both celestial and terrestrial, which give rise to polarisation; and we have given such general account of them as will serve to connect them together, and to show that they all belong to one system of laws affecting the nature of light. I should, however, regret, and I feel confident that you would share in that regret, if we were to leave the subject with its surface as it were merely scratched, and without any attempt to penetrate deeper into its substance. With your permission, therefore, we will devote such time as you may be still willing to grant me to a few elementary experiments in polarisation, which, while certainly not less beautiful than those which you have already seen, will, perhaps, better illustrate the nature of the processes which we are now trying to investigate.

Polarised light, as indicated at the outset, is distinguished from common light by the presence of certain peculiarities not ordinarily found, and these peculiarities are to be detected only by means of special instruments. Light which has been reflected or transmitted at particular angles from various substances, light which has been scattered by small particles, is found to be in this peculiar condition. So likewise is light which has passed through this transparent piece of Iceland spar, or Nicol's prism, as it is called. Yet the light which has so passed through, and which is now projected on the screen, is to the unaided eye in no way different from the same light before its passage. Nevertheless, if we examine or analyse it by means of a second Nicol, we shall find the peculiarity of its condition revealed. For if either of the Nicols be turned gradually round (and remember that they are both transparent colourless blocks of crystal) the light gradually fades until, when it has been turned through a right angle, the light is absolutely extinguished. On turning the Nicol further the light revives, and afterwards again fades, in such a manner that in a complete revolution the light is twice at its brightest, and twice is extinguished. Now, light is due to extremely small and rapid vibrations of a very subtle medium, which is supposed to pervade all space. The fact that vibrations (*i.e.* motions to and fro) in one direction can produce waves advancing in another will be familiar to all of you who have watched the movement of a cork floating on the sea. You will have noticed that the cork has simply moved up and down, or nearly so, while the waves have passed, as it were, under it, along the surface of the water.

Now, in order to make clearer to our minds how this wave motion is produced, I will throw the electric light upon a machine devised for the purpose. You now see a horizontal row of knobs. As the slider is pushed in the knobs at one end begin to rise in succession until each has in turn attained its greatest elevation. Immediately after reaching its highest position it begins to descend; so that the knobs first rise and then fall in regular succession, and continue to rise and fall in the same manner so long as the motion is continued. Each of the knobs, beginning from number one, is thus successively at the highest position, while at the same moment those immediately before and behind it are at lower positions. And as the knob which is at the highest position represents what we call the crest of the wave, the crest will pass successively along all the knobs, beginning from the first. Thus the waves are transmitted along the line, while the vibrations take place across it. If the line of knobs represent the direction of a ray, their motions will represent the vibrations and waves to which the light is supposed to be due. In ordinary light these vibrations may take place in any directions perpendicular to the ray; and the effect of the crystal of which the Nicol is made, is to restrict these vibrations to a particular direction. In the arrangement now before you the first Nicol causes the vibrations to be altogether horizontal. When the second Nicol is placed similarly to the first, it will obviously have no further effect upon the light; but if it be turned through an angle, it will transmit only vibrations inclined to the horizontal at that angle; that is, only such part of the original horizontal vibrations as can be brought into the inclined direction; in other words, it will transmit only part of the light. And as the inclination is increased the part of the light transmitted will diminish, until, when the second Nicol is in a position to transmit only vertical vibrations (*i.e.*, when it has turned through a right angle), the light will vanish. Such is an explanation of this fundamental experiment in polarisation on the principle of what is called the Wave Theory of Light; and I have ventured to give it in some detail, because it is the key to all others, and forms a starting point for any who may desire to go further in the subject; and it is a remarkable feature in this Wave Theory of Light that the results of many other experimental combinations, to some of which we will now proceed, might be predicted upon the principles already laid down.

If a plate of crystal, such as selenite, be placed between the two Nicols, and turned round in its own plane, it will be found that in certain positions at right angles to one another no effect is produced. These may be called neutral positions. In all other positions the field is tinted with colour, which is most brilliant when the plate has been turned through half a right angle from a neutral position. If one of the Nicols be turned, the selenite remaining still, the colour will fade and entirely vanish when the Nicol has turned through half a right angle. After this position the complementary colour will begin to appear, and will be brightest when the Nicol has completed a right angle.

The colours so produced depend upon the thickness of the plate; thus if we take a plate of selenite merely split and not ground to a uniform thickness, we shall have a variety of tints indicating the thickness of each particular part; or we may, by a careful arrangement of suitable thicknesses, produced a coloured pattern of delicacy and variety dependent only upon the skill with which the pieces have been worked.

A plate of the same crystal worked into a concave form is interesting as showing not only that the colours are dependent upon the thickness, but also that when, with an increasing or diminishing thickness of crystal, they have run through their cycle, they begin again; in other words, that the phenomenon is periodic. The field is then covered with a series of concentric rings, each of which is tinted with colours in a regular order.

In all these instances it is clear, from the experiments themselves, as well as from other experiments which form no part of our present subject, that the modifications which light undergoes are due to the internal structure of the crystals used. And it becomes a question of interest whether it be not possible, by some mechanical process, performed upon a non-crystalline substance, such as glass, so far to imitate a crystalline structure as to reproduce some of the optical results already shown. For this purpose let us take a bar of glass. On interposing it in its natural state between the Nicols when crossed, we find that no effect is produced in the dark field upon the screen. If, however, I merely press it as though with the intention of bending or breaking it, there will be at once brought about a condition of strain capable of affecting the vibrations of the ray falling upon it, to such a degree that some of them will find their way through the screen. And this result may be explained on precisely the same mechanical principles as in the case of the crystal. The effect may be heightened by placing the piece of glass in a vice, and screwing it up so as to bend or compress it to a greater degree than was possible by the hand alone. When this is done the direction and even the relative amount of torsion or compression of the different parts will be noted down as it were by the forms and hues of the figures thrown upon the screen.

The same kind of effect is shown by a piece of glass unevenly heated; but better still by glass which has been rapidly and unevenly cooled,—unannealed glass, as it is called. In the pieces now before you, the outside, having become first cooled and solidified, has formed a rigid framework, to which all the interior has been obliged to conform. The interior parts have consequently undergone strains and pressures in different directions and in different degrees, in accordance with which each part has become the subject of a definite internal molecular arrangement; and these, by each in its own way, modifying the light which they transmit, give rise to the figures now before you.

I will conclude this series of experiments by one which, although not so beautiful or striking as those which you have already seen, is still interesting as bringing the subject home to us, and as the only application of polarisation to commercial life which has yet been made. You will recollect the brilliant sequence of colour shown by a quartz plate when submitted to polarised light. Well, the effects produced by that quartz plate are also produced by not only some other crystals, but, what is very remarkable, also by many of their solutions, *e.g.* by that of sugar. Into this tube I have put a solution of sugar; when it is placed before the lamp, polarisation colours are shown on the screen, while the liquid itself remains colourless. If the solution be strengthened by the addition of more sugar, the tints vary; and by accurate observation of the colours for different positions of the Nicol, the strength of the solution may be determined. An instrument constructed with proper means of registering these phenomena with accuracy is called a saccharometer.

These experiments might be multiplied almost indefinitely, and many a long winter evening might be spent in following polarisation into other branches of science upon which it has something to say. For example, on examining a variety of vegetable and animal tissues, slices of wood, fronds of fern, scales of fish, hair, horn, mother of pearl, &c., with a suitable polariscope, we should find that they exhibit, internally, definite structural characters, capable of affecting the light, which they transmit in the same general way as do crystals. Or again, if we were to apply the principles established in an early part of this lecture to the conditions of sky, aspect, and time of day under which the photographer notices that he can obtain the most perfect image in his picture, we should find that they correspond with those which will furnish him with daylight in the most perfectly polarised condition.

Once more, among the many and curious phenomena which are visible during a solar eclipse, there is one which has longer than any other refused to lift its veil to the solicitations of science. I mean that halo of light, or corona as it is called, which extends beyond the dark disc of the moon, beyond those red flames of burning gas which the researches of Lockyer, of Janssen, and of others have brought almost home to us, far away for millions of miles into distant regions of space. It was pre-eminently to investigate this phenomenon that the last Eclipse Expedition, furnished with funds by Her Majesty's Government at the instance of this British Association, was sent out. And upon this investigation all the powers of the twin instruments of modern times, the spectroscope and the polariscope, were turned. The spectroscope could tell us the nature of the substances to the combustion of which the light is due, and even the conditions of temperature and of pressure under which the combustion is taking place; but it could not disentangle those parts of the phenomenon which are due to direct, from those which are due to reflected or to scattered light. It was for the polariscope to tell us whether the corona is a terrestrial effect,—a mere glare, in fact, from our own atmosphere,—or a true solar phenomenon; and in the latter issue, whether any of it is due to direct rays from incandescent matter, or all of it to rays originating in such incandescent matter below, but scattered laterally from gases which have cooled in the upper regions surrounding the sun. This question has not even yet received a definitive answer. But the brief account given within the last few days by Mr. Lockyer, in anticipation of his more complete digest of the voluminous reports from the various branches of the Expedition, seems to justify us in the conclusion that the corona is substantially a solar phenomenon due not to direct but to reflected or scattered rays.

The principle upon which the polariscope enables us to make these refined distinctions in such far off phenomena is after all very simple. If the corona were due wholly to the effect of our atmosphere on such light as reaches us during a total eclipse of the sun, the whole of that light would be similarly affected, because it comes very nearly from the same part of the heavens. In other words, its polarisation would be uniform, and the corona, when examined by a Nicol and quartz, would appear of a uniform colour. But if the phenomenon were wholly due to the sun and its surroundings, the light would be affected, if at all, differently in different directions drawn outwards (like spokes or radii of a wheel) from the sun as a centre. In other words, its polarisation would be arranged spoke-wise, or, to use the technical term, radially; and the corona, when examined as before, would vary in colour on different sides of the sun.

I have already drawn largely, perhaps too largely, upon your patience. But it will not have been without purpose that, besides witnessing the exhibition of a few experiments, you should have seen, at least in outline, what manner of thing a scientific investigation is. Well, whatever it is (and I will not weary you with a dry statement of its processes), the foundation of it must always be laid in careful, accurate, and intelligent observation of facts. And it is a consideration which may well stir the hearts of us outsiders of science, especially on an occasion when we come face to face with some of the greatest philosophers of our time, than any one of us, by practising his eye and riveting his attention, may contribute some natural fact, some fragment of knowledge, to the common stock. And surely has not this a particular significance and importance to us, at a period when, by shortening the hours of labour, more leisure, as we may hope, will be at the command of many? It will, I take it, be our own fault if we spend that leisure in walking through dry places seeking rest; for, to those who have the eyes to see and the spirit to discern, the world is neither dry nor barren; but rather, it is like the mountain as it appeared to the servant of the prophet when his eyes were opened, full of beauty and wonder, of mystery and power,—full of hosts from all nations, striving manfully onward to promised lands of knowledge and of truth, and waging ceaseless warfare against ignorance and prejudice, and the long train of evils which are consequent upon them. And if, as the eventide of life draws on, our eye wax dim, and our step grow weary, so that we can no longer follow, we may still lay us down to rest in some unknown spot, in the full confidence that others will not be wanting to fill our places and gain fresh ground, though we may not live to see it.

SECTION A

Sectional Proceedings

Ad interim Report on the "Results Obtained" by the British Association Eclipse Expedition of 1871, by J. Norman Lockyer, F.R.S.

I. New Instruments

These were as follows:—

1. A train of five prisms to view the corona.
2. A large prism of small angle placed before the object glass of a telescope.

On these instruments I may remark that the Royal Astronomical Society, in the first instance, invited me to take charge of an Expedition to India merely to conduct spectroscopic observations; but although this request did me infinite honour, I declined it; because the spectroscope alone, as it had been used before, was, in my opinion, not competent to deal with all the questions now under discussion. Thus some of the most eminent American observers had come to the conclusion that the spectrum of hydrogen observed in the last eclipse round the sun, to a height of 8 minutes, was a spectrum of hydrogen "far above any possible hydrogen" at the sun. Hence it was in some way reflected. Now with our ordinary spectroscopic methods it was extremely difficult, and one might say impossible, to determine whether the light which the spectroscope analysed was really reflected or not; and that was the whole question.

It became necessary, therefore, in order to give any approach to hopefulness, to proceed in a somewhat different way in the 1871 expedition, with regard to the spectroscope, and, to guard against failure, to supplement such observations with photographs.

To understand the method adopted, let us suppose a train of prisms. Take one prism out of the train, and consider what will happen if we illuminate a slit with a monochromatic light and observe it through the prism. If we render sodium vapour incandescent and illuminate the slit by means of it, we get a bright yellow image of the slit, due to the vapour of the metallic sodium only giving us yellow light. But why is it that we get a line? Because we employ a line slit. If, instead of a straight line, we have a crooked line for the slit, then we see a crooked line through the prism. Going one step further: Suppose that instead of a line, whether straight or crooked, we have a slit in the shape of a ring, we see a ring image through the prism. And then comes this point: If, when we work in the laboratory, we examine these various slits, illuminated by these various vapours, if we observe the corona in the same way, we shall get a ring built up by each ray of light which the corona gives to us; since we know, from the American observations, that there were bright lines in the spectrum of the corona, as observed by a line slit: in other words, the corona examined by means of a long train of prisms should give us an image of itself painted by each ray which the corona is competent to radiate towards us.*

These were the considerations which led to the adoption of this new attempt to investigate the nature of the corona now in question. It was, to use a train of prisms, pure and simple, using the corona as the slit, a large number of prisms being necessary to separate the various rings we hoped to see, by reason of their strong dispersion.

This principle, good for a train of prisms such as I have referred to, is good also for a single prism in front of the object-glass of a telescope. Such was the method adopted by Prof. Respighi, the distinguished Director of the Observatory of the Capitol of Rome, who accompanied the expedition.

This method, if it succeeded, would be superior to the ordinary one in this way. If we were dealing merely with scattered light, then all the rings formed by vapours of equal brilliancy at the base of the chromosphere would be of the same height, while, if such scattering were not at work, the rings would vary according to the actual height of the vapours in the sun's atmosphere.

3. Integrating spectroscopes *driven by clockwork.*
4. A self-registering integrating spectroscope, furnished with telescopes and collimators of large aperture and large prisms. (This instrument was lent by Lord Lindsay.)
5. A polariscope-telescope, so arranged that the same observer could almost simultaneously observe both with the Savart and the Biquartz.
6. A polariscope-telescope, arranged for rapid sweeping round the corona at a given distance from moon's limb.

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* After I had thought of this arrangement, and had secured an instrument to carry it out, Prof. Young, in a communication to Nature, suggested the same method of observation.

II. *The Main Results—Spectroscopic Observations*

It has been established that the idea that we do not get hydrogen above 10 seconds above the sun is erroneous, for we obtained evidence that hydrogen exists to a height of 8 or 10 minutes at least above the sun.

Just as the sun disappeared Prof. Respighi employed the instrument to which I have already referred to determine the materials of which the prominences which were then being eclipsed were composed, and he got the prominences shaped out in red, yellow, and violet light; a background of impure spectrum filling the field; and then as the moon swept over those prominences they became invisible. He saw the impure spectrum and the yellow and violet rings gradually die out, and then three broad rings painted in red, green and blue gradually form in the field of view of his instrument; and as long as the more brilliant prominences on both sides of the sun were invisible he saw these magnificent rings.

These rings were formed by C and F, which show us that hydrogen extends at least 7 minutes high, for had we been dealing with mere glare, *had we not been dealing with hydrogen itself we should have got a yellow ring as well.* In addition to the red ring and the blue and violet, which indicate the spectrum of hydrogen, he saw a bright green ring, much more brilliant than the others due to 1474.

While Prof. Respighi was observing these rings by means of a single prism and a telescope of some four inches aperture, some 300 miles away from him—he was at Poodocottah and I was at Bekul—I had arranged the train of five prisms. My observation was made intermediately, as it were, between the two observations of Prof. Respighi's. The observations may be thus compared:—

Respighi ...	C D^3		F G	Prominence at beginning of eclipse.
Lockyer ...	C	1474	F G	Corona 80 seconds after beginning of totality.
Respighi ...	C	1474	F	Corona mid eclipse.

I had no object-glass to collect light, but I had more prisms to disperse it, so that with me the rings were not so high as those observed by Respighi, because I had not so much light to work with, but such as they were I saw them better because the continuous spectrum was more dispersed, and the rings—the images of the corona—therefore did not overlap. Hence doubtless Respighi missed the violet ring which I saw, but both that and 1474 were very dim, while C shot out with marvellous brilliancy, and D^3 was absent.

These observations thus tend to show, therefore, that instead of the element—the line of which corresponds with 1474—existing alone just above the prominences, the hydrogen accompanies it to what may be termed a great height above the more intensely heated lower levels of the chromosphere, including the prominences in which the lower vapours are thrown a greater height. With a spectroscrope of small dispersion attached to the largest mirror of smallest focus which I could obtain in England, the gaseous nature of the spectrum, as indicated by its *structure*, that is bands of light and darker intervals as distinguished from a continuous spectrum properly so called, was also rendered evident.

Photographs and Structure of Corona

The photographic operations (part of the expense of which was borne by Lord Lindsay) were most satisfactory, and the solar corona was photographed to a greater height than it was observed by the spectroscope, and with details which were not observed in the spectroscope. Mr. Davis was fortunate enough to obtain five photographs of great perfection at Bekul, and Captain Hogg obtained some at Jaffna, but the latter lack in detail. The solar nature of most, if not all, of the corona recorded on the plates is established by the fact that the plates, taken in different places, and both at the beginning and end of totality, closely resemble each other, and much of the exterior detailed structure is a continuation of that observed in the inner portion independently determined by the spectroscope to belong to the sun.

This structure I was also enabled to observe in my 6¼ in. equatorial, even three minutes after totality was over, and we may now say that we know all about the corona, so far as the structure of its lower brighter levels—that portion, namely, which time out of mind has been observed both before and after totality—is concerned. It may be defined as consisting of cool prominences, that is to say, in this region of the corona we will find the same appearances as in prominences, minus the brightness. We find the delicate thread-like filaments which all are now so familiar with in prominences,—the cloudy light masses, the mottling, the nebulous structure, all are absolutely produced in the corona; and I may add that the fainter portion of the ring, some 5 minutes round the sun, reminded me forcibly in parts of the nebula of Orion, and of that surrounding η Argus, as depicted by Sir John Herschel in his Cape observations.

While both in the prism and the 6¼-inch equatorial the corona seemed to form pretty regular rings round the dark moon, of different heights according to the amount of light utilised by the instrument, on the photographic plates the corona, which, as I have before stated, exceeds the limits actually seen in the instrument I have named, has a very irregular, somewhat stellate outline, most marked breaks or rifts *(ignored by the spectroscope)*, occurring near the sun's poles, a fact perhaps connected with the other fact that the most active and most brilliant prominences rarely occur there.

Sketches

From the photographs in which the corona is depicted actinically we pass to the drawings in which it is depicted visually. I would first call attention to two drawings made by Mr. Holiday, who formed part of the expedition, and in whose eye every one who knows him will have every confidence.

First there is a drawing made at the commencement of the totality, and then a drawing made at the end. There is a wonderful difference between these drawings; the corona is in them very much more extensive than it is represented actinically on our plates.

In another drawing, made by Captain Tupman, we have something absolutely different from the photographs and from Mr. Holiday's sketches, inasmuch as we get an infinite number of dark lines and a greater extension than in the photographs, though in the main the shape of the actinic corona is shown.

The corona, as it appeared to me, was nothing but an assemblage of such bright and dark lines; it lacked all the structure of the photographs, and appeared larger; and I have asked myself whether these lines do not in some way depend on the size of the telescope, or on the absence of a telescope. It seems as if observations of the corona with the naked eye, or with a telescope of small power, may give us such lines; but that when we use a telescope of large power, it will give, close to the moon, the structure to which I have referred, and abolish the exterior structure altogether, leaving a ring round the dark body of the moon such as Prof. Respighi and myself saw in our trains of prisms, and I in the 6-inch telescope, in which the light was reduced by high magnification so as to bring the corona to a definite ring some 5 minutes high, while Prof. Respighi, using a 4-in. telescope, brought the corona down to a ring something like 7 minutes high.

Many instances of changing rays, like those seen by Plantamour in 1860, were recorded by observers in whom I have every confidence. One observer noted that the rays revolved and disappeared over the rifts.

Polariscopic Observations

Mr. Lewis, in sweeping round the corona at a distance of some 6' or 7' from the sun's limb, using a pair of compensating quartz wedges as an analyser, which remained parallel to itself while the telescope swept round, observed the bands gradually to change in intensity, then disappear, bands of a complementary character afterwards appearing, thereby indicating radial polarisation.

Dr. Thomson at Bekul saw strong traces of atmospheric, but none of radial polarisation, with a Savart. With the same class of instrument the result obtained by myself was precisely similar, while on turning in the Biquartz, at the top and bottom of the image of the corona, *i.e.*, near the sun's equator, faint traces of radial polarisation were perceptible for a short distance from the moon's limb. Captain Tupman, who observed with the polariscope after totality, announces strong radial polarisation extending to a very considerable distance from the dark moon.

Reversal of Lines at beginning and end of Totality

Captain Maclear, who was observing with me at Bekul, for some time just before the commencement of totality, but when the light of our atmosphere was cut off by the interposition of the dark moon, saw a large number of very fine lines of different heights at the base of the chromosphere.

Mr. Pringle, also at Bekul, saw many lines flash into the field of an analysing spectroscope carried by clockwork at the end of totality.

Captain Fyers, the Surveyor-General of Ceylon, observing with an integrating spectroscope, saw something like a reversal of all the lines at the beginning, but nothing of the kind at the end.

Mr. Fergusson, observing with an instrument of the same kind, saw reversal neither at the beginning nor the end, though during totality he saw more lines than Captain Fyers.

Mr. Moseley states that at the beginning of the eclipse he did not see this reversal of lines. Whether it was visible at the end he could not tell, because at the close the slit had travelled off the edge of the moon.

Prof. Respighi, using no slit whatever, and being under the best conditions for seeing the reversal of the lines, certainly did not see it at the beginning, but he considers he saw it at the end, though about this he is doubtful.

From the foregoing general statement of the observations made on the eclipse of last year, it will be seen that knowledge has been very greatly advanced, and that most important data have been obtained to aid in the discussion of former observations. Further, many of the questions raised by the recent observations make it imperatively necessary that future eclipses should be carefully observed, as periodic changes in the corona may then possibly be found to occur. In these observations the instruments above described should be considered normal, and they should be added to as much as possible.

SECTION D

SUB-SECTION ZOOLOGY AND BOTANY

OPENING ADDRESS BY THE PRESIDENT, SIR JOHN LUBBOCK, BART., M.P., F.R.S.

ALTHOUGH this would not, perhaps, be a fitting opportunity for discussing the importance and best mode of introducing the study of Natural Science into our great public schools, and though the question is still in a far from satisfactory position, yet I think I may congratulate the Section that some progress has been made in that direction during the last few years. To this result the influence of the British Association has no doubt greatly conduced. As yet, indeed, Natural Science is generally taught but to some of the elder boys, and certainly is very far indeed from having its due share of attention in relation to other subjects. I am happy to say, however, that most of the regulations which are being drawn up under the Public Schools Act, by the new governing bodies of the public schools, contain a provision that Natural Science shall be taught to all boys in their passage through the school. As the Royal Society has a representative in the governing bodies of all the public schools, we may fairly hope that this clause will not be allowed to remain a dead letter. I have no reason to suppose that any head-master will oppose the change; but it will of course be necessary for the governing bodies to allot a sufficient amount of funds to this purpose, so as to enable the head-masters to carry out the clause in an efficient manner. In several cases, moreover, and eventually I hope in all public schools, special scholarships and exhibitions will be devoted to Natural Science. When these changes come into full operation, they will doubtless greatly influence the system of education pursued in our preparatory schools. At present, I regret to say, that I know of no private school in England where Natural Science receives the attention it deserves. I must, however, in fairness add, that private schoolmasters are almost compelled to give, not the kind of education which they would themselves prefer, but that which is the most effective preparation for the course of study pursued at the public schools.

The Association has also urged on Government the importance of introducing the elements of Science into the elementary schools of the country, and a deputation from the Council waited on Mr. Forster with this object. In the new code shortly afterwards promulgated, Mr. Forster has admitted the principle, and allotted certain payments to extra subjects, coupled, however, with conditions which, as stated in the report of the Royal Commission on Scientific Instruction, render the promise to a great extent illusory. The subject is no doubt one of great difficulty; but Mr. Forster has distinctly stated that the Government have discarded the idea that the educational functions of Government should be confined to the encouragement of reading, writing, and arithmetic. The experience of some of our best schools, such, for instance, as those of Dean Dawes and Prof. Henslow, show clearly that elementary science can be introduced with the most excellent results, and I rejoice to see that some of our most important School Boards, for instance, those of London and Liverpool, have determined that elementary instruction in science shall be given in all the schools under their control.

If it is said that in such cases but a smattering can be given, I might ask, who has more? Those who are the most advanced in knowledge will be the first to admit how slight that knowledge is.

Indeed, every fresh observation, every new discovery, opens up new lines of inquiry. Take, for instance, the results of Mr. Darwin's great work on the Origin of Species. Mr. Darwin, as almost all biologists would now admit, has thrown a great light on a very interesting and difficult problem; yet in doing so, it suggested various new lines of inquiry, and in removing to a certain extent the veil from our eyes, discovered to us fresh fields for research, which promise most interesting results to those who will study them with diligence.

It is surprising how much, in spite of all that has been written, Mr. Darwin's views are still misunderstood. Thus Browning in one of his recent poems says:—

> That mass man sprung from was a jelly lump
> Once on a time; he kept an after course
> Through fish and insect, reptile, bird, and beast,
> Till he attained to be an ape at last,
> Or last but one.*

Speaking to such an audience as the present, it is unnecessary for me to point out that this is a theory which Mr. Darwin would entirely repudiate, which is utterly inconsistent with his views. Whether fish and insect, reptile, bird, and beast are derived from one original stock or not, they are certainly not links in one sequence. I do not, however, propose on this occasion to discuss the question of Natural Selection. But I may observe that it is one thing to acknowledge that in Natural Selection or the Survival of the Fittest Mr. Darwin has called attention to a *vera causa*, has pointed out the true explanation of certain phenomena: it is quite another thing to assume, as is too often done, that all animals are descended from one primordial source. For my own part, I am quite satisfied that Natural Selection is a true cause. Whatever may be the final result of our present inquiries, whether animated nature is derived from our ancestral source, or from a number of successive creations, the publication of the Origin of Species will not the less have constituted an epoch in the History of Biology.

How far the present condition of living beings is due to Natural Selection; how far, on the other hand, the action of Natural Selection has been modified and checked by other natural laws, by the unalterability of types, by atavism, &c.; how many types of life originally came into being; whether they arose simultaneously or successively,—these and many other similar questions remain to be solved, even if we admit the theory of Natural Selection. All this has, indeed, been clearly pointed out by Mr. Darwin himself, and would not need repetition, but for the careless criticism by which in too many cases the true question has been obscured.

[The remainder of the President's Address was occupied by an enlargement of his views respecting the Origin of Insects,† which we hope to present to our readers at a future time in a complete form.]

SUB-SECTION ANATOMY AND PHYSIOLOGY

OPENING ADDRESS BY THE PRESIDENT, PROF. BURDON SANDERSON, F.R.S.

WE are met here for the purpose of hearing papers on Anatomy and Physiology. It would not have been inappropriate to have given you some account of the limits of the two very distinct sciences which are so designated; but as I am anxious to occupy your time for as short a period as possible, I shall content myself with saying that the few observations I have to make will have reference only to the two sciences to which I am myself attached. I make this preliminary explanation; for the positions of the two sciences in England are so different that much that I may say about Physiology is not applicable to Anatomy.

I should have been glad if it had been possible to have occupied this time in giving you a retrospective account of the

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* Prince Hohenstiel Schwangau, p. 68.

† See Sir John Lubbock's paper in NATURE, vol. v. p. 27.

Progress of *Physiological Research* during the past year. I had intended to do so, but was led to abandon my intentions on the ground that although the work done has not been inconsiderable, we in England have taken very little part in it. If I had attempted the task, I should have been but chronicling the doings of our friends in Germany, who are now holding their own scientific assembly in Leipzig. As I do not wish to talk about German physiologists to-day, I find it more agreeable and more encouraging to look forward than to look back; for although we English physiologists (I say physiologists advisedly, because the anatomist is not in the same position) must admit with regret that we have had very little to do with the unprecedented development of our science during the last two decades, we do not intend to continue in the same inactive condition in future.

Considering that half the purpose of our meeting in this section is to promote the progress of physiology, I do not think I can more properly occupy your time than in endeavouring to show in what direction efforts must be made to improve its position, and particularly to secure a future more fruitful of substantial results than the past has been.

I shall begin by asserting a general principle, which, as I go on, I shall endeavour to justify—that one great reason why physiological research is less successfully pursued in England than we could wish it to be, lies in the general want of scientific education. In illustration of this position, I shall refer first to that higher training which is required for the production of scientific workers or investigators; secondly to what may be called the education of public opinion, by the popularising agency of books and lectures; and lastly to the introduction of Natural Science as an element of education in our great schools and universities.

Training of scientific workers.—If a man wants to be a physiologist, he must, as things at present stand, study medicine. There is no logical reason for this; for, although medicine ought to be built on physiology, there is no reason why a physiologist should know anything about the art of curing diseases. Practically, however, it is the case that the kind of education which a man requires in order to be a physiologist is best obtained through a course of medical study. I confess myself to be of the opinion that this close relation between medicine and physiology is likely to be a permanent one, on the general ground that any science is likely to be studied with more earnestness by those who have to practise an art founded upon it than by others. For example, in England there can be little doubt that it is to our pre-eminence over all countries in the mechanical arts that our exceptionally greater men in the physical sciences on which those arts are built, is due. The reason why the same sort of beneficial reaction of art upon science has not manifested itself in our own sphere is that the connection between the two, *i.e.*, between physiology and medicine, is much less substantial. We physiologists are not yet in a position to advise the doctors, and they, resting on the more reliable teaching of experience, are quite willing to do without us.

If I am right in supposing that the pursuit of physiological research will always be closely connected with medical study, it becomes a matter of interest to us to know in how far the existing institutions for teaching are fitted for the training of scientific men.

We, who are personally concerned in the teaching of medicine, must, I think, admit that, as regards English schools, an ordinary medical course is not a very good preparation for scientific work. The reason of this is that the "medical sciences" as they are called—chemistry, anatomy, and physiology—have developed far too fast for the resources of our schools. Physiology, which twenty years ago might (without very flagrant absurdity) have been called the handmaid of medicine, has become a great science quite independent of the art which brought her into existence. No longer learning from medicine as she used to do, but based entirely on experiment, she claims much closer relationship with the other experimental sciences, and particularly, of course, with physics and chemistry, than with her parent art.

Let us suppose ourselves carried back, say twenty years. Twenty years ago a lecture-room, with a gallery for showing preparations under the microscope, was all that was thought necessary for teaching physiology, even in the best appointed schools; but then how different was that time from the present as regards the position of the science. I can only refer to one or two of the directions in which progress has been made. All that we knew twenty years ago about the gases of the blood was founded on the imperfect methods and erroneous results of Magnus. Take, for example, the exchange of gases in respiration. In 1852 all that we knew on this subject was founded on the imperfect methods and analyses of the physicist Magnus. Now Ludwig and his pupils have put us in possession of a knowledge which for exactitude may be compared with that of the fundamental facts of physics, with methods directly applicable to a number of most important questions. The same physiologist, Ludwig, had lately written his earliest papers on arterial pressure, and has thus by the introduction of new methods inaugurated a new era in the physiology of two mechanical functions. Du Bois Reymond has scarcely begun that series of researches by which he, like Ludwig, rather founded a new science than extended the limits of an old one. In France Brown-Séquard had discovered the functions of vasomotor nerves, and Bernard the glycogenic function of the liver.

Great as was the intrinsic value of all these investigations, it was surpassed by that of the influence which they exercised on the future progress of science. How rapid that progress has been may be readily judged of by any one who chooses to read any of the text-books of twenty years ago in the light of recent researches. With the exception of the somewhat obscure region of what is called animal chemistry, every chapter has been rewritten on the sure basis of direct observation and experiment, the mechanics of the circulation, the chemical changes in the blood and tissues in respiration, the relation between muscular movements and the central organs of the nervous system which preside over them, the electrical changes which go on in nerves and muscles when in and out of action, and in physiological histology, the mode of central and peripheral termination of nerve fibres, and the anatomy of the lymphatic glands and the mode of origin of the absorbent system in the tissues.

In this great progress one would rather not have to admit that Germany has done so large a proportion of the work; for France, notwithstanding her great leaders in science and her great scientific institutions, has accomplished much less than she ought to have done. In taking her part England has been represented by us, the teachers in her medical schools; but we, possessing neither space nor appliances for the prosecution of experimental inquiries, have contented ourselves only too readily to reap the fruits of other men's labours.

It would not be pleasant to make this admission, were it not possible to look forward with considerable confidence to something better. In the great medical schools of London, in the old Universities, and in one or two, at least, of the provincial schools, great efforts are now being made to provide adequate buildings and competent persons for the experimental teaching and study of physiology. It is, I think, a most encouraging sign of the times that the initiative in this movement has been taken by Trinity College, Cambridge. That wealthy corporation, whose very name recalls to our recollection the intellectual glories of our country, has condescended to provide a place for physiologists to study and labour in, from which (short though the time is for which it has existed) one or two valuable researches have already sprung. To what the University of London has done during the last twelve months in establishing a laboratory for inquiries into that most important though comparatively new branch of physiology which relates to the origin and nature of diseases, it is scarcely possible for me to refer, excepting in so far as to express my hope that its influence will eventually be felt in strengthening the hold of physiology on practical medicine.

Notwithstanding these efforts, it will take years to regain the position which we in England once had, and ought never to have lost. The appliances and places for work are now forthcoming, and can be extended as they are required. This is a great step forwards, but we still want the pecuniary resources requisite for carrying out systematic and continuous researches, and above all, we have still to educate workers.

Of the two wants I have mentioned, the want of money and the want of workers, the second is the most important. The difficulties which lie in our way in this respect are very great indeed. The obvious difficulty—the objection, I mean—which is always adduced by young men as a sufficient reason for not giving up their time to scientific research, is that it does not pay; but it need scarcely be said that the real difficulty is a more general one. It lies in that practical tendency of the national mind which leads us Englishmen to underrate or depreciate any kind of knowledge which does not minister directly to personal comfort or advantage, a tendency which was embodied in the philosophy of Bacon, and has been thought by some to constitute its great weakness. I have no doubt there are as many in England as in Germany who would not be deterred by the prospect of comparative poverty, which in every country must be the part of those who devote themselves to abstract science, but very few who have the courage and resolution to follow this course in spite of a public opinion which estimates science on utilitarian principles.

This leads me naturally to my second position, which is that the most efficient means we can take to improve the position of our science in England are those which have for their object the enlightenment of public opinion, and that this is to be effected partly by diffusing this knowledge of our labours among the public, and so inducing them to take an interest in them, partly by introducing training in physical science into our schools.

In the art of exposition, *i.e.*, of making difficult subjects plain, we have one among us who is a master—whose powers in this respect have been acknowledged, not only in England, but in France, and still more emphatically in Germany. His work on elementary physiology has been presented to the German public by one of the leading German physiologists (who is himself a model of clearness of style), who tells his countrymen in his preface that no German writer could expound the experimental facts which are the basis of physiological knowledge as Huxley can.

In the existence of such a man as Huxley I find a great source of encouragement for the future of English physiology, not only on account of his own work, large though that has been (for no one builder can lay many bricks in an edifice where every brick requires such careful laying), but also for his influence on national life.

At one time I confess that I was disposed to underrate the value of popularising science;—now I see the power of exposition to be a great power for good. We have an example of the good that it effects in the history of this Association. We have another in that of the Royal Institution, which has lately been made familiar to us by the accounts which have been given of that great and good man who for so many years was its life. Faraday, the greatest physicist of his time, was equally master of the art of exposition. Of the influence which his mind thereby exercised on the minds of men, women, and children, there can be no doubt. Nor do I think that he lost by it himself, for although we cannot suppose that he taught without some exhaustion of his energies, I cannot believe that the effort was a useless one even to himself.

One would not venture to say of such a man that, in explaining to children the fundamental conceptions which in his mind were already so clear, these became still clearer; but I think it may be so.

I pass at once to the second part of my position, that which relates to the teaching of science, and particularly physiology, in schools. This I may deal with very shortly.

The teaching must necessarily be elementary. If it is thorough and genuine, it is good.

To wedge a little bit of Bowdlerised physiology, something about the structure and functions of the human body, into the ordinary course of a school education, may be an ornamental addition to it, but can scarcely be really useful. Our reform, if it is to be attempted at all, must be much more complete and radical. It must consist, not in adding natural science to the system of instruction in which we ourselves and our predecessors were brought up, but in substituting for some of the old drudgeries something better and more substantial.

As regards that higher education which may be defined as introductory to the studies of the University, most people are now disposed to recognise that there exists at the present day a tendency to increase its extent at the expense of its thoroughness. On the one hand, a powerful effort is made by the *laudatores temporis acti* to maintain the old discipline; while on the other a general though somewhat vague notion prevails that no system of education can be regarded as complete from which science is excluded. To reconcile these antagonistic tendencies, the only method which has been found is that of addition and accumulation. Instead of displacing some of the old requirements, an additional load of new subjects has been imposed on the unfortunate examinee in the form of chemistry, physics, animal physiology, &c. No wonder that to the victim who has just passed through one of our modern ordeals the very names of these sciences are sickening; for in addition to the disagreeable task of getting them up from text-books (text-books, however excellent, are at best but very poor reading), the competitor, whether successful or not, has the consoling reflection that he has been doing treadmill work after all—learning a number of facts and laws of great value to the man who is able to possess himself of them, but to him rendered absolutely useless from the mode of study to which the present system of examinations has compelled him.

The way to obviate this I have already hinted at. Let it be clearly understood that if natural science is to be made a part of our educational system, it cannot be introduced as an ornamental addition or accomplishment, but as part of the groundwork. To serve as a groundwork, we must admit that physiology and anatomy are not adapted.

The corner-stone must, of course, be mathematics. Side by side with mathematics the subjects which ought to claim preference are physics and chemistry. The latter, when taught and studied experimentally, is specially fitted to cultivate that certainty, that convincedness of mind, that clear realisation of facts seen not by the bodily but by the intellectual eye, which constitute the scientific spirit. A boy who has learnt to feel the certainty of the laws of chemical combination, of the relations between density and combining weight, and between both and specific heat, can never, so long as he retains his mental soundness, relapse into that state of vague indifference about facts which characterises many uneducated persons, or lose the habit of exactitude of conception and statement to which he is compelled by practice in chemical reasoning.

It is clear that anatomy and physiology cannot be recommended on the same ground, yet I believe that it may be wisely included in ordinary education, not as a discipline, and not as a subject of examination, but on the ground that it is so usefully applicable to the common affairs of life. It is undoubtedly useful that every one should know something of the structure and functions of his own body, and this for several reasons—first, because he is enabled thereby to take better care of himself, and to understand how to preserve himself by reasonable precautions against some of the well-recognised causes of disease. Another reason is, he would not be so likely as he would otherwise be to become the dupe of the many quackeries which are afloat—more ready to take the advice of the doctor as regards the regulation of his mode of life, less credulous about the efficacy of drugs.

Let us now, in conclusion, say one word as to the influences which the general adoption of a system based upon scientific training would exercise on scientific progress, and particularly on the progress of the science in which we are interested.

I can illustrate this best by taking the medical student as an example. We teachers of physiology to medical students know that when we begin first to talk to them about the principles of the subject, *e.g.*, about chemical change as the essential condition of all vital phenomena, about the relation between the production of heat and external motion, about the exchange of gases in respiration, and many other fundamental subjects, the great difficulty is that our auditors are utterly at fault for want of those conceptions about matter and its powers, which are expressed by the words we are constantly using, such as solid, liquid, gas, vapour, weight, density, volume, &c., all of which to the average-finished schoolboy are perfectly meaningless. The result is that these fundamental conceptions, not having been mastered at first, are not mastered at all, and the student begins to build the superstructure without having had any opportunity of laying the foundation. If the *Vorbildung* were different, if students were to come to their work with the scientific habit of mind already formed, it would not only make them better students, but would retain its influence on him through life. The details might fade from the memory, but the spirit would remain.

I trust that it will not appear to the members of the Section that I have, in any of the observations I have made, forgotten that the object for which we are assembled here is the promotion of the science of anatomy and physiology. Although I cannot claim for our science a more direct interest in scientific training than for others, there are reasons (as I have endeavoured to show) why it suffers more from the want of it than others. The chief one being that, as compared with what we feel and know to be its real importance to the future welfare of humanity, the practical benefits which immediately arise from it are not so obvious.

I have said very little indeed of another pressing difficulty which we have now, and, I believe, will have for many years, to contend with—the want of pecuniary resources—because I know that in this country if educated public opinion can be interested on behalf of any scientific object, and particularly if the intelligent classes of the community can be shown on good ground, that the furtherance of abstract science is a matter of vital importance to our national existence, no one believes that now the really trifling public expenditure which would be required to enable us to compete at least on equal terms with Germany, Austria, Bavaria, and Russia, would at once be forthcoming.

In the meantime it is the function and duty of all who have the means and are interested in scientific progress, and especially of us, the members of this section of the British Association, to afford such aid as we can to those who, supported by their own enthusiasm rather than by the prospect of honour or emolument, are willing to devote their lives to physiological and anatomical researches.

SUB-SECTION ANTHROPOLOGY

Opening Address by the President, Colonel A. Lane Fox

(*Concluded from page* 324)

Amongst the earliest improvements upon the primitive arts of man would be the substitution of the throwing-stick by the bow as a means of accelerating the flight and force of the javelin. So decided an advance in the employment of missile force would lead to the discontinuance of the throwing-stick for ordinary purposes wherever the bow was introduced. The throwing-stick is now found only in distant and unconnected regions, viz., in Australia, and amongst the Esquimaux, and the Furus Furus Indians of South America; and it has been assumed, on account of the isolated positions in which it is found, that it must be indigenous. On the other hand, the use of the bow is almost universal; and it has equally been assumed, on account of its world-wide distribution, that it must be indigenous in different localities, and not derived from a common centre. Geographical distribution, however, although affording the best evidence obtainable, cannot be relied upon with certainty in the case of so early an invention as the bow appears to have been. I cannot concur in thinking that we have any sure evidence that the bow originated in different places; on the contrary, what evidence we have appears to me to be of a contrary tendency.

As by degrees the use of the bow spread over the world, that of the throwing-stick would tend to disappear. We have some grounds for supposing that the latter instrument was formerly in use in the Pelew Islands, and Mr. Franks has found it amongst some Mexican relics probably preserved in a tomb. May it not also have existed formerly in other localities where it has not been preserved in tombs, and where no trace of it now exists? If this were the case, where should we now expect to find it retained? In such localities as the Arctic Seas, where lack of suitable materials still renders the construction of the bow a work of great difficulty, as is shown by the manner in which several pieces of hard bone are sometimes fastened together to form one, or in Australia, where the knowledge of the use of the bow has never penetrated.

Closely connected with the bow, the harpoon may be instanced as an example of early origin and wide distribution. The harpoon is found in some of the French caves, amongst the earliest bone relics of human workmanship that have been brought to light. Its present distribution is almost universal, being found in Australia, North and South Africa, North and South America, and in all regions where its use has not been superseded by more suitable contrivances.

In proportion as our investigations are carried into the higher phases of civilisation, we find our areas of distribution more limited, and of more and more value to us in tracing the continuity of culture; and when we come to the distribution of the metallurgic arts we find them defined by marked geographical boundaries which are not the boundaries of the great primæval races of mankind.

If we draw a line across the globe from Behring Strait in a south-westerly direction through Wallace's line, leaving Australia on the east, and take for our period the date of the first discovery of America, we shall find that—putting aside the metallurgic culture of Mexico and Peru, which, it may be observed, is grouped round a single centre—this line separates the area of stone culture on the east from the area of metallurgic culture on the west, but it passes straight through the primæval racial boundaries.

If we take what we may call the metallurgic area more in detail, and endeavour to trace the distribution of the implements of the bronze period, we find that the same class of weapons and tools extends over a continuous area, including the whole of the northern, western, and central parts of Europe, as far as Siberia on the east: these implements, including palstaves, leaf-shaped swords, and socket celts, with the moulds for casting them, are of a character to prove that the diffusion of the bronze culture throughout this area must have been connected and continuous. In Egypt, Assyria, India, and China, we have also bronze, but the forms of the implements do not, as a rule, correspond to those of the area above mentioned; our knowledge of the bronze weapons of India and China is, however, extremely limited as yet. I have elsewhere given my reasons for believing that the knowledge of the use of iron in Africa must have been derived from a common centre; not only is the mode of working it the same throughout that continent and in India, but the forms of the weapons fabricated in this metal, and especially the corrugated blades, are the same in every part, and appear to have been copied and retained through habit, wherever the use of iron has penetrated. I have lately traced this peculiar form of blade in several parts of the Indian Peninsula and Burmah, and I have no doubt it will eventually be found further to the north, so as to connect the area of its distribution continuously with those of the same identical construction that are found in the Saxon and Frankish graves.

I have thus briefly alluded to the distribution of some of the arts associated with early culture, with the view of showing that as our knowledge increases we may expect to be able to trace many connections of which we are now ignorant, and that we should be careful how we too readily assume, in accordance with the theory which appears popular among anthropologists at the present time, that coincidences in the culture of people in distant regions must invariably have originated independently, because no evidence of communication is observable at the present time. Owing, perhaps, to a praiseworthy desire to refute the arguments of Archbishop Whately, and others who have erroneously, as I think, assumed that because no race of existing savages has been known to elevate itself in the scale of civilisation, therefore, the first steps in culture must have resulted from supernatural revelation, we have now had a run upon the theory of what may be called the spontaneous generation of culture, and the pages of travel have been ransacked to find examples of independent origin and progress in the arts and customs of savage tribes.

Owing to this cause we have, I think, lost sight in a great measure of the important fact which history reveals to us, that, account for it as we may—and it is one of the great problems of Anthropology to account for it if we can—the civilisation of the world has always advanced by means of a leading shoot, and though constantly shifting its area, it has within historic times invariably grouped itself round a single centre, from which the arts have been disseminated into distant lands, or handed down to posterity. In all cases a continuous development must be traced before the problem of origin can be considered solved; the development may have been slow, or it may have been rapid, but the sequence of ideas must have been continuous, and until that sequence is established our knowledge is at fault. As with the distribution of plants, certain soils are favourable to the growth of certain plants, but we do not on that account assume them to be spontaneous offspring of the soil; so certain arts and phases of culture may flourish amongst certain races, or under certain conditions of life. But it is as certain that each art, custom, and institution had its history of natural growth, as it is that each seed which sprouts in the soil once fell from a parent stem. The human intellect is the soil in which the arts and sciences may be said to grow; and this is the only condition of things compatible with the existence of minds capable of adapting external nature, but possessing no power of originality.

If I am right in supposing that it is one of the primary objects of Anthropological Science to trace out the history and sources of human culture, a consideration of the relative value of the various classes of evidence on which we rely for this purpose, will be admitted to be a question of no slight importance in connection with our subject. We must distinguish between those branches of study which we are apt to look upon as intrinsically the highest, and on that account the most attractive, and those which are of most value as evidence of man in a low condition of culture. To the religions, myths, institutions, and language of a people we are naturally drawn, as affording the best indications of their mental endowments; but it is evident that these carry us no farther back in time than the historic period, and however necessary to be studied as branches of our science, they fail to afford us any direct evidence of those vast ages during which our species appears to have gradually taken upon itself the characteristics of humanity. Every age has, however, left us the relics of its material arts, which, when studied comprehensively in connection with the geological record, may be taken as evidence of mental development from the earliest period of time. Nor is it in point of time alone, but also by reason of their stability, that the material arts afford us the surest evidence on which to reconstruct our social edifice. The tendency to constant variation within narrow limits is a psychical characteristic of the uncultivated man; but the material arts are not subject to those comparatively abrupt changes to which, prior to the introduction of writing, all branches of culture are liable which are dependent for their transmission on the memory, and are communicated by word of mouth.

Few who have read the works of Prof. Max Müller or Mr. Farrar can fail to have been struck with the value of the evidence, as far as it goes; but, on the other hand, the very short distance to which it carries us back in investigating the origin of speech; nor is this surprising when it is considered how constant must have been the changes to which language was subject in prehistoric times. Amongst the one hundred islands occupied by the Melanescan race, the Bishop of Wellington informs us there are no less than two hundred languages differing from each other as much as Dutch and German, and this diversity of languages and dialects is confirmed by Mr. Turner in his account of his nineteen years' residence in Polynesia. Amongst the Penons, or savage tribes of Cambodia, Mr. Muhot speaks of the great number of dialects spoken by tribes whose manners and customs are the same. Amongst the Musgu of Central Africa, Barth tells us that, owing to the absence of friendly intercourse between the several tribes and families, such a number of dialects had sprung up as to render communication between them difficult. Upon the River Amazon Mr. Bates mentions that in a single canoe he found several individuals speaking languages so different as to be unintelligible to the others. In a state of culture in which such diversity of tongues existed, what could have been the chance of preserving unchanged the myths, religions, and all those manifestations of intellect which are dependent on tradition?

The greater stability of the material arts as compared with the fluctuations in the language of a people in a state of primæval savagery, is well shown by a consideration of the weapons of the Australians and the names by which they are known in the several parts of that continent. As I have already mentioned, these people, from the simplicity of their arts, afford us the only living examples of what we may presume to have been the characteristics of a primitive people. Their weapons, respecting the distribution of which we have more accurate information than we have of their vocabularies, are the same throughout the continent; the shield, the throwing-stick, the spear, the boomerang, and their other weapons, differ only in being thicker, broader, flatter, or longer in different localities, but whether seen on the east or the west coast, each of these classes of weapons is easily recognised by its form and uses. On the other hand, amongst the innumerable languages and dialects spoken by these people, it would appear that almost every tribe has a different name for the same weapon.

If, then, it is evident that much of the history of our prehistoric ancestors has been for ever lost to us, we may console ourselves with the reflection that in their tools and weapons and other relics of their material arts, the most reliable source of evidence as to their intellectual condition has continued to our time. As to the myths, religions, superstitions, and languages, with which they were associated, we may content ourselves by devoutly thanking Providence that they have not been preserved. As it is, anthropological studies are said to have their fair share in the creation of lunatics, and we can easily believe that no sane intellect would have survived the attempt to unravel such a complex and tangled web of difficulty as the study of these subjects would have presented to our minds.

The consideration of the value of evidence naturally leads us to the third part of my subject, namely, the mode of collecting it and of digesting it after it has been brought together; and as this is, I believe, the most defective part of our organisation—or to speak more properly, the part of our existing institutions in which our want of organisation is most conspicuous—I had intended to have spoken at greater length on this subject; but as I have already trespassed upon your time so long, I am under the necessity of curtailing what I had proposed to say on the subject of organisation. If I am wrong, as I have heard it suggested by some anthropologists, in supposing that the greatest difficulties under which we labour are attributable to the absence of reliable evidence, and if we already possess as much information about savages and about prehistoric men as we require—and we have nothing to do but to read the books in our libraries, and write papers calculated to promote discussions, and fill journals with interesting controversies and speculations—if, as I gravely heard it asserted not long ago at a public meeting, it would be a pity to explore Stonehenge for fear so remarkable a monument should be divested of that mystery which has always attached to it, owing to our entire ignorance as to its origin and uses, then to those who entertain such views the few remarks I shall venture to offer on this subject must appear not only superfluous but mischievous. But if, on the other hand, I am right in supposing that our existing evidence is lamentably deficient, and in many cases false; that it has been collected by travellers, many of whom have had but little knowledge what to look for and observe; and if, this being the state of our knowledge, the evidence which we desire to obtain is now rapidly disappearing from off the face of the earth. The Tasmanians have been swept away before we know anything about them; the New Zealanders and all the Polynesian Islanders are fast changing their habits; and it is now difficult to find a North American Indian in a state of unadulterated savagery; whilst at home our prehistoric monuments are broken up and ploughed down day by day in the construction of buildings and railroads; it is evident that a set of societies which provide no organisation whatever for promoting exploration at home or abroad can only be regarded as fulfilling very imperfectly the functions which institutions established for the purpose of anthropological investigation might reasonably be expected to serve. Beyond the limits of this Association, there is but one society in this country which has the funds necessary for promoting explorations, and that is the Geographical Society. Every expedition which goes out under the auspices of that Society is necessarily brought in contact with the races inhabiting the districts which are explored; but it can hardly be expected that the Geographical Society should do as much as could be desired in the way of promoting anthropological investigation, as long as Anthropology and Ethnology are excluded from the functions of that Society. A Geographical Society should be regarded as the eyes and ears of an Anthropological Society abroad, in the same way that the archæological societies should fulfil the functions of eyes and ears directed to the past history of man, and the most intimate alliance ought to exist between them. A step in the right direction has lately been taken, at the suggestion of Mr. Clements Markham, by the establishment of a joint committee of the Geographical Society and Anthropological Institute, to draw up questions for travellers whom it is proposed to send to the Arctic Seas; and this, it is to be hoped, will be the first step towards a more intimate alliance in the future. As to the Archæological Societies, whose name is legion, and the functions of which are necessarily anthropological in a great degree, they are, as a rule, the most impotent and unprogressive bodies; living from hand to mouth, with funds barely sufficient to maintain a secretary, and to produce a small volume of transactions annually; without the means of promoting exploration, they are dependent entirely upon the casual communications of members, the substance of which is sometimes repeated over and over again in the different societies. If we inquire what useful purpose is served by these divisions of the metropolitan societies, we are told that one is a society, another is an association, and a third is an institute; and yet it does not appear that any one of these societies, associations, or institutes, perform any special function which cannot equally well be served by the others. They constitute divisions of persons rather than divisions of subjects; instead of promoting division of labour they serve only to promote repetition of labour; and so ill do any of them answer the expectations of those who devote themselves to the close study of any one branch of archæology or anthropology, that it has lately become necessary to establish an additional metropolitan society for promoting protohistoric archæology under the title of the Society of Biblical Archæology, embracing subjects which fall mainly within in the domain of anthropology. Much as I should feel disposed to condemn the multiplication of societies under existing circumstances, I cannot but think that by promoting the close study of a particular branch, the establishment of this society is a step in the right direction; and I therefore trust that it may be found to flourish at the expense of those which appear to have no special function to perform. As a prehistoric archæologist, I can only add my humble testimony to that of others who think that this branch of anthropology is very unsatisfactorily dealt with by the metropolitan societies in which it is discussed. Quite recently this happy family has been increased by the birth of a fine child under the title of an Historic Society, and I observe that by way of specialising the functions of this society, it commenced life with a paper on Prehistoric Man. But there are no signs of any limitation to this improvident childbearing; it is announced that a Psychological Society is confidently expected. No one would be more disposed than myself to welcome psychology as a special branch of study if this family of gutter children is to go on increasing *ad libitum;* but it will be admitted that a Psychological Society, of all others, is liable to grow up scatterbrained, if completely severed from the influence of its more experienced kinsfolk.

I trust that I have made it apparent that Anthropology in its various branches includes some of the most popular and widely-disseminated scientific interests of the country; that the loss of power is enormous; not only is there no means of organised exploration, but the information which is published is either repeated over and over again in the different societies, or it is so scattered as to be beyond the reach of the majority of the students. They labour also under the disadvantage of being supported chiefly by men of small means, for the well-to-do classes in this country do not, as a rule, take any interest in either scientific or anthropological investigations. During the past year, a single American has done more in the way of anthropological exploration than the whole of the English societies, institutes, and associations together.

I will now briefly state my views as to the remedies for the evils of which I have spoken. I am averse to the principle of amalgamation; narrow views are often the most pronounced, and if they become dominant are liable to bring down the standard of an amalgamated society instead of enlarging its sphere of usefulness; besides, this amalgamation necessarily entails a certain loss of income by the loss of double subscriptions.

If my experience as a member of the council of most of the societies of which I speak does not deceive me, it should be the object of those who have the progress of anthropological studies at heart to induce the metropolitan societies to specialise their functions. The following might then become the titles of the various societies included under the term Anthropology, and they would represent not only the natural divisions of the science, but practically the divisions which are most consonant with the organisation of the existing societies. Setting history and historic archæology aside as beyond our province, we should have (1) Protohistoric Archæology; I adopt the term proposed by Mr. Hyde Clarke for this branch, which practically includes all that comes under the head of Biblical Archæology at present; (2) Prehistoric Archæology; (3) Philology; (4) Biology, including Psychology and Comparative Anatomy, in so far as it relates to man; (5) Descriptive Ethnology, viz., original reports of travellers on the races of man, conducted in association with geographical exploration. Under these heads we should, I believe, include all the various classes of special workers. These should constitute independent but associated societies; that is to say, the members of one should be privileged to attend the meetings and take part in the discussions of the others, but not to receive the publications of any but their own society. By this means each would profit by the experience of the other societies, but the funds necessary for the maintenance of each would be secured. As branch sections of anthropology they would be under the control of a general elected council, only in so far as would be necessary to prevent their clashing with each other, and for the control of any measures which it might be necessary for the several sections to undertake in concert; under the auspices of the general council might also be held the anthropological meetings devoted to such general subjects as either embraced the whole, or were not included in the sections. By these means the standard of anthropological science as a comprehensive study of the science of man in all its branches would be secured, and the possibility of its becoming narrowed under the influence of any dominant party would be obviated. It is hardly necessary to say that the chief advantage of such an arrangement as I suggest would consist in the employment of a single theatre and library for these cognate societies; they would employ a single printer, and the arrangements might include one or more artists, lithographers, and map-drawers, by which a great increase, and at the same time economy, would be effected in the illustrations. The saving effected by the union of these societies in a single establishment might be applied to conducting explorations either at home or abroad in connection with the Geographical Society. The question of the utilisation of apartments is one which commends itself especially to the notice of Government in regard to those societies, for which apartments are provided at the public cost. It should be made a *sine quâ non* that the societies so favoured should fairly represent all the branches of their subject.

As regards the local societies, it has been proposed to republish a selection of their papers under the auspices of this association. It is to be hoped that some arrangement, such as that proposed by the committee of which Sir Waller Elliot is secretary, may be carried out. I have only one suggestion to make on this point: re-publication is simply a repetition of cost and labour, if the desired object of bringing the papers together can be accomplished by other means. As to selection, I have no faith in it. If local and metropolitan societies could be induced to adopt a uniform size for their publications—not necessarily a uniform type—the papers relating to the same subjects might be brought together without the cost of reprinting. It would only be necessary to establish a classification of papers under various headings, such as, for example, those which constitute the sections of this Association. The societies might then print additional copies of their papers under each heading, in the same manner that additional copies are now struck off for the use of authors. A single metropolitan society might be recognised as the representative of each branch, and under its auspices the whole of the papers of the local and metropolitan societies relating to its branch might be brought together and printed in a single volume. Time does not allow me to enter into the details of the arrangements which would be necessary to carry out such a measure. I believe the difficulties would not be so great as might at first sight appear, especially as the evils of the existing arrangements are much complained of; but it should be a primary object of any arrangement that may hereafter be made that the independence of the several branches should not be sacrificed unnecessarily; it should be endeavoured to stimulate them and train them into useful channels, rather than to bring them too much under central control.

SECTION E

GEOGRAPHY

OPENING ADDRESS BY THE PRESIDENT, FRANCIS GALTON, F.R.S.

THE functions of the several Sections of the British Association differ from those of other Institutions which pursue corresponding branches of science. We, who compose this Section, are not simply a Geographical Society, meeting in a hospitable and important provincial town, but we have a distinct individuality of our own. We have purposes to fulfil which are not easily to be fulfilled elsewhere; and, on the other hand, there are many functions performed by Geographical Societies which we could not attempt without certain failure. Our peculiarities lie in the brief duration of our existence, combined with extraordinary opportunities for ventilating new ideas and plans, and of promoting the success of those that deserve to succeed. We are constituents of a great scientific organisation, which enables us to secure the attention of representatives of all branches of science to any projects in which we are engaged; and if those projects have enough merit to earn their deliberate approval, they are sure of the hearty and powerful support of the whole British Association.

These considerations indicate the class of subjects to which our brief existence may be devoted with most profit. They are such as may lead to a definite proposal being made by the Committee of our Section for the aid of the Association generally; and there are others, of high popular interest, which cannot be thoroughly discussed except by a mixed assemblage, which includes persons who are keen critics, though not pure geographers, and who have some wholesome irreverence to what Lord Bacon would have called "the idols of the Geographical den."

We may congratulate ourselves that many excellent memoirs will be submitted to us, which fulfil one or other of these conditions. They will come before us in due order, and it is needless that I should occupy your attention by imperfect anticipations of them. But I must say that their variety testifies to the abundance of the objects of geographical pursuit, other than exploration. There is no reason to fear that the most interesting occupation of geographers will be gone, when the main features of all the world are known. On the contrary, it is to be desired, in the interests of the living pursuit of our science, that the primary facts should be well ascertained, in order that geographers may have adequate materials, and more leisure to devote themselves to principles and relations. I look forward with eagerness to the growth of Geography as a science, in the usually accepted sense of that word; for its problems are as numerous, as interesting, and as intricate as those of any other. The configuration of every land, its soil, its vegetable covering, its rivers, its climate, its animal and human inhabitants, act and re-act upon one another. It is the highest problem of Geography to analyse their correlations, and to sift the casual from the essential. The more accurately the crude facts are known, the more surely will induction proceed, the further will it go, and, as the analogy of other sciences assures us, the interest of its results will in no way diminish.

As a comparatively simple instance of this, I would mention the mutual effects of climate and vegetation, on which we are at present very imperfectly informed, though I hope we shall learn much that is new and valuable during this meeting. Certain general facts are familiar to us: namely, that rain falling upon a barren country drains away immediately. It ravages the hill-slopes, rushes in torrents over the plains, and rapidly finds its way to the sea, either by rivers or by subterranean water-courses, leaving the land unrefreshed and unproductive. On the other hand, if a mantle of forest be nursed into existence, the effects of each rainfall are far less sudden and transient. The water has to soak through much vegetation and humus before it is free to run over the surface; and, when it does so, the rapidity of its course is checked by the stems of the vegetation. Consequently, the rain-supplies are held back and stored by the action of the forest, and the climate among the trees becomes more equable and humid. We also are familiar with the large differences between the heat-radiating power of the forest and of the desert, also between the amount of their evaporation; but we have no accurate knowledge of any of these data. Still less do we know about the influences of forest and desert on the rate of passage, or upon the horizontality, of the water-laden winds from the sea over the surface of the land: indeed, I am not aware that this subject has ever been considered, although it is an essential element in our problem. If we were thoroughly well informed on the matters about which I have been speaking, we might attempt to calculate the precise difference of climate under such and such conditions of desert and of forest, and the class of experiences whence our data were derived would themselves furnish tests of the correctness of our computations. This will serve as an example of what I consider to be the geographical problems of the future; it is also an instance of the power of man over the phenomena of nature. He is not always a mere looker-on, and a passive recipient of her favours and slights; but he has power, in some degree, to control her processes, even when they are working on the largest scale. The effects of human agency on the aspect of the earth would be noticeable to an observer far removed from it. Even were he as distant as the moon is, he could see them; for the colour of the surface of the land would have greatly varied during historic times, and in some places the quantity and the drift of cloud would have perceptibly changed. It is no trifling fact in the physical geography of the globe, that vast regions to the east of the Mediterranean, and broad tracts to the south of it, should have been changed from a state of verdure to one of aridity, and that immense European forests should have been felled.

We are beginning to look on our heritage of the earth much as a youth might look upon a large ancestral possession, long allowed to run waste, visited recently by him for the first time, whose boundaries he was learning, and whose capabilities he was beginning to appreciate. There are tracts in Africa, Australia, and at the Poles, not yet accessible to geographers, and wonders may be contained in them; but the region of the absolutely unknown is narrowing, and the career of the explorer, though still brilliant, is inevitably coming to an end. The geographical work of the future is to obtain a truer knowledge of the world. I do not mean by accumulating masses of petty details, which subserve no common end, but by just and clear generalisations. We want to know all that constitutes the individuality, so to speak, of every geographical district, and to define and illustrate it in a way easily to be understood; and we have to use that knowledge to show how the efforts of our human race may best conform to the geographical conditions of the stage on which we live and labour.

I trust it will not be thought unprofitable, on an occasion like this, to have paused for a while, looking earnestly towards the future of our science, in order to refresh our eyes with a sight of the distant land to which we are bound, and to satisfy ourselves that our present efforts lead in a right direction.

The work immediately before us is full of details, and now claims your attention. There is much to be done and discussed in this room, and I am chary of wasting time by an address on general topics. It will be more profitable that I should lay before you two projects of my own about certain maps, which it is desirable that others than pure geographers should consider, and on which I shall hope to hear the opinions of my colleagues in the Committee-room of this Section.

They both refer to the Ordnance Maps of this country, and the first of them to the complete series well known to geographers, that are published on the scale of one inch to a mile. It is on these alone that I am about to speak; for, though many of my remarks will be applicable more or less to the other Government map publications, I think it better not to allude to them in direct terms, to avoid distracting attention by qualifications and exceptions.

English geographers are justly proud of these Ordnance Maps of their country, whose accuracy and hill-shading are unsurpassed elsewhere, though the maps do not fulfil, in all particulars, our legitimate desires. I shall not speak here of the absence from the coast-maps of the sea *data*, such as the depth and character of the bed of the sea, its currents and its tides (although these are determined and published by another Department of the Government—namely, the Admiralty), neither shall I speak of the want of a more frequent revision of the sheets, but shall confine myself to what appear to be serious, though easily remediable, defects in the form and manner of their publication. It is much to be regretted that these beautiful and cheap maps are not more accessible. They are rarely to be found even in the principal bookseller's shops of important country towns, and I have never observed one on the bookstall of a railway station. Many educated persons seldom, if ever, see them; they are almost unknown to the middle and lower classes; and thus an important work, made at the expense of the public, is practically unavailable to a large majority of those interested in it, who, when they want a local map, are driven to use a common and inferior one out of those which have the command of the market. I am bound to add that this evil is not peculiar to our country, but is felt almost as strongly abroad, especially in respect to the Government of France. I account for it by two principal reasons. The first is, that the maps are always printed on stiff paper, which makes them cumbrous and unfit for immediate use; it requires large portfolios or drawers to keep them smooth, clean, and in separate sets, and an unusually large table to lay them out side by side, to examine them comfortably, and to select what is wanted. These conditions do not exist on the crowded counter of an ordinary bookseller's shop, where it is impossible to handle them without risk of injury, and without the certainty of incommoding other customers. Moreover, their stiffness and size, even when published in quarter-sheets, make them most inconvenient to the purchaser. Either he has to send them to be mounted in a substantial and therefore costly manner, or he must carry a roll home with him, and cut off the broad ornamental borders, and divide the sheet into compartments suitable for the pocket, which, to say the least, is a troublesome operation to perform with neatness. The other of the two reasons why the maps are rarely offered for sale, is that the agents for their publication are themselves map-makers, and therefore competitors, and it is not to be expected of human nature that they should push the sale of maps adversely, in however small a degree, to their own interests.

The remedy I shall propose for the consideration of the Committee of this Section is, to memorialise Government to cause an issue of the maps to be made in quarter-sheets on thin paper, and to be sold, folded in a pocket-size, like the county maps seen at every railway station, each having a portion of an index-map impressed on its outside, to show its contents and those of the neighbouring sheets, as well as their distinguishing numbers. Also, I would ask that they should be sold at every "Head Post-office" in the United Kingdom. There are about seven hundred of these offices, and each might keep nine adjacent quarter-sheets in stock, the one in which it was situated being the centre of the nine. An index-map of the whole survey might be procurable at each of these post-offices, and, by prepayment, any map not kept in stock might be ordered at any one of them, and received in the ordinary course of the post. This is no large undertaking that I have proposed. The price of a quarter-sheet in its present form, which is more costly than what I ask for, is only sixpence; therefore the single complete set of nine sheets for each office has a value of not more than four shillings and sixpence, and for all the seven hundred Head Post-offices, of not more than 160*l*.

I believe that these simple reforms would be an immense public boon, by enabling any one to buy a beautiful and accurate pocket-map of the district in which he resides, for only sixpence, and without any trouble. They would certainly increase the sale of Government maps to a great extent, and they would cause the sympathies of the people and of their representatives in Parliament to be enlisted on the side of the Survey, and they probably be imitated by Continental nations.

It has often been objected to any attempt to increase the sale of Government maps, that the State ought not to interfere with private enterprise. I confess myself unable to see the applicability of that saying. It would be an argument against making Ordnance maps at all, but the nation has deliberately chosen to undertake that work, on the ground that no private enterprise could accomplish it satisfactorily; and, having done so, I cannot understand why it should restrict the sale of its own work in order to give a fictitious protection to certain individuals, against the interests of the public. It seems to me to be a backward step in political economy, and one that has resulted in our getting, not the beautiful maps for which we, as taxpayers, have paid, but copies or reductions of them, not cheaper than the original, and of very inferior workmanship and accuracy.

So much for the first of the two projects which I propose to bring before the consideration of the committee of this section. It is convenient that I should preface my second one with a few remarks on colour printing, its bearing on the so-called "bird's-eye views," and on its recent application to cartography. Colour-printing is an art which has made great advances in recent years, as may be seen by the specimens struck off in the presence of visitors to the present International Exhibition. One of these receives no less than twenty-four consecutive impressions, each of a different colour from a different stone. This facility of multiplying coloured drawing will probably lead to a closer union than heretofore between geography and art. There is no reason now why "bird's-eye views" of large tracts of country should not be delicately drawn, accurately coloured, and cheaply produced. We all know what a geographical revelation is contained in a clear view from a mountain top, and we also know that there was an immense demand for the curiously coarse bird's-eye views which were published during recent wars, because even such as they are capable of furnishing a more pictorial idea of the geography of a country than any map. It is therefore to be hoped that the art of designing the so-called "bird's-eye views" may become studied, and that real artists should engage in it. Such views of the environs of London would form very interesting, and it might be, very artistic pictures.

The advance of colour-printing has already influenced cartography in foreign countries, and it is right that it should do so, for a black and white map is but a symbol—it can never be a representation—of the many-coloured aspects of Nature. The Governments of Belgium, Russia, Austria, and many other countries, have already issued coloured maps; but none have made further advance than the Dutch, whose maps of Java are printed with apparently more than ten different colours, and succeed in giving a vivid idea of the state of cultivation in that country.

I now beg to direct your attention to the following point:—It is found that the practice of printing maps in more than one colour has an incidental advantage of a most welcome kind, namely, that it admits of easy revision, even of the most beautifully executed maps, for the following reason. The hill-work in which the delicacy of execution lies, is drawn on a separate plate, having perhaps been photographically reduced; this has never to be touched, because the hills are permanent. But it is in the plate which contains nothing else but the road-work where the corrections have to be made, and that is a very simple matter. I understand that the Ordnance Survey Office has favourably considered the propriety of printing at some future time an edition of the one-inch maps on this principle, and at least in two colours—the one for the hills and the other for the roads.

This being stated, I will now proceed to mention my second proposal

Recollecting what I have urged about the feasibility of largely increasing the accessibility and the sale of Government maps, by publishing them in a pocket form and selling them at the Head Post-offices, it seems to me a reasonable question for the committee of this section to consider whether Government might not be memorialised to consider the propriety of undertaking a reduced ordnance map of the country, to serve as an accurate route-map and to fulfil the demand to which the coarse country maps, which are so largely sold, are a sufficient testimony. The scale of the reduced Government map of France corresponds to what I have in view; it is one of five miles to an inch, within a trifle ($\frac{1}{320000}$ of Nature), which is just large enough to show every lane and footpath. Of course it would be a somewhat costly undertaking to make such a map, but much less so than it might, at first sight, appear. Its area would be only twenty-fifth that of the ordinary ordnance map, and the hill-work of the latter might perhaps be photographically reduced and rendered available at once. The desirability of maps such as these, accurately executed and periodically revised, is undoubted, while it seems impossible that private enterprise should supply them except at a prohibitive cost, because private publishers are necessarily saddled with the cost of re-obtaining much of what the Ordnance Survey Office has already in hand for existing purposes. A Government department has unrivalled facilities for obtaining a knowledge of every alteration in roads, paths, and boundaries of commons, and Government also possesses an organised system in the post-offices, fitted to undertake their sale. The production of an accurate route map seems a natural corollary to that of the larger Ordnance maps, and has been considered so by many Continental Governments.

I therefore intend to propose to the committee of this section to consider the propriety of memorialising Government to cause inquiries to be made as to the cost of construction, and the probability of a remunerative sale of maps such as those I have described; and, if the results are satisfactory, to undertake the construction of a reduced Ordnance Map, on the same scale as that of France, to be printed in colours, and frequently revised.

These, then, are the two projects to which I alluded—the one to secure the sale of one-inch Ordnance Maps, on paper folded into a pocket form, to be sold at the Head Post-offices of the United Kingdom—700 or thereabouts in number, each office keeping in stock the maps of the district in which it is situated; and the other to obtain a reduced Ordnance Map of the kingdom, on the scale of about five miles to an inch, to fulfil all the purposes of a road map, and to be sold throughout the country at the post-offices, in the way I have just described.

I will now conclude my Address, having sufficiently taxed your patience, and beg you to join with me in welcoming, with your best attention, the eminent geographers whose communications are about to be submitted to your notice.

Sectional Proceedings—Friday, August 16

Discoveries at the Northern End of Lake Tanganyika, by H. M. Stanley.

THE President, Mr. Galton, in announcing the programme of proceedings for the day, explained the circumstances connected with Dr. Livingstone's discoveries previous to Mr. Stanley's expedition.

Mr. Stanley then read his paper, in which the following are the most important items, omitting everything of merely personal interest:—

"If you will glance at the south eastern shore of the Tanganyika, you will find it a blank; but I must now be permitted to fill it with rivers and streams and marshes and mountain ranges. I must people it with powerful tribes, the Wafipa, Wakawendi, Wakonongo, and Wanyamwezi, more to the south with ferocious Watuta and predatory Warori, and to the north with Mara, Msengi, Wangondo, and Waluriba. Before coming to the Malagarazi, I had to pass through Southern Wavinza Crossing that river, and after a day's march north, I entered Ubha, a broad plain country, extending from Uvinza north to Urundi, and the lands inhabited by the Northern Watuta. Three long marches through Ubha brought me to the beautiful country of Ukaranga, and a steady tramp of twenty miles farther westward brought me to the divisional line between Ukaranga and Ujiji, the Liuche Valley, or Ruche, as Burton has it. Five miles farther westward brought us to the summit of a smooth hilly ridge, and the town of Ujiji embowered in the palms lay at our feet, and beyond was the silver lake, the Tanganyika, and beyond the broad belt of water towered the darkly-purpled mountains of Ugoma and Ukaramba.

"The connection between the Tanganyika and the Albert Nyanza was a subject of interest to all geographers before I went to Central Africa. Livingstone even was almost sure that the Albert Nyanza was no more than a lower Tanganyika, and indeed he had very good reason for believing so. He had perceived a constant flow northward. All the Arabs and natives persisted in declaring that the Rusizi ran out of the Lake Tanganyika. Before I arrived at Ujiji he had never been to the north end of the Tanganyika. As we hugged the coast of Ujiji and Urundi, looking sharply to every little inlet and creek for the outlet that was said to be somewhere in a day's pulling, we would pass by from fifteen to twenty miles of country. It took us ten days' hard pulling to reach the head of the lake, a distance of nearly 100 geographical miles from Ujiji. Two days sufficed for the coast of Ujiji, the remaining eight we were coasting along the bold shores of Urundi, which gradually inclined to the eastward, the western ranges, ever bold and high, looking like a huge blue-black barrier some thirty miles west of us, to all appearances impenetrable and impassable. Only two miles from shore I sounded, and though I let down 620 feet of line I found no bottom. Livingstone sounded when crossing the Tanganyika from the westward, and found no bottom with 1,800 feet of line. The evening before we saw the Rusizi a freedman of Zanzibar was asked which way the river ran—out of the lake or into it? The man swore that he had been on the river but the day before, and that it ran out of the lake. I thought the news too good to be true. I should certainly have preferred that the river ran out of the lake into either the Victoria or the Albert. Livingstone and I resolved, if it flowed into the Victoria Nyanza, to proceed with it to that lake, and then strike south to Unyamyembe, and, if it flowed into the Albert Lake, to proceed into the Albert and cruise all round it, in the hope of meeting Baker. Just after dark we started, and in the morning we arrived at Muighewa, and started for the mouth of the river. In about fifteen minutes we were entering a little bay about a mile wide, and saw before us to the north a dense brake of papyrus and matete cane. Until we were close to this brake we could not detect the slightest opening for a river such as we imagined the Rusizi to be. We followed some canoes which were disappearing mysteriously and suspiciously through some gaps in the dense brake. Pulling boldly up, we found ourselves in what afterwards proved to be the central mouth of the river. All doubt as to what the Rusizi was vanished at once and for ever before that strong brown flood, which tasked our exertions to the utmost as we pulled up. I once doubted, as I seized an oar, that we should ever be able to ascend; but, after a hard quarter of an hour's pulling, the river broadened, and a little higher up we saw it widen into lagoons on either side. The alluvial plain through which the river makes its exit into the lake is about twelve miles wide, and narrows into a point after a length of fifteen miles, or a narrow valley folded in by the eastern and western ranges, which here meet at a distance of a couple of miles. The western range, which inclines to the eastward, halts abruptly, and a portion of it runs sharply north-westward, while the eastern range inclines westward, and after over-lapping the western range shoots off north-westward, where it is lost amid a perfect jumble of mountains. The chief Rubinga, living at Mugihewa, said that the Rusizi rose from the Lake Kivo, a lake fifteen miles in length and about eight in breadth. Kwansibura was the chief of the district in North-eastern Urundi, which gives its name to the lake. Through a gap in a mountain the river Rusizi escaped out of Lake Kivo. On leaving Kivo Lake it is called Kwangeregere; it then runs through the district of Unyambungu, and becomes known as the Rusizi or Lusizi. A day's march from Mugihewa, or say twenty miles north of the mouth, it is joined by the Luanda, or Ruanda, flowing from a north-westerly direction, from which I gather that the river Luanda is called after the name of the country—Ruanda, said to be famous for its copper mines. Besides the Luanda, there are seventeen other streams which contributed to the Rusizi; these are the Mpanda, Karindwa, Wa Kanigi, Kaginissi, Kaburan, Mohira, Niamagana, Nya Kagunda, Ruviro, Rofuba, Kavimvira, Mujove, Ruhuhha, Mukindu, Sange, Rubirizi, Kiriba. Usige, a district of Urundi occupying the head of the lake, extends two marches into the north, or thirty miles; after which comes what is called Urundi Proper for another two days' march; and directly north of that is Ruanda, a very large country, almost equal in size to Urundi. Rubinga had been six days to the northward. There were some in his tribe who had gone farther; but from no one could we obtain any intelligence of a lake or a large body of water, such as the Albert Nyanza, being to the north. Sir Samuel Baker has sketched the lake as being within one degree north of the Tanganyika; but it is obvious that its length is not so great as it is represented, though it might extend thirty or forty miles south of Vacovia. Ruanda, as represented to us by Rubinga, Mokamba, chiefs of Usige, and their elders, is an exceedingly mountainous country with extensive copper mines. It occupies that whole district north of Urundi Proper between Mutumbi on the west and Urundi on the east, and Itara north-east. Of the countries lying north of Ruanda we could obtain no information. West of Urundi is the extreme frontier of Manyuema, which even here has been heard of. In returning to Ujiji, after the satisfactory solution of the river Rusizi, we coasted down the western shore of the Tanganyika, and came to Uvira at noon on the following day. We were shown the sandy beach on which the canoes of Burton and Speke had rested. Above, a little south of this, rises the lofty peak of Sumburizi, fully 4,500 feet above the level of the lake. Mruti, the chief of Uvira, still lives in the village he occupied when Burton and Speke visited his dominions. A day's march, or fifteen miles south of this, Uvira narrows down to the alluvial plains formed by the numerous streams which dash down the slopes of the western range; while the mountainous country is known as Uhembe, the land of the cannibals, who seldom visit the canoes of the traders. South of Uvira is Usansi, peopled by a race extremely cannibalistic in its taste. From Usansi we struck off across the lake, and, rowing all night, at dawn we arrived at a port in Southern Urundi. Three days afterwards we were welcomed by the Arab traders of Ujiji, as we once more set foot on the beach near that bunder. We have thus coasted around the northern half of the Tanganyika, and I might inform you of other tribes who dwell on its shores; but the principal subject of my paper was to show you how we settled that vexed question, "Was the Rusizi an effluent or an influent?" There is, then, nothing more to be said on that point.—In reply to a question from the President, Mr. Stanley said that Burton and Speke halted on a sandy beach just thirteen miles from the extreme end of Tanganyika. Had they gone but half way up the mountain, to the village where resided Amruta, the King of Uveri, they must have seen the northern head of Tanganyika plainly. But in drawing up at this point they simply took the point where the eastern and western ranges meet. The western range halts abruptly; the eastern ranges overlap it. I would not wish for sweeter water than the water of Lake Tanganyika.

Dr. Livingstone's Recent Discoveries, by Colonel J. A. Grant.

The two letters from Dr. Livingstone to Mr. Gordon Bennett, of the *New York Herald*, inform us that he had traced the southern waters from 11° to 5° south latitude, and he supposed they must flow on to the Nile by the Bahr Gazal, at 9° north latitude. I must say that this is an extravagant idea which cannot be entertained, for there are many circumstances precluding such a thing. The distance still unexplored by Dr. Livingstone may be roughly stated as nearly one thousand miles between his most advanced position and the mouth of the Gazal. In this distance we have Speke's Mountains of the Moon, and the great bend (to the west) of the Nile at 7° to 8° north latitude as the principal obstructions to Dr. Livingstone's theory. We also have three hundred miles of longitude between the two positions, but the crowning objection to Dr. Livingstone's waters reaching the Nile is the fact that we already know that the source of the Gazal was visited and determined only a few years ago by the eminent botanist Schweinfurth, who fully satisfied all geographers that the source of the Gazal is about 5° north of the Equator, and not, as Dr. Livingstone supposes, 11° south of it. My observations on the Gazal, made in March 1863, when descending the Nile from Gondokoro with my late companion, show that it is insignificant when compared with the Nile; it seems to be a swamp with little current, for the Nile branch, along which we were sailing, was not increased in width by the water from the Bahr Gazal, the Nile maintained its width of one hundred yards till after the Giraffe and Sooba joined it, then the stream was increased to a breadth of five hundred yards. The Gazal had no perceptible stream; at the junction its waters were still, and looked like a backwater, half a mile across, and surrounded by rushes. Our boatman and others told us that no boats were able to ascend it that year, 1863, as its channel was choked with reeds and the ambatch tree. There is, therefore, no regular traffic on it by boats; some years it is completely blocked, a contrast to the Nile, which is navigable to large dahabieh, all the year round, between Gondokoro and Khartoom. If anything were wanted to prove that the Gazal has no connection with the southern waters of Livingstone, reference might be made to several men who have been in the Gazal country; but Dr. Schweinfurth, who is now in Europe, would be most able to give definite information. The narrative of Dr. Livingstone contains some curious incidents which are quite novel to me, for, in our journey from Zanzibar to Egypt, when travelling on the watershed of the Nile, we never saw any race of cannibals, any signs of gorilla—neither did we find that any race of natives ever kept pigs in a domesticated state; they eat one species of wild hog, but no race in the valley of the Nile was ever seen to keep pigs tame. Oysters must be a misprint. Taking into consideration these remarkable differences from the country we traversed, I cannot but think that Dr. Livingstone, having no chronometers to fix his longitude, has got farther to the west than he supposes, and that he had got amongst races similar in most respects to those on the west coast of Africa, described by M. Du Chaillu. In conclusion, this fresh discovery of lakes and rivers by Livingstone has defined a distinct new basin, and left clearer than ever the position given by Speke to the Nile in 1863. Besides the two despatches to Mr. Gordon Bennett, we have now seen Dr. Livingstone's letters to Lords Stanley, Clarendon, and Granville, dated between 1870 and 1872. It is much to be regretted that they contain so little mention of latitude, longitude, and altitude. for his vast discoveries of new country cannot be laid down by our map makers with any degree of certainty—indeed, no two men could make a similar map out of all the geography he has forwarded. He informs us that his drainage, from 12° south latitude, has been traced by him up to 4° south latitude, and that he believes these waters continue to flow north, and from the valley of the Nile by joining the Nile of Speke at 9° north latitude by the Bahr el Gazal. No such thing can happen, for we have ample evidence, from independent sources, against the doctor's theory, besides which there are curious circumstances in his letters forbidding any connection with the Valley of the Nile. Livingstone tells us that the natives keep pigs, and that he had met with the skull of a gorilla. This shows a region distinct from the Nile races and the Nile animals, for nowhere on our route did we meet with pigs domesticated, or gorilla in the forests. Though this is but circumstantial evidence, it appears strong to me, and it also appears that he must have been farther west in his longitude than he supposed.

Sir Samuel Baker states that the Nile receives the following rivers from the west, namely, "The Yé, third class, full from 15th April to 15th November, also another smaller river, third class, full from 15th April till 15th November," and "the Bahr el Gazal, little or no water supplied by this river." Having seen this river in March of the same year, I can testify to the correctness of Sir Samuel's description, from the notes made by me upon it. These notes state the Bahr Gazal gave little or no water to the Nile, which was not increased much in size after its junction. The Bahr Gazal was still water, and the Nile flowed past it at the rate of about two miles per hour. It must be remembered also that this was only one branch of the Nile; the other, the Bahr Giraffe, supplied half as much again of water, and flowed at the rate of four miles an hour where it joined the Nile. Another fact worth noting is, that the water of the Bahr Gazal is described as being clear by Sir Samuel—this implies that the water had settled, that it was still water, and that there can be no current, but if additional proof was necessary to show that Dr. Livingstone's waters from 12° south latitude do not join the Nile here, we have the crowning evidence—which Dr. Livingstone is not aware of—of the German traveller, Dr. Schweinfurth, who determined the source of this Bahr Gazal at 3° or 5° north latitude. Dr. Livingstone has been informed by natives that Speke's Victoria Nyanza consists of three or four lakes, the Okara, Kavirondo, the Naibash, and the Baringo; but we know from Speke's map and from his writings, that the Okaro is the Ukereweh lake, the Naibash is the Naivasha, on the east of his lake, and that the Baringo is to the north-east of his lake. Captain Speke tells us that he had seen quite half of the lake, as laid down by him, his longitudes and latitudes and altitudes gave him its general outline, and where he could not obtain those he had to content himself with native information. We, therefore, think that his information is perfectly reliable, and that Dr. Livingstone has been misinformed—at all events, the Victoria Nyanza discharges quite sufficient water to form a navigable river from the Ripon Falls to the Mediterranean, and no stream which joins it in its course can for a moment be compared to it in size. Dr. Livingstone makes a mistake as to its size; he calls it eighty to ninety yards wide, writes of it as the "little river," and I cannot conceive where he got his information. Speke's estimate makes it a width of 150 yards—not feet—across the actual waterfall, and immediately above this, he tells us that it is 300 yards wide. From this we turn to the Tanganyika Lake. Unfortunately, its altitude is not mentioned, so we must take it for granted that Speke's measurement of 1,800 feet was correct; but Dr. Livingstone tells us that it has some influence on the Nile. It is tantalising to be told this bare statement, which leads us to conclude that it runs into his line of drainage, and not into the Indian Ocean—as I suppose it does. However, if life and health be spared to the doctor he will determine this point when he has visited the southern end of this lake; and it is to be hoped that he will send us despatches with more frequency, as there is nothing to prevent his doing so by the hands of the numerous traders travelling between Zanzibar and Ujiji.

Consul Petherick was then called on by the President to give an account of the Bahr el Gazal river, the great tributary of the Nile discovered by him.

Dr. Beke then said it was by no means pleasant for him to have to recant the opinions which he had so long maintained, but he was perfectly convinced that Livingstone had not discovered the sources of the Nile. Capt. Speke made Tanganyika 1,700 ft., Baker made the Albert Nyanza 2,700 ft., or, as it had been corrected, 2,500 ft. Even making an allowance of 200 ft. or 300 ft., it seemed impossible, on account of the levels, that the river Lualaba should flow into either of those lakes. He concluded that the Lualaba must either go into the Ulle or into some lake, or turn round to the Congo. He did not, however, think it went to the Congo, owing to the ascertained levels. It was a mortifying thing to have to acknowledge that what he had so long contended for was wrong, but the facts which had previously been made known led to the inevitable conclusion that what Livingstone had discovered was not the source of the Nile.

Sir H. Rawlinson said he was glad to have this opportunity of bearing testimony to the great value which the Royal Geographical Society attached to Mr. Stanley's services, and also of expressing the high opinion they entertained of his merit as a traveller. Livingstone had, no doubt, achieved a great geographical success in discovering the great interior system of river beds, but from his letters it was evident that almost to the very last he had strong misgivings about his being upon the Nile basin. Over and over again he said it had occurred to him that he might have been on the Congo. What did really become of this great river system which he had discovered it was impossible to say authoritatively. All that could be said was, that it was a completely new discovery, but he trusted that Livingstone himself would be the discoverer of where those great central rivers run, for he should be sorry if he did not carry out to a successful issue the great work upon which he had been so long, and so honourably, and so conscientiously engaged. In a matter of this sort all must be conjecture, but putting all the arguments side by side, he confessed to the supposition that this great river system fell into a large central inland lake. He should very much doubt its ever reaching Lake Chad, but there was a very large space in the interior of the continent which might very well be occupied by such a river-stream draining all the surrounding mountains. The discovery of the lake (if there be such a lake) into which the central waters ran, would, he trusted, be the crowning success of Livingstone's African travels. He had announced that he was going to the source of those waters, and when he had satisfied himself of that, he would return northward, and with the supplies furnished him in a great measure by Mr. Stanley, he would be then in a position to follow the system beyond a point where he was previously arrested. His great difficulty on former occasions was owing to the incapacity and hostility of his attendants, but it was most gratifying to know that he had now at his command a faithful and efficient body of followers.

Mr. Clements Markham asked Mr. Stanley whether the country of Balegga, which Livingstone alluded to, was the same as that laid down in Sir Samuel Baker's map as Malegga. Baker gave the name of Malegga to a range of mountains, and Livingstone spoke of it as a mountainous country.

Mr. Stanley said he believed the two names referred to the same region. No importance could be attached to difference in spelling, as it was purely a matter of hearing. To one traveller the word might appear to be pronounced one way, and to another traveller another. It was not even a difference of dialect. It was simply a difference in the spelling owing to the difference in the hearing.

The President, in closing the discussion, said that there was not the slightest doubt that Dr. Livingstone could only draw inferences as to where the waters he saw found an outlet. It was entirely a matter of theory. Geographers dealt with the facts that he had made known to them, and from those facts they drew their conclusions, just as Dr. Livingstone, from the same facts, drew his conclusions.

SCIENTIFIC SERIALS

THE *Monatsbericht der k. preussischen Akademie der Wissenschaften zu Berlin*, from January to March, 1872. The most numerous scientific papers in these three numbers of the Berlin *Monatsbericht* are of a chemical nature, consisting of notes by Prof. Hofmann on aromatic phosphines, on the products of oxidation of methylphosphine and ethylphosphine, and on derivatives of the ethylene-bases, and a paper by Prof. Rammelsberg on the chemical composition of ambygonite. Prof. von Rath gives an account of a meteorite which fell at Ibbenbièhren, in Westphalia, on June 17, 1870, and consists chiefly of silica, protoxide of iron, and magnesia, with small quantities of protoxide of manganese, lime, and allumina. Structurally it consists of a granular mass, containing imbedded crystaline grains, the composition of both being mainly identical. Prof. H. A. Schwarz, of Zurich, communicated a continuation of his investigations upon special mineral surfaces. Prof. Riess has a paper on the reaction of the induced currents in an unaltered circuit upon the main current of the Leyden battery. Prof. Helmholtz communicates a summary of the results obtained by Dr. W. Dolerowolsky, of St. Petersburg, by experiments upon the sensibility of the eye to differences in the luminous intensity of various spectral colours The natural history papers consist of a notice by Prof. Ehrenberg of Prof. Whitney's recent investigations of the Californian Bacillarian rocks, a paper by Dr. Roth on the geology of the Philippian islands, and two geographical memoirs by Prof. W. Peters, one giving the synonyms of the species of the chiropterous genus *Megaderma*, and describing a new form (*M. cor* Peters, from Abyssinia), the other discussing in considerable detail the species of Batrachia, collected by Spix in Brazil, after an inspection of the original specimens in the museum at Munich.

Journal of the Chemical Society, June 1872.—This number opens with a lecture on the "Chemistry of the Hydrocarbons" by C. Schorlemmer, which is a condensed summary of the history of this subject. In the opening part of this paper the definitions of organic chemistry are discussed, Mr. Schorlemmer preferring to define it as "the chemistry of the hydrocarbons and their derivatives." The lecturer then proceeds to show how far our knowledge of the constitution of the hydrocarbons has advanced. The chemistry of the paraffin series of hydrocarbons is first entered upon; these perhaps have been more completely studied than any of the succeeding series. All the paraffins of known structure may be divided into four groups; the first or normal paraffins have been to a great extent worked out by the author, and are those in which each carbon atom is directly combined with at least two other carbon atoms forming a symmetrical chain. The other three groups have not been studied completely, and are not here discussed. By abstracting two atoms of hydrogen from the paraffins a series of hydrocarbons is obtained called the olefines. The probable constitution is not, as was at one time supposed, that they have carbon atoms with free combining units, thus—$CH_2 - CH_2$—; but that one carbon atom is linked by two combining units to another carbon atom, thus $CH_2 = CH_2$. The hydrocarbons of the acetylene series were next introduced; these are formed by again abstracting two other atoms of hydrogen from the olefines; in acetylene, for instance, it is probable that the carbon atoms may be linked together by three combining units of each atom, thus $CH \equiv CH$. The aromatic hydrocarbons have been very much worked on during the last few years. The present theory of their constitution is due to Kekulé, who supposes that the six carbon atoms are united together in a sort of chain or hexagon by one and two combining units alternately, which would then leave six combining units unsaturated. These when combined or saturated with hydrogen will yield the hydrocarbon benzol, which may be considered as the starting point of the aromatic series. The differences observed in certain groups of isomeric aromatic compounds may be accounted for by the supposition that the different relative positions of certain elements or radicals as attached to the carbon nucleus cause a difference in the physical condition of the substance. The constitution of various other hydrocarbons, such as naphthaline, anthracene, &c., is discussed; but they are too complex to be here described in detail.—E. A. Letts has prepared benzyl isocyanate by the action of the benzyl chloride on argentic cyanate, benzyl isocyanurate being also formed during the reaction. By the action of ammonia on the former body monobenzyl urea is formed, which by treatment with water yields dibenzyl urea. By substituting aniline in the place of ammonia in the last-named reaction phenyl benzyl urea is produced.—T. E. Thorpe contributes some laboratory notes on various subjects, and Prof. Himly a paper "On the estimation of carbonic acid in sea water," the method recommended is to precipitate the carbonic acid by baryta water, and to estimate the baric carbonate produced, it having been found that simply boiling the water is insufficient to drive off the whole of the carbonic anhydride. A detailed description of an apparatus for the collection of sea water below the surface is promised, which is also to be provided with the means of adding the reagent below the surface of the sea so as to avoid any loss of carbonic anhydride during and after the collection of the sample. The abstracts which follow the paper are as usual full of interest, containing as they do the pith and substance of many important papers.

BOOKS RECEIVED

ENGLISH.—The Graft Theory of Disease: Jas Ross (J. and A. Churchill). Magnetism and Deviation of the Compass: Merrifield (Longmans).—A Handbook of Chemical Technology: R. Wagner (Churchill).

FOREIGN.—Forstzoologie: Dr. B. Altum—Nomenclatur Botanicus, Vol. i., Nos. 1—6: L Pfeiffer.—Anatomische Untersuchungen: G. Retzius.—Aufgaben aus der analytischen Mechanik, Nos. 1, 2: Dr. A. Zuhrmann.—Theorie der Bewegung der Kräfte, ein Lehrbüch der theoretischen Mechanik. Dr W. Schell.—Faune des Vertébrés de la Suisse: Dr. V. Fatio.

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ERRATA—Vol vi, p. 300, 1st col, line 2 from bottom, for "this" read "the," 2nd col., line 10 from bottom, for "Fig 4" read "Fig. 1;" last line, for "Fig 5" read "Fig. 2," p. 301, 1st col, line 12 from bottom, for "Fig 6" read "Fig 3;" p. 302, 1st col, line 28, for "Fig. 1" read "Fig. 4," line 39, for "Fig 2" read "Fig. 5." line 45, for "Fig. 3" read "Fig. 6;" 2nd col, line 22 from bottom, for "modified" read "modifying;" p. 303, 2nd col, line 13, for "the" read "this."

THE BRITISH ASSOCIATION.—*Authors of papers are requested to favour the Editor of* NATURE *with copies or abstracts of their communications as soon as possible, as by these means alone can an accurate and early notice be insured. The Editor appeals to men of science to aid him in his attempt to give an account of the results of their investigations to their brethren throughout the world.*

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, AUGUST 29, 1872

SCHELLEN'S SPECTRUM ANALYSIS

Spectrum Analysis in its Application to Terrestrial Substances, and the Physical Constitution of the Heavenly Bodies. By Dr. H. Schellen, Director der Realschule, I.O. Cologne. Translated from the second enlarged and revised German edition by Jane and Caroline Lassell, edited, with notes, by W. Huggins, LL.D., D.C.L., F.R.S. (Longman and Co., 1872.)

IT is not difficult to deliver interesting lectures or to write an instructive book on spectrum analysis. The rapid succession of brilliant discoveries in this new branch of science, the amount of fundamental facts added by it to human knowledge, especially in the field of the cosmical world, assure the lecturer or writer appealing to the intelligent but not scientific public of useful and legitimate success. But what is not so easy to do is to interest at the same time the *gens du monde* and scientific men, by offering a selection of the most recent discoveries in a bright and literary form attractive to the former, and yet keeping for the latter the appearance of precision, and exactness of the numerical results.

All these conditions are very happily filled in "Schellen's Spectrum Analysis," edited by Mr. W. Huggins from the second German edition. I shall commence by giving a brief account of the chief points of the book.

The first part, introductory, is occupied by a description of the artificial sources of high degrees of heat and light, of which the study is so intimately connected with the chemical and astronomical phenomena embraced in the field of spectrum analysis; various apparatus, for instance, the gas-burner, the magnesium lamp, the Drummond lime-light, the electric spark of the induction coil, the Geissler's tube, and the electric light produced by voltaic batteries are described, and the practical adjustments are briefly but sufficiently referred to for a good understanding of the subject.

The second part is devoted to an elementary abstract of the geometrical and mechanical properties of light. The fundamental analogy between light and sound is developed, in order to explain to a reader unlearned in optics how the colour of a ray is the corresponding element of the pitch of a musical sound, and how it is possible to define a coloured ray by the time of its luminous vibrations. The description of refraction phenomena, especially the paths of rays through a prism, leads naturally to the separating process of the different colours on which spectrum analysis is founded.

Some examples of such an analysis of light by means of a prism are given, amongst which we may mention the screen-projections of the spectra of the electric, sun, or magnesium light; a sufficient number of illustrations enables every one easily to repeat the experiments.

A considerable number of chapters is devoted to the construction of the simple and compound spectroscope. The chief points of this construction, especially the contrivances for the simultaneous comparison of two spectra, the determination of the position of lines in the spectrum are carefully described. Afterwards a practical account of the methods for exhibiting spectra of terrestrial substances, for instance, metallic salts volatilised in a gas-burner, &c., will certainly interest chemists.

The beautiful appearance in the spectroscope of heated gases in Geissler's tubes, their bright lines, and also the important question concerning the change of spectra with temperature and pressure, all these subjects are sketched in general outlines; it is nevertheless to be regretted that an account of the beautiful experiments of MM. Frankland and Lockyer have not a place amongst these descriptions; the difficult problems raised by these experiments are not completely solved, and we by no means can accept every assertion developed in this interesting chapter.

The curious absorption phenomena to be observed in the spectrum analysis of light which has passed through certain liquids, especially of organic origin, deserved a peculiar notice; the author has not neglected to describe one of the most remarkable spectra—the absorption-bands of the blood, and to indicate what advantage natural history is able to derive from such observations even on microscopical objects.

An interesting chapter contains the theoretical and experimental explanation of the reversal of the spectra of gaseous substances. This phenomenon, studied independently by Foucault and Angström, and definitely generalised by Kirchhoff, is perhaps the chief point of the history of spectrum analysis, and certainly the beginning of its utilisation as a powerful method of investigation.

The only practical example of reversal given in the book is that of sodium vapour; but recent experiments have proved that nearly all metallic vapours heated conveniently in the voltaic arc show the reversal of a great number of bright lines into black ones.

The third part of the book, the most important in extent and results, is devoted to the application of the spectrum analysis to the heavenly bodies.

The sunlight, according to its brightness and to the peculiarities of its spectrum, is the best and easiest example to study. The dark lines in infinite number which it shows, called "Frauenhofer lines," from the discoverer, deserve special attention; therefore the author has illustrated the description of the sun-spectrum with two sets of maps. The first is a reduction of Kirchhoff's maps engraved on wood, representing in several tints the lines from *A* to *G*; the second series is a reduction to about half size of the admirable *normal solar spectrum* of Angström, in which the Frauenhofer lines from *a* to H_1H_2 are co-ordinated according to their wave-lengths. The accuracy of these lithographic plates is really wonderful; they will have the great merit of introducing amongst physicists and astronomers the wave-length scale for the designation of lines instead of Kirchhoff's scale, which is an arbitrary one; and in any case they will facilitate the transformation of the data from one to another. I must add that Angström's maps have been introduced into the present edition by the English editor, and that such an addition is certainly one of the greatest attractions of this book for scientific men.

A good abstract of Kirchhoff's and Angström's memoirs on the coincidence of the dark solar lines with the bright lines of metallic vapours leads to the hypothetical constitution of the sun; this problem is so difficult, that it is necessary to leave to every one the responsibility of his own ideas on this subject. I ask, then, for permission to decline any critical notice of this part of the book.

I must mention also a useful description, illustrated with maps, of the telluric and atmospheric lines from the works of Brewster, Gladstone, Angström, and Janssen.

The remaining part of the book is entirely devoted to the most delicate applications of spectrum analysis to astronomy. A preliminary description of the sun-spots, faculæ, and other peculiarities of the surface of the sun, of the prominences round the disc, and so on, is given before the spectroscopic process for analysing these appearances is introduced, and enables the reader to understand very well the difficulties of the problem and the interest of its solution. I must mention especially the interesting account of the three total solar eclipses of 1868, 1869, 1870. A large series of drawings and photographic *fac similes* give the best idea of the phenomena, and show the improvements due to photography and spectroscopy; the relatively great extent devoted to this account is justified by the importance of the subject; the spectrum analysis of the prominences is in fact one of the most considerable results obtained for a long time in the sciences of cosmogony.

Now from this discovery of Janssen's it is easy to observe every day the solar prominences by utilising the bright lines of their spectrum. Janssen's method, discovered in India soon after the eclipse of 1868, was independently discovered again some weeks after by Lockyer, who has the real merit of announcing two years before the possibility of such an important observation, and would very likely have had the honour of priority if he had had beforehand the material means of carrying out his designs.

Schellen's book contains a complete account of the improved telespectroscopes of Lockyer, Respighi, Secchi, Huggins, Janssen, Young and Zöllner, and a beautiful series of coloured sketches, representing some daily observations of prominences all varying, but truly characteristic of their form. I must confess, however, that some of those beautiful pictures seem to me rather too much embellished by the imaginative fancy of one of the observers. The sun-spots and faculæ observed with a telespectroscope give a good number of new facts which have led Lockyer and Secchi to the most important inductions upon the constitution of the sun.

The spectroscope, as it is known, is able to give an exact measurement of the proper velocity of the luminous bodies. A German physicist, Doppler, deserves to be mentioned as the first who called the attention of astronomers to this subject, though a good number of his assertions may be incorrect. After him, Fizeau, a French physicist, to whom we are indebted for the first determinations of the velocity of light on the surface of the earth, showed the errors of Doppler in a little paper not very well known, published in 1849 ("Bulletin de la Société Philomatique de Paris"), and calculated the apparent change of refrangibility which would be produced by the proper motion of some heavenly bodies; but no direct experiment was made before the complete application of spectrum analysis to the sidereal phenomena. In this way Schellen's book gives a good abstract of the works of Huggins and Secchi. In these researches the velocity of rotation of the sun was to be tested as a verification of the general law of the phenomenon. I ought to say, that, the rather discordant results want a theoretical analysis, because the problem seems to me, in the case of the sun, more complicated than it appears at first sight. However, the influence of the velocity of the gas streams, especially of hydrogen, which constitute the greater part of the prominences, is unquestionably verified by Lockyer's observations. In the same way Huggins has proved and determined the proper motion of Sirius by the apparent change of refrangibility of the F line.

The remaining part of the book is devoted to stellar and meteoric spectrum analysis. It is impossible to give a superficial notice of the beautiful researches of Huggins and Secchi, researches which are always going on; the reader will find with interest various important results of these studies—for instance the existence in many stars of a good number of terrestrial substances—hydrogen, nitrogen, magnesium, sodium, &c.

One of the most interesting facts is the observation of the temporary star which appeared in May 1866; the great brightness of the star was due, as indicated by the spectroscope, to an immense mass of incandescent hydrogen.

At the end of the work the author gives some very important observations of Huggins and others on the spectrum of nebulæ; the chief result is the possibility, with the aid of the spectroscope, of distinguishing by the composition of their light the true nebulæ from the clusters of stars.

Finally, a description of the spectrum of the aurora borealis, the identification of its bright lines with some bright lines of the solar corona, a description of various meteors, lightnings, and their spectra, show into what difficult objects this new branch of science has pushed its investigations.

On the whole, this book must be considered as a good type of a "popular work;" it deserves the attention of the public, and the esteem of scientific men; and finally, it recommends itself by a gracious side. It was translated into English by two ladies, who have had the double merit of giving a proof of their good scientific taste, and of showing an example of the help which their sex is able to afford to science.

CORNU

OUR BOOK SHELF

Health and Comfort in House-building. By J. Drysdale, M.D., and J. W. Hayward, M.D. (London: E. and F. Spon, 1872.)

Sewer-gas, and how to keep it out of Houses. By Osborne Reynolds, M.A. (London: Macmillan and Co., 1872.)

THE first of these works supplies a want long felt by that section of the public who are desirous of obtaining a good supply of fresh air in their houses, without being subjected to the cold draughts usually associated with almost every system of ventilation. The book is most carefully written, and is evidently the result of much thought, time, and intelligent labour. After reviewing very fairly the systems of ventilation which have been proved to be ineffectual for supplying fresh warm air to the whole of a house, although perhaps very appropriate for single rooms, we are told that the key-note of this new system consists in utilising the kitchen fire, which is almost constantly kept alight in summer and in winter. "Endeavour is made to prevent the air from entering the house at all except by the inlet provided in the lowest story of the house, with conditions available for the warming, cleaning, disinfecting, or otherwise improving the quality of the incoming fresh air, and regulating its quantity; the fresh air is then conducted into the central private hall, which is protected from smells, and all other means of pollution: it is from this private hall that the rooms draw their supply, even when the doors are shut. Having served its purpose in the rooms, the air is drawn off through the ceiling into the foul air chamber, and thence down and behind the kitchen fire, up the chimney-stack, and discharged high up in the open air, all possibility of back draught being prevented by the length and heat of the exhausting-syphon." It is a work which can be highly recommended to the officer of public health, the architect, and the householder, as a guide to the true principles of healthy ventilation. In "Sewer Gas; a Handbook on House-drainage," we have a very simple and original plan suggested for preventing noxious gases and exhalations from drains entering our houses. It is shown that these gases, being specifically lighter than atmospheric air, frequently ascend in pipes, and that they are also occasionally drawn in by the suction caused by the warmth of a house through accidental crevices in the drain pipes. It is proposed to remedy these evils by doing away with all traps except those connected with the pans of closets, and by placing a large trap in the pipe which connects the house drains with the sewer. A plan of this trap is given, showing that it is easily accessible, and can be cleaned at any time by even an inexperienced workman. The subject is one of even more importance than good ventilation. When we recollect that one of the most valued lives in Great Britain has been so recently imperilled from a mere defect in a system of drainage, we cannot too highly estimate the efforts of those who suggest, both by precept and experiment, the adoption of such measures as will ensure the safety of all sensible householders.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Hindrances to Students of Mathematics

It was the opinion of Dr. Samuel Johnson that everything ought to be persecuted in order that we may know whether it is worthy to live or not. There is, doubtless, a good deal of truth in this opinion, and the idea or the man that cannot endure and overcome a considerable amount of difficulty is of but little value. Still there must be a reasonable limit to persecutions and difficulties, and hence I hope that the praiseworthy efforts of the English mathematicians to improve their text-books of geometry will be successful. In considering such a matter as the improvement of text-books, an extensive knowledge of the experience of all classes of students will be valuable, and as many of the mathematical books profess to be written for those who are not fortunate enough to have a teacher, an account of the difficulties which such a one has experienced may be of some interest.

1. I place first among these difficulties the practice common to nearly all mathematical writers, of restricting the number of axioms or fundamental assumptions, making them fewer than they naturally are. It is worse than useless to attempt to prove something that is self-evident, or which is so nearly so that it is impossible to make any proof illustrate it. In all such cases it would be better to state frankly and clearly that we make an assumption, depending on observation to justify it. An example of this superfluous proof may be found in many of the books on rational mechanics, where we are told that a body cannot move out of the place of the forces, because we know of no reason why it should move to one side rather than the other; therefore, &c. Of useless definitions we have an example in a popular work on arithmetic, where we are told that "time is the measurement of duration," and a few pages further on that "duration is a portion of time." Allied to this is the contemptible habit of those who explain, with kind condescension and with great detail, all insignificant matters, while at the same time they cover up or dodge by some such phrase as "it is evident" the really difficult points.

2. I do not object to a frequent and thorough application of the differential calculus in a text-book, and such an application seems to me better than the coarse processes under which this calculus is sometimes concealed; but there is a habit, common to young writers, of introducing forced and difficult demonstrations where more simple ones would be better. An illustration may be found in one of our best books on astronomy. In the first edition of this book the author gave a long and difficult demonstration of the well-known formulæ for the transformation of rectangular co-ordinates in a plane. The demonstration was made to depend on the solution of functional equations by means of the differential calculus, and is an awkward thing to place at the beginning of a text-book. In the second edition, having removed this demonstration and supplied its place by a simple one, the author has made the first chapter of his book the best synopsis of spherical trigonometry that I know of.

3. An error of the English text-books written by Cambridge men is, I think, the great number of examples given at the close of each chapter. At least one-half of these should be omitted. It is a great mistake to keep the student lingering over the never-ending questions of conic sections, of maxima and minima, &c., and to give him the habit of solving petty problems, when he should be led forward as soon as possible to the study of the memoirs of those who have created the science. In this connection it seems to me a mistake in treating the differential calculus to confine ourselves rigorously to the notion of a limit. Although the doctrine of limits may be the only logical foundation of this calculus, the student as he advances must soon become familiar with differentials, and it is well that he should make their acquaintance in his text-book.

4. A defect, perhaps of teaching rather than of text-book, is the ignorance of all American students of numerical and logarithm calculations, and from my slight observation I infer that such is the case also with English students. It is not uncommon to hear such calculations spoken of with contempt, but there is nothing that gives one a clearer idea of the meaning of analytical formulæ than to make a numerical application of them. In this matter it seems to me that the assistance of a teacher is of much more importance than in dealing with theoretical difficulties, since with these a student must generally be left to himself, while a little advice from a skilful computor will save the beginner much time and trouble.

5. Finally I mention, as a source of some confusion and perplexity to the student, the changes of notation and the introduction of new names. Some such change and inventions will be necessary with the progress of science, but any which tends to mar the symmetry of analytical expressions, and render less easy the reading of the great mass of mathematical literature that we already possess, should be avoided. To call a well-known function a "wonnumetonomy," or a "subcontra-wonnumetonomy," does not of course endow it with any new properties, or make its discussion one whit easier, although we may gain a slight advantage in the way of brevity of reference. For my own part I hope that this introduction of words of thundering sound, and the calculation of almost interminable formulæ, for which no more ingenuity is required than for a numerical calculation, is only premonitory to the invention of a calculus of operations which shall furnish us with shorter and more powerful methods of investigation. ASAPH HALL

Washington, August 16

Jeremiah Horrocks

In the course of research (for literary purposes) concerning Jeremiah Horrocks, the astronomer, born at Toxteth, near Liverpool, 1619, I have been unable to discover his parentage. Could any of your readers help me in this matter?

London, Aug 12 E.

The "Mors Electrique"

With reference to your notice of M. Sidot's "Mors Electrique," I may mention that in India it has been proposed to use Magneto Electricity for the purpose of starting a jibbing horse, by passing a spark between two points attached to the breeching. I believe this would be really practicable and useful.

August 9 J. F. TENNANT

MAGNETICAL AND METEOROLOGICAL WORK AT BOMBAY

WE have received from Mr. C. Chambers, F.R.S., the Director of the Colaba Observatory, Bombay, three memoirs, to appear eventually as appendices to the volume; observations dealing with (1) the Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay, and its Secular and Annual Variation, (2) on the Lunar Variations of Magnetic Declination at Bombay, and (3) a description of a new Self-Registering Rain Gauge. In the first memoir Mr. Chambers refers to the diminution of terrestrial magnetic action with increase of height above the ground. He states, "I am aware that experiments have at times been made to determine the effect upon the terrestrial magnetic force, of change of elevation or depression, both upon mountains and in mines; and it may be that such have been made also upon high buildings; but excepting the observations made in the vaults of the Paris Observatory, which I have not seen any discussion of with reference to this point, I believe that no long series of observations—capable of detecting small differences of the kind now pointed out—have been made elsewhere than at Bombay; and that the facts so strongly brought to light by the Bombay observations have not previously been forcibly commented on. It has now been shown—by the discussion of independent observations in each case—that diminution of effect with increase of height extends to—(1) the Secular Variation of Declination, (2) the Secular Variation of Horizontal Force, and (3) the Diurnal Inequality of Horizontal Force. Consistent testimony of this kind—even allowing for the possibility of explaining the first case on a different hypothesis—gives probability to the supposition that the phenomenon of sensible diminution of terrestrial magnetic action with moderate and practically attainable elevations above the earth's surface is general."

The object of the new rain gauge is to produce a complete record of rain-fall by means of photography, with this additional advantage, that whenever a barometer is kept in continuous operation there need be no additional expense in working the rain gauge.

SCIENCE IN JAPAN *

PROF. W. E. GRIFFIS writes us a very encouraging letter from Fukuwi, Japan, where he is giving practical instruction in a chemical laboratory established a year ago. Sixty students attend his daily lectures on chemistry and physics, properly illustrated by experiments, and twelve students do actually practise in the chemical laboratory. What he says of Japan is equally true here in the United States, only that the rubbish of astrology and Chinese philosophy, which prevent rapid progress there, are here represented by notions not less common nor less obstinate. He says:—"In teaching physical science in Japan, one has need to begin at the lowest foundation, to demonstrate everything, and to clear away much rubbish of astrology, Chinese notions of philosophy, falsely so called, &c.; yet the students are fairly intelligent, and promise hopefully to fill, in some measure, the greatest educational need of the country—good teachers."

The following will also merit attention:—"It may please you to know that Japan, just entering upon her course of modern civilisation, has begun by not only assigning a foremost place to physical science in her schools, but has already established several laboratories, in which students receive practical instruction from German and American professors. The chief laboratory in Osaka is presided over by a German professor, having nearly one hundred students. Another laboratory, it is expected, will be established in Yeddo. There is one in the province of Kaga, in charge of a German professor; another, also under a German, is at Shidzoka, in the province of Suruga. The laboratory in Fukuwi, province of Echeyen, has been established nearly a year." This is the laboratory of Prof. Griffis, above spoken of.

It gives us, indeed, great pleasure to record these significant evidences of progress in the far-off Japan. These facts, as well as many others, show that at length commerce, the arts, and physical science, have commenced their missionary career in Japan, and will soon introduce the blessings of civilisation in that great country leaving the Japanese and Chinese gods to take care of themselves, if they can.

THE "HASSLER" EXPEDITION

THE GALAPAGOS ISLANDS

THE numbers of NATURE for July 11 and 18 contain reports from Prof. Agassiz himself of his *Hassler* Expedition; we are glad to be able to furnish the following continuation of these reports from a writer in the *New York Tribune* who accompanies the Expedition:—

"About sundown on Thursday June 6 we bade good-bye to the Ossipee and to Payta, and, with a fair wind and smooth sea, started for the Galapagos. Darwin's account of this archipelago had excited our curiosity and interest to the highest degree. Of course our visit was all too short to settle the many interesting questions which his narrative suggests. We landed on Charles early Monday afternoon, June 10, and left early Wednesday morning. We landed at Tagus Cove, on Albemarle, Thursday morning, and remained until Friday afternoon. On Saturday at noon we anchored off James Island, and remained until Sunday at 10 o'clock, when we visited Jervis Island, and remained until sunrise on Monday. We anchored at Indefatigable Island on Monday by 9 o'clock A.M., and were detained repairing our engine until Wednesday afternoon, June 19, when we started for Panama. In so brief a visit to so remarkable an archipelago, no conclusions can properly be drawn from what we did not see, and yet one of the most interesting points of inquiry was precisely one that can be definitely settled by negative testimony only—the inquiry whether plants and animals found on one island are wanting on the other islands in the group. That this should be the case is not incredible; even on the continent we sometimes find plants and animals confined to very narrow localities. And although we had a very limited opportunity to examine the five islands on which we landed, our observations, as far as they go, tend to confirm the statements heretofore made that the Galapagos have a fauna and a flora decidedly of an American type, yet decidedly peculiar to themselves, and that even each island differs from the other islands; nay, that this difference extends, in some degree, to the fishes in the bays around their shores.

"The islands are to my eye much more recent in their formation than Juan Fernandez. Indeed, Narborough and Albemarle have so fresh a look that you could easily believe that there had been extensive eruptions there within the present century. Immense domes, 4,000 to 5,000 feet high, stand upon very flat truncated cones, twelve or fifteen miles in diameter. Over the whole surface of such mountains are scattered craters, chimneys, and small truncated cones. From many of these craters streams of lava have flowed toward the sea, some of them spreading out to miles in breadth and miles in length, and this lava is so fresh and black that as you walk over it you scarce find even a lichen adhering to it; it is a rough field of hard black slags or clinkers. This crust of cooled lava is cracked into rude hexagonal blocks of from six inches to several feet in diameter, and between the blocks you may find cracks so fine that the rain will scarcely penetrate them, or you may find chasms a yard in width and many feet in depth. The thickness of this crust also greatly varies. Here may be a swelling in the ground, a little hillock bursting at the summit, and showing a lava-crust of unknown thickness, and a few yards off a similar hillock, or a black ridge, may show through its openings that it is a mere shell, from which the fluid, molten contents were drained; while the crust was barely thick enough to sustain itself. The general level of the field is thus diversified by innumerable pits, caves, small cones, and craters, which, especially in such hard, rough material, make it a very "hard road to travel." The remaining surface of the mountain is similar, but composed of older lava, in the cracks of which a few scattered trees and bushes find a foothold, and give a meagre clothing to the land. Occasionally a patch of volcanic sand, or sandstone, gives the vegetation a better chance. The more eastern islands of the group are simply like these better parts of the western. There were many indications that our visit was in a time of drouth—for example, an abundant growth of a delicate fern, *Adiantum*, on James Island, was withered to the roots. This drouth may have been one reason why the whole archipelago, with the exception of James, and small patches on Jervis and Indefatigable, had a blasted look. The trees and shrubs were nearly all leafless, and the bark of the two most abundant species was light gray, almost white. Two kinds of prickly pear—*Opuntia* and a cactus more like a *Cereus*—made a striking contrast to this white shrubbery, lifting their solid dark green masses high above the bushes and dwarf trees, particularly on Indefatigable, where all three kinds abounded. I saw in the short rambles which I had to take only one really fine kind of tree; it was a straight trunk, very smooth, glossy bark, vigorous branches, and grew on James Island. It was entirely leafless; but the dead leaves and fruit pods under it showed that it belonged to the great family in which our locusts and coffee bean are placed; it had large trifoliate leaves, and a bright scarlet bean. Another tree of the same family had a very singular appearance; the plant itself looked like a dwarf walnut or butternut; the pod was very thin and narrow, but carried four thin wings half an inch wide, thin as paper, standing at right angles, and extending the whole length of the pod.

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* From the Iowa "School Laboratory of Physical Science."

"The geologists were quite successful in getting specimens of various animals. Over fifty different kind of fishes were obtained, and of these over three-fourths are peculiar to the Galapagos. Of the Galapagos, from which the islands are named, and in which they once so richly abounded, we only got a few specimens, and those very small compared with those of olden time. They have been so eagerly hunted for their flesh that they have been driven from the more accessible places, and stand a good chance of being altogether exterminated. Their brethren in the sea, the tortuga or sea-turtle, we saw in abundance, and got some very fine specimens. There are, as is tolerably well known, two other reptiles for which this archipelago is famous—two lizards of a genus not found elsewhere, and very peculiar in their habits. The Spaniards called them iguanas, from their resemblance to that reptile in the West Indies and Central America. But they differ so much from their American cousin that they ought to have a name of their own; and if the scientific *Amblyrhyncus* looks too formidable, let us translate it and call the creature a Bluntnose. On Charles Island we found abundance of the crested Bluntnose climbing with great agility over the rocks near Black Beach. The creature is about 30 in. long, nearly black, the old males having a deep red hue on the sides. It swims with great ease by its flat tail, and uses its long fingers and long nails for scrambling on the rocks, holding them while swimming close to the body. There is not a trace of web-footedness about them, and they make no use of the feet in swimming. They live on sea-weeds from the rocks in deep water, and their expression is mild and herbivorous, with a little clear, innocent eye. I was prepared for something hideous, and was agreeably disappointed. In another respect our experience differed from Darwin's, for we sometimes had no difficulty in frightening them into the water, and they came fearlessly swimming about the *Hassler* as she lay in Tagus Cove. These crested Bluntnoses we found upon all the islands. The slightly crested Bluntnose we found only on Albemarle and Indefatigable. Its scientific name might mislead one, for its head is just as much crested as its aquatic brother's. The only differences between them apparent at first sight are these:—The terrestrial animal is somewhat stouter, his nose is longer, his eye brighter, his tail less flattened and less crested, and his colour is a dusky orange, deepening into brown on the hindquarters. His habits of life are very different, as he does not go near the sea, but lives upon land plants, and makes a burrow for himself in the sand and among the fragments of lava. He spreads his hind legs flat on the ground, raises his chest to the height of his fore legs, and then nods and winks at you in a very odd way. It looked to me very much like swallowing, and I thought it possible that the creature, with his head in that position, swallowed air like a toad, as a means of breathing—swallowing into the lungs, not into the stomach.

"One of our most interesting adventures was landing in a little bay full of seals, so tame, or rather so little afraid of men, that we could tramp past groups of sleepers on the beach without awakening half of them, and without apparently frightening half of those that we did awake. They seemed to be fond of crawling under bushes just above high-water mark, and sleeping, two or three in a place, huddled close together. Under one bush lay a mother and her two cubs, so fearless that one of our officers held a piece of cracker to the old one, and she smelled it in his fingers as fearlessly as if she had been a pet dog. The cubs quarrelled with each other as to which should cuddle nearest the mother, and they all three snarled and snapped at the flies in the manner of a sleepy dog, and all this while a party of ladies and gentlemen, creatures as large as the seals, and which the seals could scarce have seen before, stood looking on within touching distance. These seals had much more length of arm, and used their arms more in the manner of a quadruped than I had supposed any seal could do. I saw them walk on the beach with the whole chest clear of the ground, and even jump upon the sand. Their favourite gymnastic exercise, however, was to lie upon their backs and roll in the manner of a horse. The tameness of these seals and of many of the land birds was very surprising; the Bluntnoses were more shy than we had expected. I repeatedly put my fingers within half an inch of little yellowbirds and phebes, and within six inches of mocking-birds. On James Island the birds were so numerous and so tame that while I was trying the experiment whether whistling to a yellowbird would divert his attention so much as to make him allow me to touch him, six other birds—including two mocking-birds—came up and alighted on twigs within two yards of the yellowbird, to see what was going on between us. As for the flies, their tameness and pertinacity of adhesion at the Galapagos goes far beyond all travellers' accounts. I knew a good housekeeper in New England who affirmed that house-flies could not be driven out of a room unless you struck and killed one or two, in order to show the others that you were in earnest. You cannot drive the Galapagos flies from you even with that.

expedient. The birds and seals are not frightened by being stoned or shot; they don't know what stones and guns mean, and the flies are not frightened or discouraged by having any amount of their comrades killed. When a boat was coming off shore, the usual occupation, in order to prevent carrying the nuisances on board, was for everybody to be picking the flies off themselves (almost as they would burrs), killing them and throwing them into the water from the time of leaving the beach to the arrival on the deck of the ship; and the last fly slaughtered before you go into the cabin is no more afraid of you than the first one you slew at the beach. They are not biting flies—we have escaped trouble from mosquitoes and biting flies during the whole voyage—but they are crawling, tickling, adhesive, tantalising creatures. It was pleasant to find here at the Galapagos a species of penguin, smaller and more sober in dress than our old friends of the Straits of Magellan, but with the same winning, cunning manners that made the birds in the Straits such favourites with our party. And while speaking of the birds of these islands, I would not forget the splendid flamingoes, six feet high, of which we got many fine specimens. They sailed about in parties of twelve or twenty birds together, making long lines of scarlet flame floating through the air. We tried their flesh on the table, and found it the most delicious game, fully equal to the canvasback, as it seemed to us.

"One lesson I must confess to having learned at Indefatigable Island. I saw there indisputable proof that the surf of the sea is capable of rounding angular fragments of lava into pebbles, somewhat resembling in shape (but not at all in polish and grooving) glacial boulders. I had always from boyhood doubted the power of the sea to make angular fragments round; I had supposed that the action of the surf upon such fragments would be simply to pack them into a sort of McAdam's roadway. And even now, having had this proof that under peculiar circumstances the sea can make a tolerable imitation of drift, I am not a whit more ready to believe that the sea made the drift itself. You may prove to me experimentally that flour can be made from wheat with a pestle and mortar, but that will not convince me that the flour markets of the world are thus supplied. There are one or two little colonies on the island, but the colonists have a hard life, and there can hardly be any agriculture there for centuries to come. At present the two main products of the islands are terrapins (galapos), which are almost exhausted, and wild pigs, which are of little worth, and which are destroying the wild plants and animals. The archipelago offers at present a fine opportunity for a naturalist, who desires to make a residence here for several years, and thoroughly explore their structure and their productions, to throw a strong light upon the great modern question of the origin of species, and the doctrines of evolution. Younger than Juan Fernandez, purely volcanic, bringing no seeds with them from the bottom of the sea, not having had time to alter and amend species introduced from the mainland, how did these islands come in possession of their peculiarly organised beings—their Bluntnoses for example? This was the question constantly recurring to me during my visit to the Galapagos, as it had been at Juan Fernandez. Prof. Agassiz gave us a little talk one day on our way to Panama, and discussed the same point. Expressing his warm admiration for Darwin's moral and intellectual character, and earlier scientific labours, he said that he considered his present influence on science very pernicious as favouring the habit of 'filling up the wide gaps of knowledge by inaccurate and superficial hypothesis.' What we need in order to extend our knowledge of the origin of species, is not hypothesis and speculation, but a careful collation of facts, and a careful extension of our observation of facts. The hypothesis that the differences of species were produced by variations taking place in unlimited, in indefinitely long periods of time, is, at all events, strongly negatived by this occurrence of such marked peculiarities of difference from the surrounding world, in an archipelago that belongs wholly to the present geological epoch, and has not existed an indefinite time. It was very pleasant to us all to hear this greatest and most earnest opponent of Darwin rendering with such manifest sincerity his tribute of admiration for Darwin's genius and industry, and confessing with such evident pride his warm personal love toward him. As to the question of the origin of species, I think we were all willing to leave it a question. Darwin's hypothesis of gradual variation of species, and the natural selection for preservation of those whose variations were favourable to them in the struggle for life, seems to me to have few facts to sustain it, and very many to oppose it. At the same time it must be conceded that all the maxims of metaphysics and theology combine in assuring the man of science that he is always right in assuming the utmost paucity of original causes. The universe is certainly framed with infinite skill and wisdom, and there never will be found two different things, where one would answer. If the present existing forces of nature can bring an Amblyrhynchus and an Iguano out of one common parent, it would have been a waste of creative power to make two parents; that concession to the doctrine of evolution is demanded by philosophy and the principle of least action. But the facts of zoology seem to me to indicate clearly that the present acting forces of nature can do no such thing.

THE LATE PROF. DR. F. KAISER

ON July 28 last died Prof. Kaiser, Director of the Leyden Observatory.

Kaiser was born on June 10, 1808, at Amsterdam, where he was educated by his father, and, after the latter's death, by his uncle, J. F. Kaiser, himself a zealous promoter of astronomical research. In 1828 young Kaiser, whose love for astronomy had at an early period shown itself, was appointed assistant at the Leyden Observatory, which was then superintended by Prof. Wylenbrock. Till 1837 he remained in this position, improving himself by the study of all the best works in his department, when he was appointed Professor of Astronomy in Leyden University and Director of the Observatory. It is well known that this appointment marks the beginning of an epoch in the history of astronomy in the Netherlands. By unwearied exertions he soon collected some good instruments, and by means of his numerous and partly popular lectures he kindled such an interest in astronomy among the people that, in the year 1856, one of the items in the Budget was the cost of erecting a new Observatory. This he entered in 1860, furnished with many new instruments. This was followed in 1866 by permission, obtained through his never-tiring exertions, to publish "Annals." Notwithstanding that in the spring of this year he was seized with severe chest disease, which became a sad hindrance to his labours, he occupied himself with the editing of the "Annals," and with the improvement of the organisation and instruments of the Observatory. On the 4th of November last he set to work to perform some calculations necessary to complete a lecture for the third volume of the "Annals," on "The Measurement of the Diameters of Planets," and on the day after was seized with a hæmorrhage which made his illness assume a more critical character. Even from this attack he might have rallied with returning spring, had it not been for the death of his wife, with whom he had been happy for 41 years. From that blow he never recovered.

That astronomy has sustained a great loss in Kaiser, all who take an interest in the science must feel. It is to be hoped that ere long a worthy account of his life and labours will be given to the world.

NOTES

THE work of the French Association, which, as we have already announced, meets at Bordeaux from the 5th to the 13th September, comprehends—1st, At least two general meetings; 2nd, Meetings of divisions and lectures; 3rd, Scientific excursions; 4th, Public lectures. Of the last there will be four, the first of which was to have been by the late lamented M. Delaunay, on "The Constitution of the Sun;" his place will probably be filled by M. Cornu. The second public lecture is to be by M. Broca on "The Troglodytes of *Les Eysies*;" the third by M. Levasseur on "Commercial Geography;" and the fourth by Lieutenant F. Garnier, on "The Voyage of the *Cambodge* and the Political and Commercial *rôle* of France in the extreme East." There are to be seven excursions in all, one to the mouth of the Gironde for the purpose of observing the changes in the coast-line; another to the pre-historic remains and bone caverns of *Les Eysies;* the sixth is to Medoc for the purpose of visiting the great vineyards of Château-Margot and Château-Montrose.

THE International Congress of Scientific Archæology was opened at Brussels on August 22, with an attendance of 600 men of science of various nationalities, including Prof. Owen and M. Virchow. After being entertained to luncheon in the Hotel de Ville, the *savans* adjourned to the Ducal Palace, where, at two o'clock the Congress was opened under the presidency of M. d'Omalius d'Halloy, the Belgian senator and eminent geologist, who will soon be a nonagenarian. The opening address was given by M. Dupont, the distinguished director of the Brussels Natural History Museum, who gave a summary of the results of the researches relative to pre-historic times in Belgium. The Executive Committee, under the presidency of M. d'Omalius d'Halloy, was then constituted. The Belgian vice-presidents are MM. Hagemans, van Beneden, and Baron de Wit. The vice-presidents for foreign countries are M. Virchow, for Germany; M. de Quatrefages, for France; Mr. Franks, of the British Museum, for England; M. Nilsson, for Sweden; M. Steenstrup, for Denmark; and M. Conestabile, for Italy. M. Dupont, of the Belgian Museum, was installed as secretary-general. The excellent club known as the *Cercle Artistique et Littéraire* has thrown open its hospitable doors to the members of the Congress. On Saturday, Aug. 24, an excursion under the auspices of the Congress took place to the valley of the River Lesse, a tributary of the Meuse, which it joins not far from Dinant. The purpose of the excursion was to inspect the prehistoric remains which abound in the numerous caverns that exist along the banks of the Lesse. M. Dupont acted the part of guide, and his disquisitions at each particular spot of interest were followed by lively discussions among the geologists present. On returning across the Lesse from inspecting the *Trou de la Naulette*, one of the over-crowded boats had its balance disturbed and went down, happily with no worse result to the passengers than a thorough drenching. Among those on board was Mr. Franks, of the British Museum, who, with another gentleman, gallantly rescued Mme. Royer, Mr. Darwin's French translator. At Sunday's meeting it was decided to hold the meetings of the Congress biennially instead of annually. On Monday, 26th, an excursion was made to Mesvin and Spiennes. The latter place is supposed to have been a manufactory of flint implements, the ground visited being thickly covered with flint splinters and unfinished flints. After luncheon the railway cutting at Mesvin was visited, where some lively discussion ensued, the party subsequently inspecting some pits from which the flint for making tools had been extracted.

THE Government of New South Wales, following the lead of Europe and the United States, has introduced the system of telegraphing the anticipations of the weather, and has established certain stations on the coast for indicating the nature of any expected storm by means of signal masts. These signal masts support two yards, crossing each other at right angles in the direction of the cardinal points of the compass. A violent squall is to be represented by a conspicuous diamond-shaped signal; a heavy sea by a drum; a gale with clear weather is indicated by a diamond-shaped signal over a drum; and one with thick weather and rain by the same signal under a drum. The direction in which the wind is blowing is indicated by the particular yard-arm between which and the mast-head the geometrical signal is suspended. Gales that are general over a large portion of the coast are indicated without the mast-head flags, by the geometrical figures.

WE learn that Professor Nordenskjold, the originator and leader (under the direction of the Royal Swedish Academy) of the Swedish North Polar Expedition, contemplated since 1861 arrived on the 17th of last month at Tromso, in Northern Norway, and sailed away again in the iron steamer *Polhem* on the 21st. Before leaving that port for the far north he sent a letter to Mr. Oscar Dickson, merchant, of Gothenburg (thanks to whose energetic and liberal support the expedition was enabled to start this year), giving a short description of the means at his disposal. The plan of the expedition is that of wintering on the northernmost islets of Spitzbergen (the Seven Isles), whence by the aid of reindeer sledges an over-ice journey northward will be attempted. The professor is accompanied by two physicians, a naturalist, an Italian naval officer, a first mate, two engineers, ten picked seamen, and four Lapps for attending the reindeer, from forty to fifty of which, with 3000 sacks of reindeer moss and other necessaries for wintering in the Arctic regions, have been taken by another (hired) steamer, the *Onkel Adam*, to the intended winter quarters. The Swedish Government has placed the brig *Gladan* at the professor's disposal till the beginning of winter. This vessel has also started from Tromso, having on board a house, in which the exploring party is to winter in the Seven Isles; she will return to Tromso, and hence take back a second cargo, consisting of coals The expedition is in addition furnished with 1545lb. of paraffin, to serve as lighting and cooking material on the sledge journey. The house contains six living rooms, one of which is to be used as a workshop, a kitchen, pantry, bath-room, and frost-proof cellar. The expedition has also taken from Stockholm three "observation sheds." It is amply provisioned for two years, and well supplied with warm winter clothes, among which are complete suits of Lappish clothing for every person in the expedition. On the sledge journey, amongst other things, rum, paraffin, sleeping bags, tents of tarpaulin, a large sleeping carpet, &c., will be taken. Three boats, weighing respectively 300lb., 200lb., and 150lb., and specially adapted for ice travelling, with sledges, had been shipped at Copenhagen. To assist the Laplanders in the management and supervision of the reindeer, they have with them five reindeer dogs. Three live pigs form also part of the provisions. Finally, the expedition is well provided with all necessary scientific instruments.

PROF. C. H. F. PETERS, of Hamilton College, U.S., has discovered two more new planets of 11·5 and 12 magnitude respectively, provisionally numbered 123 and 122.

THE International Statistical Congress has opened at St. Petersburg its eighth session since it started its work in 1853, and is divided into five sections: the first on questions connected with the census of the population, the second on the movement of the people, the third on industry, the fourth on postal relations and commerce, the fifth on criminal statistics. The Congress is well attended by representatives of all countries, with a good sprinkling of English members. Dr. Farr, Mr. Hammick, and Mr. Lock represent the official part of our members; Mr. Samuel Brown, Mr. Hamilton, Mr. Hendriks, Dr. Mount, M. Tayler, and Mr. Levi, represent the Statistical Society of London; and there are also Mr. Freeman, Mr. Heron, and Mr. Wethered as voluntary members. The Russian Government and the municipalities of St. Petersburg are most liberal in their arrangements for the reception of visitors. A free railway ticket over the Russian territory, and free lodgings in the best hotels, for every member of Congress, have been provided. The Princess Helena gives evening entertainments. The Hermitage and Museums are open, and there are to be excursions to the Exhibition at Moscow, and to the fair at Nijni Novogorod.

THE session of the Physical Science College of Newcastle-upon-Tyne will commence on Oct. 2, and will be divided into the Michaelmas, Epiphany, and Easter Terms. No preliminary examination is required, but students must be above the age of fifteen years. All particulars will be found in the college prospectus, to be had free on application to Mr. W. Bunning, secretary to the college.

THE twenty-seventh annual meeting of the Cambrian Archæological Association has been opened at Brecon, and the Congress will sit up to Friday evening. The President for the year is Sir James Russell Bailey, M.P., of Glamish Park, Crickleweil. The proceedings will be of the usual character, including the reading of papers on subjects of archæological interest, and daily excursions to places of note in the surrounding neighbourhood.

THE *Journal of the Franklin Institute* calls attention to the following interesting lecture experiment:—It is well known that a light ball, as of cork, is sustained for some time near the summit of a vertical jet of water, issuing from an orifice of such a nature that the steadiness of the jet is maintained. The experiment becomes more striking when a vertical blast of air issuing from a large bellows is substituted for the jet of water, as in this case there is no apparent support for the ball, which comports itself in a very amusing manner. When a strong blast cannot be obtained, if a slender wire, about four times the length of the diameter of the ball, be passed through its centre, so as to have one-fourth of its length projecting from one end, and one half from the other, the balancing is more readily obtained, as any considerable change in the relative positions of the centre of gravity and the point of support is prevented by the movements of the rod.

WE learn from the *Journal of the Society of Arts* that the directors of the telegraphic lines of France have recognised the absolute necessity of improving the theoretical and practical knowledge of its clerks, and with this view elementary courses of telegraphy have been arranged in all the chief towns, at which the attendance of the *employés* is obligatory. In addition to this, a superior course of instruction is to be opened in Paris, and those clerks who have most distinguished themselves in the provinces will be sent to the capital to complete their instruction. The courses are all to commence on the first of October.

Harper's Weekly, of August 17, announces the deaths of Mr. Sidney J. Lyon, a gentleman well known for his valuable geological and archæological researches while State geologist of Kentucky; and of Mr. Edmund Ravenal, of Charleston, S. Carolina, and Dr. Hubbard, of Long Island, both eminent conchologists.

WE notice from the *Field* that on September 3 and 4 a sale of the surplus animals of the Zoological Gardens of Antwerp is to take place. The collection to be disposed of includes many of the rarer species of mammals and birds. In the former figures a young Indian rhinoceros, several species of antelopes, mouflons, and a male markhare ("which," says the *Field*, "offers a chance for any one desirous of increasing the size of our Welsh goats"). The birds include ostriches, several species of rare and new pheasants, and a considerable number of the rarer water-fowl, serpents, pythons, &c.

WE learn from the *Times of India* of July 26 that Mr. Mark Fryar, the mining engineer who has been specially engaged to develop the mineraliferous resources of British Burmah, spent the last two months of 1871 in exploring the Mergui district of Tenasserim; and the results of his explorations have just been published by the Etcetera Department as an extra supplement to the *Gazette of India*. Of the coal which exists there Mr. Fryar does not speak at all hopefully. It is deficient in quality, and could not compete with English and Australian coal; besides which, there is no demand for it, the dense forests of Tenasserim being capable of supplying the whole country with fuel for generations to come. Summing up the results of his two months' tour, Mr. Fryar says the most remarkable feature of the districts is the wonderful extent of the distribution of staniferous detritus: "In rivers on the mainland and on islands of the sea, every small dishful of sandy gravel taken up contains palpable traces of black tin-stone;" and he thinks it exceedingly probable that a thorough examination of the hills whence the rivers flow would be rewarded by the discovery of rich veins of tin ore, which could be worked with the most profitable results.

A LETTER from Bucharest, given in the *Levant Times*, reports a curious atmospheric phenomenon which occurred there on the 25th of July, at a quarter past nine in the evening. During the day the heat was stifling, and the sky cloudless. Towards nine o'clock a small cloud appeared on the horizon, and a quarter of an hour afterwards rain began to fall, when, to the horror of everybody, it was found to consist of black worms of the size of an ordinary fly. All the streets were strewn with these curious animals. It is to be hoped that some were preserved, and will be examined by a competent naturalist.

THE International Congress on Weights and Measures meets at Paris on September 24.

THE cyclone which had been heard of from the Bay of Bengal, broke over Balasore early on the morning of the 1st July. There was a heavy gale from the N.W. at about 2 A.M., and at 4 A.M. the wind veered to the N.E., and blew with tremendous violence, subsequently passing to the E. and S.E., and dying away at one o'clock. The station, which was once one of the prettiest in Bengal, is said now to be a mass of ruins. The destruction of property has been very great. Several thousand people were rendered homeless, and many without food. The telegraph lines were carried clean away for several miles on both sides of Belasore.

THE *Honolulu Gazette* reports the following interesting fact which has recently been observed respecting the growth of coral, and which deserves very careful consideration:—Somewhat less than two years ago a buoy was moored in Kealakekua Bay. Last week the anchor was hoisted in order to examine the condition of the chain. "The latter, which is a heavy 2 in. cable, was found covered with corals and oyster-shells, some of which were as large as a man's hand. The large corals measure four-and-a-half inches in length, which thus represents their growth during the period of two years that the anchor and cable have been submerged. The specimens which we have seen show the nature of the formation of the coral by the coral animals very distinctly. When taken out of the water it had small crabs on it. A question arises whether these crabs live on the coral polypes, or whether they simply seek the branches of the coral for protection. The popular idea is that corals are of extremely slow growth, yet here we have a formation equal to over seventeen feet in a century."

THE BRITISH ASSOCIATION

BRIGHTON, *Thursday, Aug.* 22

THIS year's Congress of the British Association was formally brought to a close yesterday afternoon by a largely attended general meeting held in the Dome, the main purpose of which was to allot votes of thanks to those who had officially done their best to make the meeting successful. One of the best-deserved votes was that to the Mayor and Corporation of the town, who had done all that lay in their power to promote in every respect the comfort and convenience of visitors. If the meeting has not been in all respects a success, it has certainly been from no want of hospitality and courtesy on the part of the Brightonians. Prof. Fawcett proposed the vote of thanks to the distinguished president, Dr. Carpenter. The professor's praise of the president was hearty and well-deserved. In returning thanks Dr. Carpenter paid a well-merited compliment to Mr. Griffiths and Mr. Galton, "his right and left hands, the former his *fidus Achates.*"

The following are the grants agreed to this year, with the names of the members entitled to them :—

MATHEMATICS AND PHYSICS	
*Cayley, Prof.—Mathematical Tables	£100
*Thomson, Sir W.—Tidal Observations	400
*Brooke, Mr.—British Rainfall	100
*Everett, Prof.—Underground Temperature (100*l.* renewed)	150
*Griffith, Mr. G.—Gaussian Constants (renewed)	10
*Glaisher, Mr. J.—Luminous Meteors	30
Glaisher, Mr. J.—Efficacy of Lightning Conductors	50
*Williamson, Prof. A. W.—Testing Siemens' New Pyrometer (renewed)	30
*Huggins, Dr. W.—Table of Inverse Wave-Lengths	150
*Tait, Prof.—Thermal Conductivity of Metals	50
CHEMISTRY	
*Williamson, Prof. A. W.—Records of the Progress of Chemistry (100*l.* renewed)	200
*Gladstone, Dr.—Chemical Constitution and Optical Properties of Essential Oils	30
Brown, Prof. Crum.—Temperature of Incandescent Bodies	50
Brown, Prof. Crum.—Electric Tensions of Batteries	25
GEOLOGY	
*Ramsay, Prof.—Mapping Positions of Erratic Blocks and Boulders (renewed)	10
*Lyell, Sir C., Bart.—Kent's-Cavern Exploration	150
Lubbock, Sir J.—Exploration of Settle Cave	50
*Busk, Mr.—Fossil Elephants of Malta	25
*Harkness, Prof.—Investigation of Fossil Corals	25
*Carruthers, Mr.—Fossil Flora of Ireland	20
*Harkness, Prof.—Collection of Fossils in the North-west of Scotland	10
*Bryce, Dr.—Earthquakes in Scotland	20
Willett, Mr. H.—The Sub-Wealden Exploration	25
BIOLOGY	
Lane Fox, Col. A.—Forms of Instructions for Travellers	25
*Stainton, Mr.—Record of the Progress of Zoology	100
*Christison, Sir R.—Antagonism of the Action of Poisons	20
*Balfour, Prof.—Effect of the Denudation of Timber on the Rainfall in North Britain (renewed)	20
MECHANICS	
*Grantham, Mr. R. B.—Treatment and Utilisation of Sewage	100
*Froude, Mr. W.—Experiments on Instruments for Measuring the Speed of Ships and Currents (30*l.* renewed)	50
Total	£2,025

Some of the grants, as the president remarked, were for large sums, but it is gratifying to learn that the sum realised by the sale of tickets this year will more than cover the whole amount. The total number of tickets sold has been 2,533, representing 2,649*l.*

Not the least successful, and doubtless to many of the 200 guests, not the least enjoyable of the numerous meetings which have within the last few days been held at Brighton, was the *déjeûner* given yesterday by the hospitable mayor in the banqueting-room of the Royal Pavilion.

The Press arrangements have received very general commendation. Special praise is due to the *Brighton Daily News*, whose reports of general and sectional meetings were unremitting until it came to Mr. J. F. Walker's paper, and what the *Pall Mall* calls the alarming polysyllable "Dinitrobrombenzene." The *News* might, however, have got over the word, which we shall not venture to repeat, "but," it says, "since the communication was full of such words as mononitromonobrombenzene and metamononitromonobrombenzene, we do not imagine a full report would be interesting to our general readers." Our spirited contemporary is probably right, as the *Pall Mall* remarks; half-a-dozen such words might make a handsome day's wage even for the most diligent compositor.

In alluding to Dr. Carpenter's lecture on Chalk to be delivered to-night, the opportunity must be taken of supplying an omission which we unintentionally made last week. We should have mentioned that one of the three general lectures was by Mr. J. M. Duncan, on the "Metamorphoses of Insects."

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE

On a Periodicity in the Frequency of Cyclones in the Indian Ocean South of the Equator, by Mr. Meldrum.

One of the objects for which the Meteorological Society of Mauritius was established, in 1851, was to obtain extracts from the meteorological registers of vessels visiting the harbour of Port Louis, especially of such vessels as had experienced bad weather in the Indian Ocean.

Accordingly, clerks were employed to copy all the log-books that could be procured.

In 1853 the system of registration was remodelled. Instead of having the observations contained in each log-book recorded separately, all the observations in all the log-books for the same day were recorded on the same page.

As this system has been conducted without interruption to the present time, the Society has now a large collection of observations showing more or less the state of the winds and weather over the frequented parts of the Indian Ocean, in the form of a daily journal, during the last nineteen years; so that a person may find at once what weather prevailed on any day or in any year during that period.

Together with the years 1851-2, therefore, during which the registers were differently kept, we have 21 years' continuous observation, from the meridian of Greenwich to 120° E., and from 23° N. to 45° S.

Adding to the information obtained by the Society throughout these 21 years numerous observations collected by several persons for the previous four years (1847-50), we have a more or less complete record of all, or of very nearly all, the cyclones which have taken place in the Southern Indian Ocean during the last 25 years; for Mauritius is so much in the track of these cyclones, and so much visited by vessels in distress and by others trading between the colony and England, India, and Australia, that it is scarcely possible for any violent hurricane to pass without being noticed.

Taking now, for the present, the area comprised between the equator and the parallel of 25° S., and the meridians of 40° and 110° E., and examining a table of the cyclones that have occurred there from 1847 to 1872, it is found that some years have been remarkable for a frequency and others for a comparative absence of cyclones.

The five years, 1847-51, were characterised by cyclone frequency; then came a period of comparative calm (1852-57), which was followed by six years (1858-63) remarkable for cyclones. The next five years (1864-68) showed a considerable decrease; and since 1869 there has been an increase, until, for the present year (1872), the number of cyclones is already (28th June) greater than in any year since 1861.

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* Reappointed.

What has now been said is not only borne out by the records of the Meteorological Society, which give detailed accounts of the hurricanes, but also, I have little doubt, by the books of the docks and marine establishments.

Especially in 1847-48, and again in 1860-63, the harbour of Port Louis was at times crowded with disabled ships; whereas in the years 1855-57 and 1866-68 there were very few.

It will be seen that these years correspond pretty closely with the maxima and minima epochs of sun-spots.

For the present I wish merely to call attention to the subject, in order that the connection which I think exists between sun-spot frequency and cyclone frequency may be either verified or refuted by past or future observation.

It appears to me that there is more than a mere coincidence as to time. There are three maxima and two minima epochs of cyclone frequency, corresponding nearly, if not entirely, with similar sun-spot epochs.

To examine the matter fully, it would be necessary not only to know the number of cyclones in each year, but also the extent and duration of each and the force of the wind. If we could thus get an expression for the annual amount of cyclonic energy, and could show that it varied directly as the amount of sun-spots, a connection would be established. One violent hurricane, which lasted ten days and passed over thousands of miles, might have more value than half a dozen smaller and short lived ones. However, having traced a large number of the cyclones in question, I have no doubt that the years of greatest cyclone frequency were generally, if not always, the years of greatest cyclone energy; and that the number of cyclones in a year is a fair expression of the cyclonic activity for that year.

Now, taking the maxima and minima epochs of the sun-spot period, and one year on each side of them, and comparing the number of cyclones in these three-year periods, we get the following results:—

	Years.	Number of Cyclones in each Year.	Total number of Cyclones.
Max.	1847	4	15
	1848	6	
	1849	5	
Min.	1855	4	8
	1856	1	
	1857	3	
Max.	1859	5	21
	1860	8	
	1861	8	
Min.	1866	5	9
	1867	2	
	1868	2	
Max.	1870	3	14
	1871	4	
	1872	7	

Taking two years on each side of the solar-spot epochs, we get:—

	Years.	Number of Cyclones.	Total number.
Min.	1854	3	15
	1855	4	
	1856	1	
	1857	3	
	1858	4	
Max.	1858	4	32
	1859	5	
	1860	8	
	1861	8	
	1862	7	
Min.	1865	3	15
	1866	5	
	1867	2	
	1868	2	
	1869	3	

Assuming that we have got a close approximation to the actual number of cyclones, and that the numbers fairly represent cyclonic energy, it is difficult to avoid the conclusion that the above tables point to a definite law; and that meteorology, magnetism, and solar physics are closely connected. For what holds good with regard to a large tract of the Indian Ocean probably holds good with regard to other portions of the earth's surface.

Is it not probable, also, that if there is such a connection as is here suggested between the sun-spots, or sun-cyclones (as they have sometimes been called), and earth-cyclones, there is a similar connection between the sun-spots and cyclones in the other planets?

On the Spectrum of Hydrogen, by Arthur Schuster, student of Owens College.

In a paper communicated to the Royal Society, I have shown that nitrogen has only one spectrum; the band spectrum usually obtained at low pressures being due to oxides of nitrogen. I have since subjected hydrogen to a similar investigation, and although my experiments may not seem to give an absolute proof of the correctness of the opinion advocated chiefly by Angstrom, they show clearly, I think, to what causes we have to ascribe the different results obtained by different observers. The changes through which the spectrum of hydrogen passes when the pressure is gradually diminished are, according to Wüllner,[*] as follows: the spark of the small induction coil begins to pass under a pressure of 135 mm.; the colour of the spark is white, but only under a pressure of 100 mm. it becomes sufficiently intense to be examined. The spectrum is a continuous one with shaded bands. This spectrum gradually increases in brilliancy down to a pressure of 30 mm. If the pressure is diminished still more the three lines $H\alpha$, $H\beta$, $H\gamma$, rise more and more from the continuous ground and the band spectrum gradually disappears. Under 3 mm. pressure only these three lines are seen, but if we continue to exhaust the tube, the continuous spectrum reappears in the green. Finally at the lowest pressure under which the spark passes a third spectrum of brilliant lines is seen. A short historical sketch will show in how far the above statement is in harmony with the results of Plucker, and will justify at the same time the course of experimenting which I have pursued.

The spectrum of hydrogen obtained by the passage of the induction current through vacuum tubes was first examined by Plucker in the year 1858, and described by him as follows:[†]—

Hydrogen gave a comparatively simple spectrum, in which five bands of about the same width are most prominent; a violet band A at the limit of the spectrum. Three bands in the green, B forms the limit towards the violet, and D towards the yellow. C is double the distance from D than from B. The fifth band E is in the yellow. The most intense of the three green bands is D, the colour of which is already yellowish. Next in intensity is C, and then comes B. The red is very prominent and extends over a wide space; a well-defined broad black line situated near its external limit; another broad black line separates it from the yellow band E.

The description of this spectrum agrees in its general features with the continuous spectrum described by Wullner, and it is therefore important that already in August of the same year Plucker[‡] publishes a second paper, in which he says that the above experiments had only been preliminary ones, and that the hydrogen had not been pure. Pure hydrogen, he says, shows only three lines, a red one, a greenish blue one, and a violet one which is less bright. Plucker makes repeatedly the remark that the purer the hydrogen the more concentrated is its light[§] on the above three lines, and he expresses the opinion that the bands seen in impure hydrogen are due to air. Later on,[‖] when he was looking out for different spectra of the same gas, he found that this spectrum cannot be due to air, and must therefore be a second spectrum of hydrogen. It must, however, be remarked, that Plucker never found this spectrum so well developed as Wullner, and this cannot be due to the fact that he did not examine it under the same circumstances, for he describes[¶] the change which the spectrum of hydrogen gas undergoes when the pressure is increased, and only mentions the expansion of the lines. Although the Leyden jar was introduced in these experiments, and the second spectrum would not therefore have appeared, it is not to be expected that such a widely different spectrum which changes entirely the colour of the spark should have escaped the notice of such an observer as Plucker when he was looking out for different spectra. The most important recent contribution to the history of this spectrum has been made by Angstrom,[*] who found that when the spark passing through a hydrogen vacuum giving the band spectrum, and at the same time, the three well-known lines (as is generally the case), is examined in a revolving mirror, two different images are obtained corresponding to the two spectra. Two different discharges must therefore take place in the tube, one of very short duration, which corresponds to the line-spectrum, and one which lasts much longer, and corresponds to the band spectrum. As this spectrum is, moreover, according to him, identical with the spectrum of acetylene obtained by Berthelot, Angstrom thinks himself justified in ascribing this band spectrum to acetylene.

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[*] Fortchrift der niederrheinischen Gesellschaft zur Feier des 50 jahrigen Subilaums der Univ. Bonn.

[†] Pogg. Ann. civ. p 113.

[‡] *Ibid.* cv. p. 76.

[§] *Ibid.* cvii. p 507 and p. 518.

[‖] Ph. Trans 1865, vol clv.

[¶] Phil Mag. xlii. p. 395.

My own experiments lead to the confirmation of Angstrom's supposition that the second spectrum of hydrogen is due to hydrocarbon. Generally two distinct causes, which we shall consider separately may introduce a hydrocarbon into the vacuum tube :—

1. The gas passing through india-rubber tubes will carry with it small pieces of india-rubber.

2. All the tubes are more or less greasy, and it is almost impossible to remove this greasiness entirely.

These two causes are sufficient to produce such effects as are observed by Plucker, but the spectrum obtained in consequence of these impurities will necessarily vary so much with different tubes, that the presence of impurities will soon be recognised as it was by Plucker. Such constant effects as those observed by Wullner must, however, be due to a more constant cause, and we have not to look out long for such a cause. When Wullner examined the spectrum of oxygen, he discovered two new spectra, and even before Angstrom had expressed his doubts as to the correctness of these spectra, Wullner had found out that they were due to carbon or carbon compound, which were introduced into his vacuum tubes by the greased stop-cocks by which they were shut. Thus by the results obtained with oxygen it is proved that carbon compounds were introduced into his vacuum tubes, and we have only to consider whether these carbon compounds, which were sufficient to change entirely the spectrum of oxygen, are sufficient to make themselves perceptible in hydrogen. But Berthelot has shown that benzole mixed with a large quantity of hydrogen gives a spectrum of bands, and that acetylene when mixed with a sufficient quantity of hydrogen is not decomposed by the electric current.

It seems at first sight improbable that a gas passing through an india-rubber tube should carry with it a sufficient quantity of solid particles to change the appearance of the spectrum ; but Tyndall,[†] in his experiments on actinic clouds, mentions the effect produced by an india-rubber joint through which the gas subjected to examination had passed. The quantity of matter carried away by a current of air passing through an india-rubber tubing is not so small as might at first sight appear. The following experiment shows this. Take a piece of such tubing, and fix a small piece of glass tube to one of its ends. If the air is now sucked in through the glass tube the taste of india-rubber will make itself at once perceptible, although the mouth is not in direct contact with it. When I examined the spectrum of hydrogen, which had thus passed through an india-rubber tubing, the spectrum did not vary sensibly under different pressures. The colour of the spark was whitish, even under a pressure of 2 mm. The spectrum, evidently the band spectrum, described by Wüllner, did never show itself so well developed as is described by him, and resembled much more the spectrum as observed by Plucker. In different vacuum tubes the spectrum had the same appearance ; and I concluded therefore that the cause of impurity must lie in the tubing, which was therefore removed.

The vacuum tube was now fitted into the receiver containing the hydrogen, and sealed off at one end as soon as filled with the gas. This way of proceeding, however, presented many practical difficulties, and generally, therefore, another way was adopted. A drop of water was introduced in the vacuum tube, and after the vacuum had been made, the water was boiled, and the tube exhausted again, this being repeated until all the water had been evaporated and the air driven out. The spark decomposed the vapour, and the spectrum of hydrogen was thus obtained. Its appearance now varied much with the different vacuum tubes. One of them was carefully washed, first with sulphuric acid and then with distilled water. The spectrum obtained in this tube showed the continuous spectrum only so faintly that I think it would have escaped the notice of any observer who was not looking out for it. The influence on the colour of the spark was, however, much greater. The spark did not show that saturated red colour characteristic of hydrogen, but it was always somewhat whitish. This, however, is the nearest approach to the pure spectrum of lines which I could obtain. It is thus rendered at least highly probable that organic impurities are the cause of the observed spectrum of bands, and Angstrom's supposition that it is acetylene is very plausible. Wullner examining the spectrum of acetylene, says that it is identical in the red and yellow with the second spectrum of hydrogen, but that it differs from it in the green and blue. He concludes that the acetylene is decomposed into hydrogen and carbon, that the hydrogen shows its yellow bands, and the carbon its green and blue bands. Now these green and blue bands are not at all due to carbon, but to carbonic acid, as stated by Wullner himself, and only prove that carbonic acid was present. That the acetylene was decomposed is a supposition, which is in contradiction to Berthelot's results, mentioned above.

In order to ascertain whether the last traces of air remaining in the tube might not have an influence upon the spectrum of hydrogen, I tried to obtain the spectrum of ammonia, for this is the only compound which might possibly be formed, as aqueous vapour is always decomposed. Plucker says that ammonia also is decomposed into its elements, but I succeeded in obtaining its spectrum by the following arrangement :—A few drops of a strong solution of ammonia in water are introduced into the vacuum tubes, and the induction current is allowed to pass while the pump is being worked. Thus a vacuum is obtained sufficient to allow the passsage of the current, and at the same time the gas is constantly renewed. The spectrum of ammonia is very remarkable ; while nitrogen shows a spectrum of more than 70 lines, and the four lines of hydrogen are distributed over the whole spectrum ; a combination of these two gases when luminous gives out a perfectly homogeneous light. The colour of the spark is greenish yellow, and when examined by the spectroscope, it shows a broad band of exactly the same colour. The red and blue part of the spectrum is completely dark, while in the yellow and some parts of the green a faint continuous spectrum is seen. The wave-length of this band was determined by interpolation to be 5,686 to 5,627 x^{th} metres, and the band is therefore placed at the more refrangible end of the strong terrestrial absorption band called by Brewster δ. The spectrum of ammonia bears a strong resemblance to the spectrum of sodium, when at a high temperature its lines begin to widen.

Wullner has discovered a third spectrum of hydrogen, which is a spectrum of lines, and appears under the lowest pressure which can be obtained. Not having a Sprengel's pump at my disposal, I could, unfortunately, not obtain this spectrum, and I merely mention here that it appears, from various remarks of Plucker, that he examined hydrogen under the precise circumstances under which this third spectrum ought to have come out. Thus, he says,[*] that hydrogen allows the induction spark to pass at a lower pressure, than any other gas. At another place he says that when a tube filled with dry air is exhausted as far as possible, the lines of *hydrogen* and silicium appear, the lines of hydrogen being due to the hygroscopic condition of the glass. Angstrom believes this spectrum of lines to be due to sulphur. This is in harmony with the fact that small pieces of caoutchouc (containing sulphur) form part of the sources of error ; but Wullner has shown that although all the lines of his spectrum of hydrogen seem to coincide with strong lines of sulphur, some strong lines of sulphur do not appear, and according to Wullner, the spectrum has in its general character no resemblance to that of sulphur. At any rate, it seems improbable that if this spectrum is really due to hydrogen, it should have escaped the notice of Plucker.

If we inquire now what bearing these results have on the general question of double spectra, we must remark that two different subjects have been mixed together. We have first bodies which are gases at the ordinary temperature, such as nitrogen, hydrogen, oxygen. The question, whether these bodies can give different spectra under different circumstances, must, I think, be answered in the negative. This was the opinion expressed by Angstrom from the beginning ; and although this physicist clearly obtained all the results mentioned by Plucker and Wullner, they seem always to have left the conviction in his mind, that they are due to impurities. But there are other bodies, such as iodine, sulphur, and bromine. The existence of two spectra, in the case of iodine and sulphur, seems to be satisfactorily established by the researches of Mr. Salet. One of the spectra is the reversal of the absorption bands of the vapour of these bodies; while the others are spectra of lines. In the coloured vapours, the molecular constitution must be much more complex than in gases, and it is, therefore, not astonishing that such a coloured vapour should exert an absorptive influence resembling more that of a liquid than of a gaseous body. It would, therefore, show absorption bands, and if luminous, it will not show a spectrum of lines, but a spectrum of bands. At a higher temperature, however, when the vapour becomes a gas, the spectrum may change to one of lines. As a fact bearing upon the subject, I may mention the discovery made by Kundt, that nitrogen tetroxide gives the same absorption bands, whether in the form of a liquid, or in that of a vapour.

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[*] Phil Trans. 1865.

[†] Tyndall, "Radiant Heat," p. 341.

[*] Phil. Trans. 1865.

Preliminary Report of the Committee on Inverse Wave-Lengths.

The reference of spectral lines to a standard scale of wave-numbers, instead of to a scale of wave-lengths in air of a given pressure and temperature, or to any of the other scales in use, has very marked advantages. The scale of wave-numbers furnishes to the theoretical inquirer the ratios between wave-lengths,—which are what he chiefly wants—in the simplest and most conspicuous form; since a series of rays, of which the wave-lengths are in geometrical proportion, will be represented by equidistant lines upon the map. Accordingly the committee decided on reducing to wave-numbers all the wave-lengths, whether of solar lines or of the rays of incandescent vapours, which have been determined with sufficient precision. Mr. C. E. Barton has offered his services gratuitously for making the necessary reductions, and has made considerable progress with the solar spectrum. A specimen of the catalogue of solar lines was appended to this report, containing the lines from E to *b*. It is intended that this catalogue shall contain the most useful information available—namely, references to the position of each line on Kirchhoff's or Angstrom's maps, details of the processes by which the standard wave-numbers have been deduced, and indications of the intensity, width, and origin of each ray whenever these have been determined. The rays will, moreover, be bracketed into the groups that strike the eye in looking at the spectrum, and a number will be assigned to each group which will sufficiently indicate its position on the standard scale. It is estimated that the two catalogues—namely, the catalogue of the principal lines of the solar spectrum, and the catalogue of rays of incandescent vapours, will cost about 60*l.* The committee think that they could render the second catalogue more perfect if they were in a position to employ a competent person to revise and extend the determination of the rays of incandescent vapours, and they therefore suggest that this revision be made a part of their functions, and that an addition of 50*l.* be made to the grant for this purpose. This would increase the sum to be granted this year to 150*l.* The committee accordingly recommend that they be reappointed, and that this sum be placed at their disposal, in addition to the balance at present in their hands.

SECTION B—CHEMICAL SCIENCE

Thursday, Aug. 15.—At the close of the President's address, Dr. Wright read the *Report of the Committee for Investigating the Chemical Constitution and Optical Properties of the Essential Oils used for Perfumes*, which was followed by a short discussion, in which Mr. Hanbury pointed out the necessity of paying particular attention to the characters of the samples of the various oils taken for experiment, and he instanced several essential oils classed under the same name but of widely different origin and possessing wholly different properties. He was under the impression that sufficient care had not been taken in the selection of the samples with which experiments had hitherto been worked.

Prof. Mallett, of Virginia, U.S., exhibited some specimens of fused metallic arsenic which he had prepared by fusing the metal under great pressure. The fact that arsenic can thus be fused has already been determined by Landolt.

Prof. Mallett also gave an interesting *résumé* of his experiments on the nature of the gases occluded by meteoric iron. The method employed by him was essentially that of Graham, the meteorite being heated in a vacuum and the evolved gases removed by the Sprengel pump. The nature of the iron remaining was carefully examined, and it appeared that the heat modified the metal in a remarkable manner, principally as regards its capability of being forged. The original meteorite could be readily forged and beaten out into a tolerably perfect blade for a paper-knife, but on strongly heating the iron so as to drive off the occluded gases (principally hydrogen, carbonic acid, and carbonic oxide) it became cold-short, and could not be forged even with extreme care. The cause of the remarkable alteration in the tenacity of the metal gave rise to some discussion, from which it generally appeared that it was due to an alteration of the schreiberite in the meteorite.

Mr. W. L. Carpenter gave a short account of the methods employed in the recent dredging operations for collecting deep sea water. In the discussion which followed Prof. Crum-Brown described the method to be followed in the forthcoming expedition. The apparatus had been proved to work exceedingly well in shallow water, but it remained to be seen how far it could be used in the open ocean and under great pressure.

Prof. Thorpe exhibited a modification of the filter-pump recently described by Mendelejeff. It acts upon the principle of the hydraulic ram, and by means of a fall of water of less than a yard a vacuum of nearly 700′ can be readily obtained. The modification consisted in the nature of the valve employed and in the method of determining the degree of exhaustion. The instrument has the advantage of portability and readiness of construction over the older form of Bunsted (which requires a fall of water upwards of 30 ft.), and is likely to come into general use.

Dr. Russell read a paper prepared by Dr. Moffatt on the tube ozonometer, which elicited some discussion as to the value of ozonometric observations conducted by means of iodide of potassium papers. The use of such papers was generally condemned by the members of the Section; but the meeting arrived at no definite conclusion respecting any other way in which the liberation of iodine may be utilised as a measure of ozone.

Mr. C. J. Woodward exhibited and described a very simple and cheap modification of Hofmann's Apparatus for the Electrolysis of Water.

Friday, Aug. 16.—Prof. G. C. Foster presented the *Report of the Committee appointed to investigate Siemen's Electrical Pyrometer.* The Committee had principally confined themselves to the determination of the constancy of the resistance of the platinum coil at high temperatures. Before the instrument can give perfectly concordant results it is absolutely necessary that the coil should be heated and cooled a great number of times. As some further alteration in the instrument was contemplated, the Committee recommended that a further application should be made to the Association in order to continue the investigations. In the discussion which followed, Mr. Dewar suggested the employment of a spectroscope with a compound prism of quartz and calc spar in the measurement of high temperatures.

Prof. Williamson read the *Report of the Committee for Superintending the Monthly Report on the Progress of Chemical Science.* The meeting cordially testified to its sense of the value of the Chemical Society's work in furthering the spread of chemical knowledge by the publication of its admirable series of abstracts of chemical memoirs published in the leading journals. Dr. Williamson assured the Section that the movement was rapidly becoming self-supporting, and that in a few years it would be no longer necessary to request the Association to supplement the funds at the disposal of the Chemical Society for the purpose.

Mr. Dewar described some experiments on the determination of the specific heat of carbon at high temperatures. The method of calorimetric measurement differed in no essential particulars from that usually employed. The temperatures employed were those of boiling zinc (1040°) and of the oxyhydrogen blowpipe which Mr. Dewar, by the method of Poullet and Deville and Troost, found to be about 2200° C. Between 0 - 1030 the mean specific heat of carbon was found to be 0·32, between 0 - 2000 upwards of 0·4. Mr. Dewar explained the variation in the temperature of the oxyhydrogen flame as obtained by Bunsen himself by the difference of pressure under which the combination of the two gases occurred in the two sets of experiments. His results on the specific heat of carbon in the main agree with those recently published by Weber. Starting from the difference in the heat of combination between carbon and oxygen to form respectively carbon dioxide, and monoxide, and making a certain assumption for the latent heat of carbon, Mr. Dewar arrives at the conclusion that the boiling point of carbon cannot possibly exceed 8000° C, and in all probability is somewhat near 7000° C.

Dr. Gladstone read a paper, prepared in conjunction with Mr. Tribe, *On the Mutual Helpfulness of Chemical Affinity, Heat, and Electricity in Producing the Decomposition of Water.* Dr. Gladstone commenced by describing the action of various metals upon water: some are able to eliminate the hydrogen from water, whilst others, and by far the larger number, are unable to do so. Zinc, if perfectly free from foreign metals, is without action on water; but if it be brought into contact with another metal even more stable in regard to its action on water, the electrical tension *plus* the chemical tension upsets the equilibrium between the atoms in the molecule, and hydrogen is eliminated. The effect of varying the distance between the plates was carefully measured, and it was found that the chemical action increased slowly up to a certain point, after which the action rapidly increases as the metals are brought into closer contact. Copper deposited on zinc foil is a very effective combination, and its action is materially accelerated by the meeting; thus, at 2° C. only 1 cbc of hydrogen was evolved per hour; 62 cbc were illuminated per hour at 55°, whereas at 93° C. as much as 528 cbc were produced. With magnesium and copper the action is even more marked. These re-actions afford methods of preparing exceedingly pure hydrogen, and they will doubtless be found useful in many operations of reduction.

Mr. Weldon described his process for the manufacture of chlorine by means of manganite of magnesium. The manganite is first produced by neutralising an acid solution of manganese chloride with Greek stone. By treatment with hydrochloric acid the manganite yields chlorine and magnesium and manganese chlorides. The solution is run out of the steel into an iron pot, and is afterwards boiled down until it reaches a temperature above 300° F., when it is run into a blind furnace and evaporated to dryness. On heating the dried residue chlorine and hydrochloric acid are evolved, and the manganite of magnesium is reproduced.

SECTION C.—GEOLOGY

The first paper was that by Prof. E. Hull, *On the Raised Beach of the North-east of Ireland.* All along the eastern coast of Ireland, from Dublin Bay northwards, there are to be found at intervals distinct evidence that the coast has been raised in recent times. This evidence is divisible into two kinds; first, the occurrence of a narrow fringe of varying elevation, forming a terrace extending for some distance inland from the coast, and composed of stratified sands and gravel, containing marine shells belonging to species now inhabiting the Irish Sea; and secondly, the existence of old sea-worn cliffs, forming the inland margin of these terraces, which are now beyond the reach of the highest tides. In the north of Ireland these cliffs are penetrated by caves, which have yielded bones of animals, some of which are extinct in that part of the country, while the gravels of the old beach contain amongst the sea shells worked flints in considerable quantity in County Antrim, which prove the elevation of the coast to have taken place since the human period.

The height attained by the beach above the present sea level is about 8 ft. in the south, but it rises gradually northwards, and there attains a height of 20 ft. The author considered this to be of the same age as the twenty-five feet beach of the west coast of Scotland, which falls somewhat in level towards the Solway; southwards this decrease in level continues, till the evidences of a raised beach almost disappear towards the estuary of the Mersey. The identity, therefore, of the phenomena on both shores is evident, and is a matter of some interest in the physical geology of these islands.

In the discussion which followed, Prof. Harkness, Mr. Pengelly, and the Rev. W. H. Crosskey took part, the last speaker insisting strongly upon the necessity of following these accumulations inland, and not confining our observations to the more attractive sections along the coast.

Mr. Jas. Howell then described the *Super-Cretaceous Formations of the Neighbourhood of Brighton,* in which the various deposits of the district were minutely described. Attention was called to the outlines of Tertiary beds on Furze Hill, and to the still smaller patches scattered over the Downs. The author, during the numerous excavations made in draining the town of Brighton, had observed that wherever brick-earth occurs with "Coombe rock" it is always the newer deposit of the two. From the deposits met with, in the lower parts of the town, Mr. Howell concluded that the Brighton valley, at least as far up as the London and Lewes Road, was once covered by the tides.

Mr. W. Topley followed with an account of the *Sub-Wealden Exploration.* He first gave a brief description of the Weald and of the beds therein exposed, dwelling more particularly upon the lowest known rocks, the Ashburnham beds, in which the boring commences. He then described the older rocks as exposed in and around the coal-fields of Bristol and South Wales on the west, and the Belgian coal-fields and the Lower Boulonnais on the east. These rocks, it was stated, would certainly pass beneath the Weald, and along with them would probably occur workable coal measures, but the exact position of these last is a great uncertainty. The thickness of rock at the bore-hole, before reaching the Palæozoic beds, might be only 700ft. or it might amount to 1,600ft. The author drew special attention to the parts taken by Mr. Godwin-Austen and Mr. Henry Willett in this exploration. To the philosophic papers of the former we owe our knowledge of the underground range of the older rocks, and to the energy and perseverance of the latter is due the fact that speculation on this subject is about to give rise to actual experiment.

Mr. Godwin-Austen traced the area occupied by the old coal forests of Western Europe, and described the means by which this once united area had become broken up into separate basins. The axis of Artois and the coal-fields along its line were then more particularly noticed. He stated that carboniferous limestone had been found at a small depth in the Pays de Bray, beneath Kimmeridge clay, the whole of the lower members of the oolite series being there absent. In the area between the Pays de Bray and the Boulonnais, and under the Weald on the west of that, it was possible that coal-measures might be preserved. He protested against the sub-Wealden exploration being represented as a "search for coal;" its only object was to explore the rocks underlying the Weald.

Mr. Henry Willett gave an account of the origin and progress of the undertaking, stating that it was planned in honour of the first visit of the British Association to Sussex. He repeated Mr. Godwin-Austen's protest as to this being a search for coal, and said that this bore-hole was only the first of a series which would ultimately be necessary to complete our knowledge of the range of the Palæozoic rocks. He added that the subscriptions to the fund now amounted to 1,900*l.*

Mr. Harry Seeley entered at some length into his reasons for disbelieving that the coal-measures ever covered this area; but he, in common with other geologists, was very glad of the experiment now being made, as its results would have a very high scientific value, although commercially it would, he believed, prove a failure.

The remainder of the time was occupied by the reading of Mr. G. A. Lebour's paper *On the Geological Distribution of Goitre in England.* The author had by inquiries and correspondence collected a great amount of information upon the distribution of this disease, and his facts are of the more importance, as no information can be obtained upon the subject from Government statistical returns. He traced in detail the range of goitre over the various formations, and showed that the accepted beliefs on this subject were frequently erroneous. Thus, as regards magnesian limestone, which is commonly believed to be a very goitriferous rock, he showed that goitre was by no means so common there as in some other formations. Again, whilst on some regions occupied by carboniferous limestone the disease abounds, in others, where the general character of the rock is apparently the same, it is entirely absent. In searching for a general cause regulating the distribution of goitre, the author rejected as insufficient that generally given—the hardness of water. He showed it to be more probable that metallic impurities in the water were the cause. The carboniferous limestone was characterised by goitre almost in exact proportion to the metalliferous nature of the rock. Districts where ferruginous water occurs very commonly have goitre, particularly where the iron is derived from the decomposition of iron pyrites.

Friday, August 16.—The proceedings of this section opened with the reading by Mr. Pengelly of his *Report on Kent's Cavern, Torquay.* Specimens of bones and flint implements during the preceding year were exhibited to the meeting. Mr. Pengelly afterwards read a note *On the Occurrence of Machairodus latidens at Kent's Cavern.* This animal had been found there many years ago by Mr. M'Henry; but doubts had often been expressed as to the accuracy of this observation, and it was highly satisfactory to find that recent researches had confirmed M'Henry's discovery.

Mr. Evans made some remarks upon the flint implements found during the past year.

Prof. A. Gaudry described the various species of Machairodus, and Mr. W. Boyd Dawkins remarked upon the range of this animal, stating that the genus certainly occurred in the forest bed of Norfolk, although the species there was doubtful.

Dr. Carpenter followed with his paper *On the Temperature and other Physical Conditions of Inland Seas, considered in reference to Geology.* The general results obtained by recent deep-sea soundings were first described, the author afterwards passing to the special subject of the paper. Where seas were shut off by a narrow and comparatively shallow barrier from the great ocean it was found that the lowest bottom temperature of such seas was controlled by the lowest winter temperature of the surface. The bottom temperature of the Red Sea is about 71°, and since it appears that the distribution of reef-building corals depends not so much upon depth as upon temperature, we may expect to find them living at much greater depths in the Red Sea than anywhere else in the world. The great rivers flowing into the Mediterranean bring down a great quantity of organic matter, and the decomposition of this carries off much of the oxygen from the deeper water. Probably to this fact is owing the scarcity of life at great depths within that sea

The author pointed out the bearing of these and similar facts upon geological speculation, and in these remarks he was followed by Prof. Phillips, who spoke of the great light which Dr. Carpenter's researches were throwing upon geology, explaining as they did how great areas of sea-bottom might be almost destitute of life, just as we find great deposits of rock to be.

SECTION D—DEPARTMENT OF ZOOLOGY AND BOTANY

Report of the Committee appointed for the purpose of promoting the Foundation of Zoological Stations in different parts of the world.

The Committee report that, as stated in the Report of the last meeting, the Zoological Station at Naples will be ready and in working order at the beginning of January 1873, the progress of the construction being such as to enable Dr. Dohrn to make this assertion.

This undertaking has received much official and private assistance, not only from public authorities, but in a very high degree from private persons. The Committee feel obliged to acknowledge especially the extraordinary services rendered by Mr. W. A. Lloyd of the Crystal Palace Aquarium in giving every assistance to Dr. Dohrn in so far as technical difficulties are concerned.

Special care has been taken to secure donations to the library of the Station. The eminent firm of Engelmann in Leipsig has presented all its works on Biology not previously possessed by Dr. Dohrn. Veweg in Brunswick has also sent all his publications on Biology. Theodor Fischer in Cassel has done the same. Important donations are promised by Dr. Alexander Agassiz of Cambridge, Mass., comprehending the publications both of his father and himself.

To secure the development of the library on a greater scale, it will be necessary to make general applications. For this purpose Dr. Dohrn, assisted by several of the greatest German publishing firms, is preparing an appeal to all German publishers, and he hopes also to succeed with a similar demand in Italy. The Committee hope that the British Association will lend its moral support to a similar demand in this country, not only by granting a complete set of its own publications, but by recommending a similar act to other Scientific Bodies and private persons.

The Committee are further glad to announce that some steam-navigation companies are prepared to grant a free passage to the naturalists and free transport for the goods to and from the station. As transactions are still pending between these Companies and Dr. Dohrn, the latter does not think it desirable to publish details on this point, or to mention the names of the Companies in question.

Dr. Dohrn contemplates a new step for the purpose of returning a larger income for the Naples Station. He is about to offer to several Governments, Universities, and Scientific Bodies, working tables in the laboratory of the Station for a certain annual sum. This sum would confer on the subscribing Government, University, or Society, the right of appointing a naturalist, who, on presenting a certificate to the administration of the Station, would be furnished with a working table, and admitted to a participation in all the very extensive advantages of the Station.

The Committee think well earnestly to advocate this new step of the administration of the Naples Station, the more as it lessens the burden of the single naturalist, enabling even such as are destitute of means to profit by the manifold advantages of the Station, while it secures a fixed income to the Station which would be employed in improving the technical and other means of investigation.

Mr. Lankester gave some additional account of the Zoological Station about to be established by Dr. Dohrn at Naples. During the present year he had personally had the opportunity of seeing the arrangements which were in progress.

"On the narrow strip of coast which separates the park of the Villa Reale from the sea, a large stone building is at present being erected at Naples, quietly and almost unnoticed—at least the Neapolitan press has paid no attention to it. The strength of the foundations—it has taken three months to lay them—shows that they are intended for an edifice of considerable size and durability; and on making inquiries I have learnt that this is the Zoological Station, which has been occasionally mentioned by Italian, German, and English journals during the last few months. It has been organised and is being built by a young German naturalist, Dr. Anton Dohrn, of Stettin, who, until a few years ago, was a *privat docent* at the University of Jena. He has paid nearly the whole of the expenses, which amount to about 50,000 thalers (7,500*l.*), out of his own pocket, the only assistance he has received having come from a few personal friends, who have lent several thousands of thalers for the purpose. The following is a short sketch of his plan:—The ground floor of the building, which covers an area of almost 8000 square feet, contains a great aquarium, which will be opened to the public. Dr. Dohrn hopes that the money thus obtained will not only suffice for all the expenses of the aquarium, but also afford a surplus to be employed in covering a part of the requirements of the upper story, which is to be exclusively devoted to scientific purposes. Besides the officials and servants employed in the aquarium, several young zoologists will be attached to the Station, and receive a regular salary from the director, Dr. Dohrn. Thus, a number of new positions will be opened up for young scientific men. But this is not all. As the only duty of these zoologists will be to devote themselves to certain branches of scientific work, and their exertions will be carefully directed and organised, as has long been the case in astronomical and meteorological observatories, there is every reason to hope that scientific research will be greatly facilitated and advanced by their labours. In the upper story of the Zoological Station laboratories will also be prepared for the use of naturalists coming from other parts of Italy and from abroad. For this purpose a large scientific library will be founded, Dr. Dohrn's very considerable private collection serving as a nucleus, and about twelve tables, fully furnished with the necessary appurtenances, established. Each of the latter will be provided with a number of tanks supplied with a constant stream of sea-water. Sea-fishing and dredging will be conducted on an extensive scale by means of several boats, to which, if the necessary means are forthcoming, a small steam-yacht will be added. The animals taken will be given to the zoologists for scientific treatment. It is more than doubtful whether all these rich and expensive conveniences can be furnished to zoological visitors without any pecuniary compensation; but I hear that Dr. Dohrn has drawn up a plan which will enable even naturalists of limited means to enjoy the advantages of the Station. He proposes to offer one or more tables to various governments and scientific societies for a fixed annual sum. These tables, and all the scientific resources of the Station, will at once be placed at the disposal of any naturalist who brings a certificate from the government, university, or scientific body to which the table has been let. This plan, among its many other advantages, seems to be a successful attempt to solve the difficult question as to how it is possible to unite a complete self-administration on the part of scientific bodies with the reception of pecuniary assistance from their governments.

"Dr. Dohrn speaks in the most grateful manner of the assistance rendered him by the German authorities in Italy, especially by Mr. Stolte, the consul-general at Naples, while at the same time he warmly acknowledges the interest in his undertaking displayed by the government of Italy, more particularly Signor Correnti and Signor Sella, the late and the present Ministers of Public Instruction. The difficulties in the way of the execution of his plan were neither few nor small, as may be gathered from the fact that, in spite of the readiness displayed by the municipal authorities of Naples, more than two years elapsed before a definitive contract could be concluded between the town and Dr

Dohrn with respect to the cession of a suitable site for the building."

Report of the "Close Time" Committee.

The Committee re-appointed at Edinburgh, for the purpose of continuing the investigation on the desirability of establishing a "Close Time," for the preservation of indigenous animals, report as follows:—

Believing the time had come for advantageously urging the Legislature to take further action whereby the object for which your Committee was appointed might be promoted, your Committee, after due consideration, prepared a bill, intituled an Act for the Protection of Wild Fowl, which being entrusted to the care of Mr. Andrew Johnston, M.P., was by him, Colonel Tomline, M.P., and Mr. Brown, M.P., brought into the House of Commons on February 15, and read a first time.

This bill was based on the "Sea-Birds Preservation Act of 1869," and *mutatis mutandis* only, strictly followed the provisions of this Act, which experience has shown to have fully effected the object for which it was passed, and to have given very general satisfaction to the country at large.

On the motion for the second reading of the bill in the House of Commons, June 12, the Hon. Auberon Herbert, M.P., proposed as an amendment that it was "desirable to provide for the protection of all wild birds during the breeding season;" but this amendment, which would have been fatal to the bill, was withdrawn, the bill was read a second time and ordered to be committed, June 21.

In the debate in the House of Commons on the notice for going into Committee, Mr. Herbert moved, according to notice, "That it be an instruction to the Committee that they have power to extend the protection, given under the bill to Wild fowl during the breeding season, to other wild birds." The House divided: Ayes 20, Noes 15; and thereupon Mr. Herbert moved a number of other amendments of which he had given notice, and these being accepted by the House, the bill, instead of being the moderate measure contemplated by your Committee, became one of general and indefinite scope.

By this means the fate of the bill, which had hitherto met with no serious opposition, was rendered very uncertain; and notice was given of a motion to throw it out; but on the report being taken, the bill on Mr. Johnston's proposal, was referred to a Select Committee, by whom it was still further modified; the objections urged against its sweeping clauses being overcome by limiting its effects to certain kinds of birds named in a Schedule, while the penalties for its infringement were diminished. In this form it went back to the House of Commons, and with a few other alterations finally passed that House, and was sent to the House of Lords.

In the Upper House, charge of the bill was taken by the Earl of Malmesbury, and, some fault being found with it, its provisions were further altered in committee, a person convicted of a first offence being rendered liable to a reprimand and the payment of costs and summons only. Thus modified it was returned to the House of Commons, and has since received Her Majesty's assent.

Your Committee cannot look with unmixed favour on this measure. It appears to them to attempt to do too much, and not to provide effectual means of doing it. In their former Reports they have hinted at, if not expressed, the difficulty or impossibility of passing any general measure, which without being oppressive to any class of persons, should be adequate to the purpose. Further consideration has strengthened their opinion on this point. They fear the New Act, though far from a general measure, will be a very inefficient check to the destruction of those birds, which, from their yearly decreasing numbers, most require protection, its restraining power having been weakened for the sake of protecting a number of birds which do not require protection at all. Your Committee have never succeeded in obtaining any satisfactory evidence, much less any convincing proof, that the numbers of small birds are generally decreasing in this country. On the contrary they believe that from various causes, many if not most species of small birds are actually on the increase. They are therefore of opinion that an Act of Parliament proposing to promote their preservation is a piece of mistaken legislation, and is mischievous in its effect, since it diverts public attention from those species which through neglect, indifference, custom, cupidity, or prejudice, are suffering a persecution that will in a few years ensure their complete extermination. At the same time your Committee are glad to state that such protection as is afforded by the new Act will be extended to the particular group of birds which in former Reports they have shown to require it most—all the wild fowl named in the bill prepared by your Committee, having being included in the schedule of the Act. It is also gratifying to your Committee to find that the principle of a "Close Time" for all birds has been admitted by the House of Commons, though the application of that principle may at present be inexpedient. Your Committee therefore trust that the Act will not be otherwise than beneficial in its results, and though greatly indebted to many noblemen and gentlemen for the assistance they have rendered, your Committee cannot refrain from especially thanking Mr. Andrew Johnston, for the skill and patience he has shown in the conduct of the bill introduced.

Your Committee respectfully suggest that they may be re-appointed.

Fourth Report on the Fauna of South Devon, by C. Spence Bate.

Attention had been principally directed to the development and habits of animals which had fallen under observation. This had been facilitated by the establishment at Plymouth of a marine pond as store for the Crystal Palace Aquarium. The observations had already proved interesting, and would become more so as the conditions of the pond became better adapted to Deep Sea species. It is formed out of a deep gulley in the limestone, partly extending back into a cave. At the entrance it is 11 ft. wide, and in other parts more than double; when the water is highest, its length is upwards of eighty feet. With the replacement of the original *Fucus* by green algæ, the water has become pellucid and clear. A list was given of the fish taken on the coast since the last report. Most of these have done well in the pond, the exceptions being fish of erratic habits, such as the mackerel. These, after restlessly roaming in search of an outlet, succumbed and died. Other fish thrive apparently unconscious of their confinement. The Blue Wrasse (*Labrus mixtus*) had exhibited marked sexual selection, a fact which had also been observed by Mr. Lloyd at Hamburgh. During the breeding time the male selects one out of many females, and afterwards regularly accompanies her. It had also been ascertained that the Blue Wrasse and the Spotted Wrasse were the same species. The male in confinement at Plymouth appears to be losing his fine colouration and approximating to that of the female; it seems, therefore, probable that the blue colour is more or less assumed at the breeding season.

With regard to the Crustacea, there are two subjects of interest. The first is the perceptible decrease in the numbers of the edible species, the decrease being more perceptible in the littoral than in the deep sea species. This arises from the custom of destroying the females as well as the males at all seasons of the year, and also from the preference given to the lobster for culinary purposes when laden with spawn. In the case of the crab (*Cancer pagurus*) there is not even this excuse. The marketable value of the female is at least one fifth that of the male. This arises from the smaller size, especially of the claws. Captured in greater numbers, they are wantonly destroyed, being hawked about the streets for a few pence. The capture of the lobster, he thought, should be interdicted from February until May, and that of the female crab altogether. To the assertion that the lobster and crab are so prolific as to render the destruction unimportant, there was the obvious reply, that in all those forms of life where the ova are most abundant, the development of the individuals is least quantitatively. In the case of the lobster, no one has ever seen that stage in its life which unites the animal as we know it with that which we have seen when it quits the egg, and, except the common littoral crab (*Carcinus mænas*), this is true of all the higher crustacea. Mr. Lloyd, of Hamburgh, has noticed that the male or soldier crab (*Pagurus*) in the spring takes hold of the shell containing the female, and carries it about for weeks together, and does not intercept its food as it would if a male were contained inside. He had found that crustacea might be preserved in a very superior way by keeping them in glycerine, and then drying them. Specimens preserved in this way two or three years ago were as flexible as if fresh. The soft parts should, if possible, be removed. He hoped to preserve fish in the same way. (Mr. Spence Bate subsequently remarked that after five or six years the structure of specimens preserved in glycerine appeared to become rotten. He suggested, therefore, the previous admixture with the glycerine of one-eighth of spirit of wine.)

Among the molluscs many species of *Aledone* had been captured. This was generally supposed to be a rare species, but

Octopus vulgaris proves to be the more difficult to obtain. Two specimens of *Sepia officinalis* were placed in the pond on the 8th of June, 1871. On the 12th July the female died, and was found to contain a large quantity of ova. Steps have been taken to have constructed in the cave behind the pond a case with a glass front for watching the habits of animals. The temperature of the water in the pond is several degrees below that in the tanks at the Crystal Palace.

The Mollusca of Europe compared with those of Eastern North America, by J. Gwyn Jeffreys, F.R.S.,

After mentioning that he had dredged last autumn on the coast of New England in a steamer provided by the Government of the United States, and that he had inspected all the principal collections of Mollusca made in Eastern North America, the author compared the Mollusca of Europe with those of Massachusetts. He estimated the former to contain about 1000 species (viz. 200 land and fresh water, and 800 marine), and the latter to contain about 400 species (viz. 110 land and freshwater, and 290 marine); and he took Mr. Binney's edition of the late Professor Gould's Report on the Mollusca of Massachusetts as the standard of comparisons. That work gives 407 species, of which Mr. Jeffreys considered 40 to be varieties, leaving 367 apparently distinct species. About thirty species may be added to this number in consequence of the recent researches of Prof. Verrill and Mr. Whiteaves on the coast of New England and the Gulf of St. Lawrence. He identified 173 out of the 367 Massachusetts species as European, viz. land and freshwater 39 (out of 110), and marine 134 (out of 257), the proportion in the former case being 28 per cent. and in the latter 52 per cent.; and he produced tabulated lists of the species in support of his statement. He proposed to account for the distribution of the North American Mollusca thus identified by showing that the land and freshwater species had probably emigrated from Europe to Canada through northern Asia, and that most of the marine species must have been transported from the arctic seas by Davis's Strait current southward to Cape Cod, and the remainder from the Mediterranean and western coasts of the Atlantic, by the Gulf Stream in a northerly direction.

Dr. Sclater said that it had always been an interesting problem how the similarity of the Fauna of North Europe and North America had been brought about, It was formerly supposed that a continuous land area existed between the two continents in the neighbourhood of Greenland. He was quite disposed to agree, however, that the communication had been brought about through the northern parts of Asia. In fact the fauna of Western America and of Eastern Asia had greater points of similarity than those of Northern Europe and Eastern America. For example, *Ursus horribilis* of Western America was intrinsically connected with *U. Arctos* if not merely a form of it. There were also some peculiar mammals which were identical; for example, an Insectivore *Urotrichus* was common to Japan and North Western America. With regard to birds several European types have turned up also in North Western America, a true Bullfinch for example.

Prof. Allman said that the distribution of Hydroids hardly accorded with that of Molluscs. Amongst recently collected *Tubularida* which he had examined, and all of which were new, two species only were common to both sides of the Atlantic. Our own islands, the coasts of Norway, Iceland, and Greenland, and the northern shores of the Atlantic down to Southern Nova Scotia belonged to one large province. At Florida all Eastern forms die out. Amongst the West Indian islands not a single species was common to both sides of the Atlantic. Looking at the facilities for distribution afforded by the locomotive buds of these organisms, these facts were not easy to understand.

Prof. Thiselton Dyer said that plant distribution quite supported the theory of communication between Asia and America. Prof. Asa Gray had shown that the Flora of Japan had a strong affinity to the North West American. Grisebach had endeavoured to invalidate this, but as Mr. Bentham said in his recent address, with little success. It was remarkable to find in a case like this an accordance between the facts of animal and plant distribution, because in the case of the Malayan Archipelago plants of the Indo-Malayan type extended far to the east of the limits Mr. Wallace found to exist in the case of animals.

Mr. M'Lachlan as confirming Dr. Sclater's remarks concerning the similarity of the fauna of Siberia and Eastern Asia with North America, stated that several genera of Insects are common to the two districts, though absent in Europe. He instanced especially the neuropterous genus Pteronarcys which formed the subject of a well-known memoir by Newport on the occurrence of external breathing filaments in the perfect kind of insect.

Mr. Jeffreys in reply, instanced some cases of marine shells which are common to the western or Pacific, and eastern or Atlantic coasts of North America; one of these is *Verticordia acutecostata*, which was at first known only as a tertiary fossil, but has lately been found living not only in the European seas, but also in the Gulf of Mexico, Japan, and probably Davis's Straits.

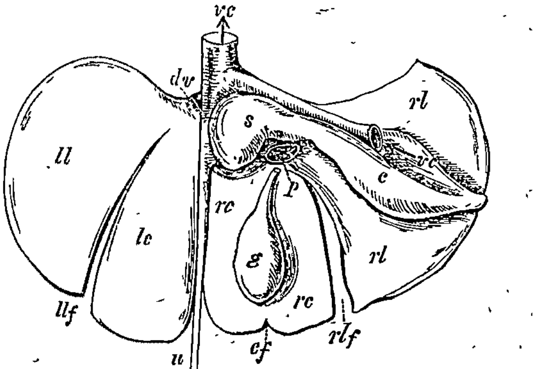
M. de Macklay had ascertained the existence of a species of sponge both in Japanese and Norwegian waters.

Department of Anatomy and Physiology

On the Arrangement and Nomenclature of the Lobes of the Liver in Mammalia, by Prof. W. H. Flower, F.R.S.

The description of the livers of various animals to be met with in treatises or memoirs on comparative anatomy are generally very difficult to understand for want of a uniform system of nomenclature. The present communication, which endeavours to supply such a system (and was illustrated at the meeting by a large series of coloured diagrams), is based upon an examination of the condition of the organ in examples of every important sub-division of the class. The difficulty usually met with arises from the circumstance of the liver being divided sometimes, as in man, ruminants, and the cetacea, into two main lobes, which have always been called respectively right and left; and in other cases, as the lower monkeys, carnivora, rodentia, &c., into a larger number of lobes. Among the latter, the primary division usually appears at first sight to be tripartite, the whole organ consisting of a middle, called "cystic," or "suspensory," lobe, and two lateral lobes, called respectively right and left lobes. This introduces confusion in describing livers by the same terms throughout the whole series of mammals, as the right and left lobes of the monkey, or dog, for instance, do not correspond with the parts designated by the same names in man and the sheep. There are, moreover, conditions in which neither the bipartite nor the tripartite system of nomenclature will answer, which we should have considerable difficulty in describing, without some more general system.

It appears desirable to consider all livers as primarily divided by the umbilical vein into two segments, right and left. This corresponds with its development, and with the condition characteristic of the organ in the inferior classes of vertebrates.



Description of Figure

Diagrammatic plan of the inferior surface of a multilobed liver of a mammal extended transversely. The posterior or attached border is uppermost. *u*, umbilical vein of the fœtus, represented by the round ligament in the adult, lying in the umbilical fissure; *d v*, the ductus venosus; *v c*, the inferior vena cava; *p*, the vena portæ entering the transverse fissure, *l l f*, the left lateral fissure; *r l f*, the right lateral fissure; *c f*, the cystic fissure; *l l*, the left lateral lobe, *l c*, the left central lobe; *r c*, the right central lobe, *r l*, the right lateral lobe; *s*, the Spigelian lobe; *c*, the caudate lobe; *g*, the gall bladder.

The position of this division can almost always be recognised in adult animals by the persistence of some traces of the umbilical vein in the form of the round ligament, and by the position of the suspensory ligament.

When the two main parts into which the liver is thus divided are entire, they may be spoken of as the right and left lobes;

when fissured as the right and left segments of the liver, reserving the term lobe for the subdivisions. This will involve no ambiguity, for the terms right and left lobes will no longer be used for divisions of the more complex form of liver.

In the large majority of mammals each segment is further divided by a fissure more or less deep, extending from the free towards the attached border, which I propose to call *right* and *left lateral* fissures (see Fig., *r l f*, and *l l f*). When these are more deeply cut than the umbilical fissure, the organ has that tripartite or trefoil-like form just spoken of, the part between them being he so-called middle, cystic, or suspensory lobe. These terms I should propose to discontinue, and to institute *right central* (*r c*) and *left central* (*l c*) for the two regions included between the umbilical and the two lateral fissures, and to use *right lateral* (*r l*) and *left lateral* (*l l*) for the regions beyond the lateral fissures. The essentially bipartite character of the organ, and the uniformity of its construction throughout the class, is thus not lost sight of, even in the most complex forms.

The left segment of the liver is rarely complicated to any further extent, except in some cases by minor or secondary fissures marking off small lobules, generally inconstant and irregular, and never worthy of any special designation. The principal differences to be noted depend on the degree of completeness of the lateral fissure (which sometimes extends quite across the hepatic tissue completely severing the left lateral lobe), and the relative size of the two lobes.

On the other hand, the right segment is usually more complex. The right lateral fissure when fully developed passes into the right extremity of the portal fissure. The right central lobe, therefore, on its under surface does not reach to the attached border of the liver; but is always bounded in that direction by the portal fissure. Moreover, the gall-bladder when present is always in relation to its under surface. The position of this receptacle with respect to the lobe may vary—sometimes it is merely applied to its surface, loosely connected by connective tissue; in other cases it is deeply embedded in a fossa. Very often it is placed near the middle of the lobe—sometimes close to one or the other of its lateral boundaries. In many cases the fossa in which the gall-bladder is sunk is continued to the free margin of the liver as an indent, or even a tolerably deep fissure. This is called the cystic fissure (*c f*); but in consequence of its irregularity of position and frequent absence it is not of the same importance as the other fissures I have named, and does not mark off any distinct divisions of hepatic substance.

The right lateral lobe always has the great vena cava (*v c*) either grooving its surface or tunnelling through its substance near the inner or left end of its attached border, and a prolongation to the left, between the vein and the portal fissure, has long been known under the name of the *Spigelian lobe* (*s*). This is always a distinct hepatic region, sometimes a mere narrow, flat track, but more often a prominent tongue-shaped process. Whatever may be its form, it is bounded in front, or towards the free surface of the liver, by the portal fissure; on the left by the fissure of the ductus venosus (unless the vessel is bridged over by hepatic substance); posteriorly and partially on the right by the vena cava, but between this vessel and the right end of the portal fissure it is continued onwards into the adjoining part of the right lateral lobe.

The main body of the right lateral lobe is most commonly divided into two parts, not by a cleft, such as the lateral fissures, passing from the upper to the lower surface of the liver, but by one which severs a part off from the under surface. This is the *caudate lobe*, and the fissure which separates it from the right lateral lobe may be called the "fissure of the caudate lobe." In man it is almost obsolete, but in most Mammals it is of very considerable magnitude, and has very constant and characteristic relations. It is connected by an isthmus at the left (narrowest or attached end) to the spigelian lobe, behind which isthmus the vena cava is always in relation to it, channelling through or grooving its surface. It generally has a pointed apex, and is deeply hollowed to receive the right kidney, to the upper and inner side of which it is applied.

For ready comparison I have found it convenient to tint the diagrams of different livers with the following colours:—The left central lobe, dark blue; the left lateral, light blue. Where the left lateral fissure is not present, as in man, the ruminants, and cetacea, the colours will shade into each other, or the whole segment may be made of a medium shade. The right central lobe, dark red; the right lateral, light red; the spigelian, yellow; and the caudate brown. By this method the homologous parts of every liver, and the essential similarity of their construction, however diverse in appearance, may be seen at a glance.*

SECTION F—ECONOMIC SCIENCE AND STATISTICS

Report of the Committee appointed on Uniformity of Weights, Measures, and Coins.

The Metric Committee of the British Association has much pleasure in reporting that another great stride has been made towards the uniformity in the weights, measures, and coins of all countries, by the passing of a law in Austria in June 1871, rendering the use of metric weights and measures permissive from January 1, 1873, and compulsory from January 1, 1876. The metric system is gradually diffusing itself all over Europe. At this moment fully two-thirds of that continent measured by population have adopted the metric system of weights and measures, and the other one-third has manifested sufficient interest in the question to justify the expectation of its early adhesion to the general agreement. But in this one-third there are comprised Russia and England, two countries which, by their population and commerce, exercise an enormous influence in the whole world.

The state of the question in Russia appears to be as follows:—In 1859 a Committee of the Imperial Academy of Russia, issued a report on the subject, which approved of the decimal division already incorporated in the Russian money system, and expressed an opinion in favour of extending such decimal divisions to weights and measures. In discussing, however, the possibility of even this moderate reform, the Academicians saw that such a considerable change would be required that they felt it would be far better for Russia at once to introduce the metric system, and this was the conclusion of their recommendations. Since the publication of the report, the Imperial Academy of Russia has taken an active part in advance of the system all over the world. In 1867 Mr. Jacobi was a member of the International Committee on weights, measures, and coins in connection with the Paris International Exhibition, and wrote the report which was agreed to by the representation of the nations who took part in the conference on the subject. And later still, in 1870, owing to the representations of the Imperial Academy of Russia to the French Government and to the scientific bodies of other nations of the need of preparing more accurate and metric standards for the use of countries which might adopt the metric system, an International Committee was appointed to prepare such standards. This Committee met in Paris, in June 1870, and will resume its labours in September next.

In the United Kingdom considerable progress has been made towards the introduction of the metric system, though much certainly remains to be done. In 1862 a Committee of the House of Commons was appointed to consider the practicability of adopting a simple and uniform system of weights and measures, with a view not only to the benefit of internal trade, but to facilitate our trade and intercourse with foreign countries. In discussing the question of the possible decimalisation of the existing system, the Committee of the British House of Commons, in the same manner as the Committee of the Imperial Academy of Russia, reported that it would involve almost as much difficulty to create a special decimal system of our own as simply to adopt the metric decimal system in common with other nations. And under the circumstances the Committee came to a unanimous recommendation in favour of the introduction of the metric system.

Nearly 200,000,000 of people in Europe have already recognised the metric system as the international system of weights and measures, 160,000,000 of whom have already adopted it in a compulsory manner. If once, therefore, Russia and England should finally place their legislation on the same footing, the other smaller states will certainly follow, and Europe will have attained perfect unity as regards weights and measures. But in other parts of the world also considerable progress has been made. In Asia the whole of India may be said to have adopted the weights and measures of capacity of the metric system, though some time may elapse before the Act passed by the Indian Government can be carried into operation.

In America the United States have introduced it permissively;

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* The principal modifications from this common plan are described in "Lectures on the Organs of Digestion in the Mammalia," in course of publication in the *Medical Times and Gazette*.

whilst Brazil, Chili, Mexico, Grenada, and other American Republics have adopted the metric system absolutely.

Nor has there been less done as regards the coinage. If we compare the coins now in use all over the world with those in use some twenty years ago it will be seen what advance we have already made everywhere towards unity. Some countries, such as France, Italy, Switzerland, Belgium, Greece, and Roumania, have already an identical system of coinage secured to them by the Coinage Convention of December 23, 1865. The Austro-Hungarian Empire issues gold pieces marked 20 florins and 8 florins, equal to 25fr. and 10fr. respectively. Spain issues gold pieces of 25 pecates, equal to the 25fr. pieces, and Sweden the Caroline, equal to 10fr.

The Committee much regret that the German Empire, which had recently a most favourable opportunity for extending the desired uniformity, an object to which she has shown her adherence by the adoption of the metric system, has issued a new gold coinage, having nothing in common either with the money of the Convention of France, Switzerland, Italy, and Belgium, or with the monetary systems of England or the United States.

During the last year the Committee have had communications with the Indian Government on the question of introducing the metric system of weights and measures into India, the original Act by which all the weights and measures of the system were introduced having been vetoed by the Home Government, and another, limited to the weights and measures of capacity, having been passed in its stead.

The ramification of the weights, measures, and coins all over the world will be fraught with immense benefit to science, commerce, and civilisation; and scientific and philosophical bodies of all nations have given their adhesion to it; the commercial classes look for it as an essential element in the economy of time and the performance of international work, and travellers all over the world regard it as the greatest boon that could be conferred. Towards the attainment of this important object the Metric Committee of the British Association for the Advancement of Science have exercised an important influence.

SECTION G—MECHANICAL SCIENCE

OPENING ADDRESS BY THE PRESIDENT, FREDERICK J. BRAMWELL, C.E.

THE point which I have to determine is what shall my one subject be—on what shall I address you? I have thought over many subjects connected with mechanical science, but I cannot discover anything more practically important than "Coal." Very few matters are of greater real interest at all times to the nation at large, and very few are more prominently before the minds of the public at the present time, and certainly no subject can be more appropriate for a mechanical engineer, if for no other reason than this, that the steam engine is still the very crowning glory of mechanical engineering, and that coal is the staff of life, and, so to speak, the breath of the nostrils of the steam engine. The raisings of coal, which in 1855 were only 64 millions of tons in Great Britain, rose to 80 millions in 1860, and to 108 millions in 1869; and I will also advert to the fact that the price of all kinds of coal has in the colliery districts risen, speaking in round numbers, about 100 per cent. within the last twelve months, and is still rising. Let us now see how we do deal with coal in those cases where coal must be used; how we might deal with it in such cases; and how we might in certain instances substitute other sources of power for the coal which we now consume. And let us first of all consider this question of finding sources other than coal for our motive power. Before the steam engine was so extensively used as it now is, the wind, the force of the streams, and the force of the tide were all employed to give motive power. With respect to the power of the wind, it is to be feared it is too irregular to enable any manufacturer to rely upon it in competition with the steam engine. With respect to the power of our streams, the altered condition of the soil due to increased drainage and cultivation has so materially interfered with the regularity of their flow, that their efficiency as sources of constant power is seriously diminished, while competition with them by steam has become much greater than it was when the water mills themselves were better off. This state of things, however, might be cured, and, in fact, has been cured in certain districts, by the union of a large number of mill proprietors to form storage reservoirs, from which the water can be delivered with regularity so as to give an uniform supply to the mills. But the third source of water power, the tide mill, which at one time was used to a considerable extent, is almost wholly discontinued. The causes of this discontinuance are sufficiently obvious. The tide mill as formerly constructed could work for only a limited period in each ebb, and to obtain the full effect it had to utilise both the night and the day tides. But while the tide mills laboured under these disadvantages, they possessed the great merit that their power, such as it was, was one that could be depended on, and one which, although it fluctuated, fluctuated regularly and within known and definite limits. I would suggest that, in those cases where there are large manufacturing districts within a few miles of the sea, and where there is a rise and fall of the tide, coupled, in the outset at all events, with natural indentations of the coast which might be comparatively readily dammed up for the storage of the water, there such storage should be made that the water should be set to work turbines of the best kind (turbines which will work with very nearly the same per-centage of the total power given out by the water at any particular moment, whether they are immersed or whether they are not); that these turbines should be employed in pumping water at a high pressure into Armstrong accumulators; and that pipes should be laid on from those accumulators to the neighbouring manufacturing town, and should there deliver their power to the consumers requiring it, to be used by them in water-pressure engines. Suppose a beginning were made with the city of Bristol, which is no doubt a very favourable instance for the application of this suggestion. Here the rise and fall of the tide might safely be taken at 24 ft. Half a square mile of water enclosed would, after the most lavish deductions for loss, yield, in Bristol at least, 5,000 horse-power, probably sufficient to replace the whole of the power of the stationary engines now at work in Bristol. I will now consider the question how coal is wasted in its use, but before doing so I will say a few words upon the loss that occurs in the coal mine itself. Happily, this loss has for some years past been greatly reduced. More economic systems of working have prevailed, plans of dealing with small coal by washing away its impurities, so as to render it fit for cokeing, have been largely adopted, and thus a great deal of that coal which a few years since would have remained buried in the mine, as not justifying the expense of raising it to the surface and of paying royalty upon it, is now brought to light and is utilised. Nevertheless we know that at ordinary prices of coal it is to the advantage of the colliery proprietor in many instances to leave a considerable per-centage of the seams that are worked rather than to endeavour to lessen that percentage by the use of a more expensive system of artificial support for the roof, and further that it also pays him to leave altogether unworked very thin seams of coal. Hereafter, when coal becomes scarce, there can be no question that the inhabitants of these islands would be glad to make use of the now despised unworked seams, and also to recover the buried coal of the worked seams; but such seams and such savings, although they can be worked and made at present, when the mines are open, if not at profit, yet with little loss, will then only be capable of being reached by a reopening and pumping out of abandoned mines, a process so expensive that great indeed must be the need of our successors if they are compelled to resort to it. I now come to the question of the way in which waste occurs in the use of the coals that are brought to the surface. This use may be divided into two great branches—the domestic and the manufacturing. I will consider first the domestic use. This is a highly important branch of the subject. It is believed that out of the total of ninety-eight or ninety-nine millions of tons of coal which in 1869 were retained for home use, eighteen and a-half millions of tons, about one-fifth of that quantity, were consumed for domestic purposes (about ten millions being exported). We all of us know intimately the way in which coals are burnt for domestic purposes. The other way in which we use coal is for purposes of manufacture, and this, again, may be divided into two branches at least—namely, the coal that is employed for obtaining power, and the coal that is employed in metallurgical and other operations not immediately connected with the production of power. To treat of those latter cases first, they are far too numerous to be dealt with in detail, and, therefore, only a few of the principal must be considered. Take the subject of coke making. How much coal is heated in clamps and in kilns to be converted into coke, and in how few instances is any use made of the whole of the heat residing in the gaseous parts of the coal which are driven off. This heat frequently amounts to 30 per cent. of the whole of that which is in the coal. We come next to the smelting of iron. Take the preliminary process of calcining the ore. In those cases where the ore is "black band," the ore so common in Scotland, the calcining is done by the combustion of the carbonaceous matter mixed with the ore. Far more than the quantity of fuel requisite for the calcination is associated with this ore, but the whole of it is burnt off, and no effort whatever is made to utilise the surplus heat. Then with regard to the blast furnaces for smelting iron. Here, still almost universally in Scotland, that large seat of the iron manufacture, and to a considerable extent in England, the waste gases are suffered to issue from the furnace top, illuminating the country for miles round, and bearing testimony to the indifference of the owner of the furnaces to a waste of our store of fuel. Upwards of sixty years ago—viz., in 1811—the utilisation of these gases was suggested in France, but not much was done for thirty years. About 1840, however, their use became not infrequent in that country, and French manufacturers and chemists taught us that the gas thus recklessly wasted might be collected and utilised, and made to replace the fuel expended in heating the hot blast stoves and in raising steam for the blowing engines. But, for the cause which has been and will be alluded to, the adoption of this plan was very slow indeed in England. It has now been in use, however, for many years in our best conducted works, but as a proof of the slowness of its introduction, the furnaces of Scotland, as I have already said, are even to this day almost universally worked upon the wickedly wasteful principle of allowing these gases to burn idly away. Take again the melting of steel in crucibles where the heat issues from the furnace, of necessity hotter than the heat of the melted steel (for were it not so it would cool it), and of this issuing heat, as a rule, no use whatever is made. Take again the heating furnace and puddling furnace of our iron works, very commonly from these heat at a greater temperature than that of welding iron escapes up the chimneys, disregarded as though it had cost nothing for its generation.

Next let us consider how we are dealing with coal when we use it for obtaining motive power in our steam engines. Steam engines may be divided into the four great heads of marine, locomotive, portable, and fixed. Including within the term steam engine the boiler as well as the engine, the waste may arise in a steam engine in two ways, in either one of them, or in both combined. It may arise from an imperfect utilisation of fuel in the production of steam—that is, a waste due to the boiler and to the firing; or it may arise in an improper use by the engine of the steam provided for it by the boiler. There can be no question that the boiler waste is, as a rule, very large indeed. I am perfectly certain there is hardly any subject more worthy the attention of the engineer than the replacing the stoker by some mechanical arrangement which shall afford absolute uniformity of firing, and therefore absolute uniformity of the conditions of the fire, and this is a subject not only worthy of attention on account of the saving of coal, but also on the ground of putting an end to a most laborious, exhausting, and, it is to be feared, unhealthy occupation, viz., that of the steamboat fireman, more particularly when he is working in a hot climate. If perfect combustion were obtained in the fire, I do not think there would be much difficulty in properly utilising by the boiler the heat evolved.

I have now laid before you some of the points in which the boilers and engines of the present day are below the standard to which engineering science has already reached, and in which, therefore, there is known opportunity for immediate improvement. There is a perpetual bugbear in the way of improvements, and that bugbear is the so-called "practical man," and he was in my mind when, in previous parts of this address, I have hinted at the existence of an obstacle to the adoption of improvement. I do not wish the section for one moment to suppose that I, brought up as an apprentice in a workshop, and who all my life have practised my profession, intend to say one word against the practical man. On the contrary, he is the man, of all others, that I admire, and by whom I would wish persons to be guided; because the truly practical man is one who knows the reason of that which he practises, who can give an account of the faith that is in him, and who, while he possesses the readiness of mind and the dexterity of action which arise from the long-continued and daily intercourse with the subject of his profession, possesses also that necessary amount of theoretical and scientific knowledge which justifies him in pursuing any process he adopts, which in many cases enables him to devise new processes, or which, at all events, if he be not of an inventive quality of mind, will enable him to appreciate and value the new processes devised by others. This is the truly practical man, about whom I have nothing to say except that which is most laudatory. But the practical man, as commonly understood, means the man who knows the practice of his trade, and knows nothing else concerning it; the man whose wisdom consists in standing by seeing, but not investigating, the new discoveries which are taking place around him, in decrying those discoveries, in applying to those who invent improvements, even the very greatest, the epithet of "schemers," and then when he finds that beyond all dispute some new matter is good and has come into general practice, taking to it grumblingly, but still taking to it, because if he did not he could not compete with his co-manufacturers. The aim and object of such a man, indeed, is to ensure that he should never make a mistake by embarking his capital or his time in that which has not been proved by men of large hearts and large intelligence. It is such a practical man as this who delays all improvement. For years he delayed the development in England of the utilisation of the waste gases of blast furnaces, and he has done it so successfully that, as I have already had occasion to remark, this utilisation is by no means universal in this kingdom. It was such men as these who kept back surface condensation for twenty years. It is such a man as this who, when semaphores were invented, would have said, "Don't suggest such a mode to me of transmitting messages; I am a practical man, sir, and I believe that the way to transmit a message is to write it on paper, deliver it to a messenger, and put him on horseback." In the next generation his successor would be a believer in semaphores, and when the electrical telegraphist came to him and said, "Do you know that I can transmit movement by an invisible electrical power, through a wire however long, and it seems to me that if one were to make a code out of this movement, I could speak to you at Portsmouth at one end of the wire, while I was in London at the other," what would have been the answer of this practical man? "Sir, I don't believe in transmitting messages by an invisible agency; I am a practical man, and I believe in semaphores, which I can see working." In like manner, when the Siemens' regenerative gas furnace was introduced, what said the practical man: "Turn your coals into gas, and burn the gas, and then talk of regeneration. I don't know what you mean by regeneration, except in a spiritual sense. I am a practical man, and if I want heat out of coals I put coals on to a fire and burn them," and for fifteen years the practical man has been the bar to this most enormous improvement in metallurgical operations. The practical man is beginning slowly to yield with respect to these furnaces, because he finds, as I have already said, that men of greater intelligence have now in sufficiently large numbers adopted the invention to make it a formidable competition with the persons who stolidly refuse to be improved. The same practical man for years stood in the way of the development of Bessemer steel; now he has been compelled to become a convert. It may be said that employers and the heads of manufactories are, as a rule, in these days, educated gentlemen, and that, therefore, it is wrong to impute to them the narrow mindedness of the practical man. I agree that in numerous instances this would be wrong; but the fact is that in many cases, I think I may say in most cases, the head of the establishment, the moneyed man, the man who by his commercial ability (that most necessary element in all establishments) keeps the concern going by finding lucrative orders, is not intimately acquainted with the practice of the business carried on by his firm. He relies upon some manager or foreman who too commonly is not the real but the so-called practical man. It is to such men as those who simply practise that which they have seen, without knowing why they practise it, that the title of practical man has most improperly been attributed, and it is on the advice of such men that the true heads of the firm too commonly regulate their conduct as to the management of their business, and as to the necessary changes to be made in the way of improvement. As I have said, the practical man derides those who bring forward new inventions and calls them schemers. No doubt whatever they do scheme, and well it is for the country that there are men who do so. It also may be true that the majority of schemes prove abortive; but it must be recollected that the whole progress of art and manufacture has depended, and will depend, upon successful discoveries, which in their inception were and will be schemes, just as much as were those discoveries that have been and will be unfruitful

But the successful discoveries, because they are successful, are taken out of the category of schemes when years of untiring application on the part of the inventors have, so to speak, thrust them down the throat of the unwilling practical man. Take the instance of Mr. Bessemer, who was beset for years by difficulties of detail in his great scheme of improvement in the manufacture of steel. As long as he was so beset, the practical men chorused, "He is a schemer; he is one of the schemers; it is a scheme." Supposing that these practical difficulties had beaten Mr. Bessemer, and that they had not been overcome to this day, the practical man would have derided him still as a schemer, although the theory and groundwork of his invention would have been as true under these circumstances as it now is. Fortunately for the world, and happily for him, he was able to overcome these most vexatious hindrances and make his invention that which it is. No one now dares to apply the term "schemer" to Mr. Bessemer or "scheme" to his invention; but it is as true now that he is a "schemer," and his invention a "scheme," as it would have been had he failed up to the present to conquer the minor difficulties. It is a species of profanation to suggest, but I must suggest it, for it is true, that Watt, Stephenson, Faraday, and almost every other name among the honoured dead, to whose inventive genius we owe the development that has taken place within the last century in all the luxuries, the comforts, and even the bare necessities of our daily existence, would, in their day, and while struggling for success, have been spoken of as schemers even in respect of those very inventions of which we are now enjoying the fruits. But I feel I need not labour this point further at a meeting of the Mechanical Section of the British Association—an Association established for the advancement of science. I know I shall be accused of decrying the practical man, and of upholding the schemers. I say most emphatically that I do not decry the practical man. I plead guilty to the charge of decrying the miscalled practical man, and I glory in my guilt; while I readily accept that which I consider the praise of upholding "schemers," and I do so for this simple reason that if there were no schemers there would be no improvement. I think it becomes a scientific body like the British Association to laud the generous effort of the unsuccessful inventor rather than to encourage the cold selfishness of the man who stands by and sees others endeavour to raise the structure of improvement without lending a hand to help, and even sneers at the builders, but when the structure is fully raised and solidly established, claims to come in to inhabit, and being in probably essays, cuckoo like, to oust the builders, and to take possession for his own benefit. One word in conclusion. Can we not devise some means by which consumers of coal may be instructed in, shamed into, or tempted to the economical use of that most valuable material? The Royal Agricultural Society of England, by its judicious efforts for many years past by the institution of trials and the giving of prizes for the best engines, has brought the consumption of coal down from 10 lbs. per horse-power to a little over a quarter of that quantity. Could we not institute a Society which should devote itself to the recording and the rewarding of the performances of steamboats and of fixed engines for land purposes? I am aware it is supposed that there is a difficulty in these cases which does not obtain in the case of portable engines that can be brought for trial upon a dynamometer, and that is, that the power exerted by marine engines varies during the voyage, and is not that which is developed at the measured mile, while in a manufactory it varies according to the conditions of the trade, and to the extent to which the British workman condescends to attend to his work. But there are implements which record the horse-power exerted from moment to moment, and register it on indices as readable as those of an ordinary counter of an engine, or as those of a gas meter. I believe that one of the very greatest incentives to economical working which the owners of steamboats could offer to their engine builders and engineers would be the application of such implements as these. Were they employed, the shipowner would know at the end of the voyage so much horse-power had been exerted as a whole, and that so much coal had been burnt, and that the result, therefore, was a consumption of so many pounds per horse-power per hour. All excuses of head-winds, and all the aid of canvas to the engine-power, would be eliminated from the calculation. The continual indicator would register truly the work the engine had to do, whether that work was made excessive by contending with head winds, or was rendered light by favourable breezes and the assistance of canvas. In the same way the proprietor of the engine for manufacturing purposes, the cotton mill, the woollen mill, the corn mill, and even the highly irregularly working rolling mills and saw mills, would be able at the end of the quarter to say: "Notwithstanding all the variations of my trade and rate of manufacture, I know that my engines have exerted so much power, I know that I have burnt so much coal, and that therefore such and such have been the economic results." Assuming that steamboat proprietors and the owners of fixed land engines would go to the expense of applying such continuous recording implements as these to their engines, and would become members of an association for the purpose of visiting and inspecting and of reporting upon their machinery, and of giving prizes to the men in charge for careful attention; prizes to the manufacturers for original good design and workmanship of the engines; and prizes to the proprietors for their public spirit in having bought that which was good instead of that which was bad and cheap, and for having employed intelligent and careful workmen instead of ignorant and careless ones,—I believe within a few years as great an improvement might be seen among the marine and manufacturing class of engines as has been effected by the laudable exertions of the Royal Agricultural Society of England among the portable ones. I think the initiation of some such society as this would be a practically useful result from the meeting of Section G.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, July 29.—Mr. Cayley presented the continuation of his memoir on the condition enabling a family of given surfaces to form part of an orthogonal system.—M. de Saint-Venant communicated a note by M. J. Boussinesq on a simple mode of determining experimentally the maximum resistance to sliding in a ductile, homogeneous, and isotropic solid.—M. Yvon Villarceau presented a note on a new theorem in general mechanics.—M. W. de Fonvielle read a notice of the results of observations on recent thunderstorms.—A note from M. P. Volpicelli on the theory of Nicholson's duplicates was read.—M. E. Becquerel presented a note by M. A. Cazin on the quantity of magnetism of electro-magnets.—M. C. Sainte-Claire Deville communicated a note by M. de Tastes on the fall of an aërolite in the commune of Lancé (Loir-et-Cher) on July 23. This fall took place about half-past 5 P.M., with a clear sky and bright sun; it was accompanied by a violent explosion heard over a great extent of country. The course of the meteor was from S.W. to N.E., and it appeared to be double, or to consist of two meteors following a parallel course. A large portion fell and buried itself in the ground to the depth of 1·50 metre.—M. Boussingault read a memoir in continuation of his researches upon the presence of iron in the organism; it related to the distribution of iron in the materials of the blood. The greatest portion is contained in the globules.—M. Daubrée presented an investigation of the meteorites of Ovifak, with regard to the amount of carbon and of soluble salts which they contain.—M. Berthelot presented a note on the constitution of acid salts in solution; M. J. A. Le Bel a note on the pyrogenated carburets of Pechelbroun; and MM. Girard and De Laire a note on the colouring matters derived from aniline, in reply to a recent communication by M. Lauth.—M. C. Sainte-Claire Deville presented a note of an examination by M. Gorceix of the gaseous emanations of Santorin during the close of the eruption of 1866.

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Errata.—Vol. vi, p. 266, 1st col., line 19 from bottom, for "bulk" read "bulb;" line 9 from bottom, for "plane" read "pane." 2nd col., line 16 from bottom, for "behind" read "between;" p. 267, 1st col., line 20, for "pressures" read "temperatures."

THURSDAY, SEPTEMBER 5, 1872

NAVAL SCIENCE *

MR. REED, who has hitherto been known to the naval world principally as a successful shipbuilder, and as the writer of two valuable works on shipbuilding, comes before it now in the additional character of editor of a magazine, which, under the title of *Naval Science*, professes to embrace all branches of science relating to naval affairs. How far this profession will be fulfilled, as the magazine, advancing from number to number, gathers strength with increasing age, remains to be seen; but at present it is impossible to avoid the remark that a very disproportionate part of the two numbers now before us, and more especially of the first, is occupied with articles devoted to the study of naval architecture. The superabundance of papers relating to this subject is one which, from Mr. Reed's antecedents, might fairly have been expected, and is to be expected in the earlier numbers, until the editor gathers round him a staff competent to write on the many other scientific topics which present, or should present, equal interest to, and as directly concern, our naval officers. Such subjects as the stability or the rolling of ships, ably and in most respects agreeably as they are treated, have, from their theoretical point of view, absolutely no connection with the duties of a naval officer, however much they may appeal to his desire to be fully acquainted with whatever bears, even remotely, on his profession. It is, of course, important that, with these new ships which have points of maximum and vanishing stability, the commanding officer, and not only he, but every executive officer, should familiarly know the limit of inclination which his ship must never be allowed to exceed; but, practically speaking, it is of no more consequence to him to know the mathematical reason of this than it is to know the geological formation of a submerged rock ahead. The relation between the period of a ship in the trough of the sea and the period of the waves amid which she would be rolling, has even still less to do with the practical duties of a seaman who is unable, under any circumstances or in any degree, to alter either "the metacentric height," "the radius of gyration," or "the period of oscillation" (p. 199); but who is able, so long as the ship is not a helpless wreck, to keep her out of the trough of the sea, the position to which these experiments and calculations refer. We by no means wish to imply that the study of such questions as these is of no importance to the naval officer; on the contrary, we are very decidedly of opinion that whatever gives him a clearer insight into the meaning of the rules by which his conduct has to be guided, renders him a more intelligent, and therefore a more capable officer; what we would say is, merely, that they have not such paramount interest as to render it desirable to devote to them at least half the space in a magazine of Naval Science. Saying this, we must also add that we are fortunate in having in Mr. Reed one who represents for one scientific branch what should be represented in all branches.

The remaining space has been allotted to more varied papers, some of which are of considerable ability. Amongst these we would specially mention one on "Naval Tactics," which contains a very interesting *résumé* of the subject, and permits us to hope that in a future number the writer will enter more fully into a discussion of the probable and possible effects of the several formations; for though no certain result can be arrived at till war has actually tested them, it seems to us that much knowledge may be gained by a consideration of the different methods of attack and defence, as opposed to each other.

We have, again, a valuable contribution to navigation in an article on "Rhumb and Great Circle Charts," which illustrates the important application of the Great Circle principle to windward sailing, in a manner more lucid and satisfactory than we remember to have seen in print before. But in treating of its more general relations to navigation and to the conduct of a long passage, the writer would seem to have momentarily overlooked the geographical as well as the meteorological constraints which are everywhere put on it; thus, for instance, in the outward passage to Australia, the great circle route leads through latitudes dangerous or impassable from ice, whilst the steadiest streak of westerly winds is to the north rather than to the south of the 45th parallel. A previous article from, if we mistake not, the same pen, enters on the long-vexed question of "The Rational Method of Teaching Navigation." There can be little doubt that the author is right as to the rationality of the method;—as to the utter irrationality of the method which has been hitherto generally adopted, the difficulty which has stood in its way has been the absolute impossibility of adapting it to the necessities of the service, and of giving very young boys the requisite grounding in elementary mathematics, in the given time and amid the bustle and disturbance of a sea life. We may fairly hope that this difficulty is about to vanish, now that Mr. Goschen recognises the advisability of materially increasing the age for the entry of naval cadets. It is on this that the hope of any real improvement in the state of naval education must principally depend. When a youngster at the age of fourteen is thrust into a world of peculiar hardship and excitement, he must have an extraordinary aptitude if he follows up book-learning one step more than he is obliged to do. It appears in the evidence before Admiral Shadwell's Committee that about seven hours a week is a good average amount of study under instruction; it does not appear in the evidence, but it is none the less true, that a young boy's attendance at study in the forenoon, after he has kept the morning or middle watch, is for the most part corporeal rather than mental, from which he derives little or no profit. When the present system has been entirely done away with, and been replaced by some other, such, perhaps, as that recently proposed by Captain Goodenough, we may hope that our young naval officers, as they grow into manhood, may possess a fair groundwork of the more essential parts of an exact education. But until some such radical alteration has been made, until a real, however moderate, amount of accurate grounding becomes the rule and not the exception amongst our young officers, we confess that we can scarcely look with confidence to the now loudly vaunted schemes for "the higher education of the navy," whether it is to be carried on at Greenwich or elsewhere.

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* "Naval Science: a Quarterly Magazine for Promoting the Improvement of Naval Architecture, Marine Engineering, Steam Navigation, and Seamanship" Edited by E. J. Reed, C.B., &c. Nos. I. and II., 8vo. Lockwood & Co)

It is, however, a mistake to speak, as is so commonly done, of the navy as a scientific profession. As a profession, the regular line of the service is practical amongst the practical; it requires and cultivates a quick eye, a restrained temper, a cool courage, a ready judgment of men and things; it calls for a minute and thorough knowledge, not only of all that relates to the management of ships and men, but of the extensive "literature" issued and demanded by the Admiralty; few have the time, even if they had the disposition, to go deep into scientific study, to which there is no inducement, for which there is no reward. One of the earlier articles in the magazine before us, "On the Necessity of forming a Naval Staff," pointedly calls attention to this. We believe that the writer has under rather than overstated his case. That beyond the regular line of the service there are duties which call for a higher and more extended knowledge, is freely admitted by all; and we are convinced that these duties will be more efficiently performed by officers specially educated for them, than by others nominated indiscriminately or by roster, after a vain endeavour to bring the whole body of officers to one universal high standard. There are few men, worth anything at all, who have not a distinct speciality, and we conceive that it is by allowing, nay, encouraging these specialities to develop themselves to the utmost, that the greatest perfection in the aggregate is to be attained. To force a man of an essentially practical turn of mind, with a judgment in the handling of a ship, or with an insight into the character of men, which seems instinctive rather than acquired, to go through a distasteful course of high mathematics, or of foreign languages, or, on the other hand, to pin a man of unusual taste and aptitude for the study of more advanced science down to the routine of the service, appears to us a most erroneous system. Yet, so far as we understand, this is what is seriously proposed. The formation of a Naval Staff, and the abolition of the existing segregation of navigating officers, would, we believe, be a radical and immediate cure. It is in evidence before the committee already referred to, that the special training of officers whom interest or taste attracts to gunnery duties, has been found to work exceedingly well; we believe that a similar system with regard to navigating duties would be as successful; nor can we admit that a small body of men, chosen by the mere accidents of birth or family connection, is likely to furnish such a number of first-rate navigators and surveyors as could be got together by special selection from the whole service;—to use a geographical simile, we would drain a larger area. Roughly speaking, about one-sixth of the lieutenants on the list have taken out gunnery certificates; the proportion of those who would take out advanced certificates in navigation would probably be considerably greater; whilst, indirectly, a large number would follow up some course of study with a view to turning it to future account. If appointments as flag-lieutenants were made in a similar way, we believe that the impetus given to study amongst the officers of our navy would lead to results quite equal to those wished for. But it is not in human nature to pursue a course of study wearisome in its beginnings, amidst the worry and turmoil of an active profession, without encouragement, without hope of reward.

We have no space to notice, in a manner proportionate to its merits, Mr. Mallet's article on the Action of Torpedoes; this, after all, is, so far as the navy is concerned, the question of the day, and any discussion that leads to a closer acquaintance with it ought to be carefully studied. Every one acknowledges, first, that the navy, at present, is in a transition state; and secondly, that more Science is wanted. We consider it a fortunate thing, therefore, that what is destined to be a high-class journal of Naval Science, under the editorship of such a distinguished man as Mr. Reed, has made its appearance at this time, and we wish it every success—a success, moreover, which is certain, so long as the stated point of view is steadily kept in mind.

OUR BOOK SHELF

Die Echinoiden der oesterreichisch-ungarischen oberen Tertiärablagerungen. Von Dr. Gustav C. Laube. Mit vier lithographirten Tafeln. (Wien, 1871.)

AGASSIZ, Desor, E. Forbes, Desmoulins, and Wright have written elaborate monographs and descriptions of Miocene Echinodermata. Prof. Ed. Forbes described (Proc. Geol. Soc., vol. iv. pp. 230-232, 1843) a large series from the miocene beds of Malta and Gozo, collected by Capt. Spratt, R.N., of H.M. surveying vessel *Beacon*; Dr. Wright in 1855 (Ann. Mag. Nat. Hist., vol. xv.) described many species from collections made by the Earl of Ducie, from the same islands; and again, in 1864 (Q. Jour. Geol. Soc., vol. xx. p. 470), through the series collected by Dr. Leith Adams at Malta, he described new miocene forms. These were all important additions to the then little known echinodermata of the miocene rocks of southern Europe. In the present monograph, which is reprinted from the "Abhandlungen der k. k. Geologischen Reichsanstalt," Dr. Gustav C. Laube also carefully describes fourteen new or previously unknown species of echinoderms from the Austro-Hungarian miocene (upper tertiary) deposits, and adds a new genus (*Brissomorpha*) to receive those forms possessing non-depressed ambulacral grooves, and an attenuated posterior border. Many of the species described by Laube are peculiar to the miocene rocks of the Austro-Hungarian area, whilst others have a much wider distribution, several being identical with the Maltese forms described by Dr. Wright. Of the fourteen genera and thirty-seven species included in Dr. G. Laube's monograph, seven species occur in Malta, seven in Corsica, and eleven in France; while the remaining twelve species are peculiar to the Austro-Hungarian beds. The following new species have been described and figured by the author in the memoir before us, viz., *Cidaris Schwabenarii*, *Echinus dux*, *E. hungaricus*, *Echinocyamus transylvanicus*, *Scutella Vindobonensis*, *Echinolampas angustistellatus*, *Pericosmus affinis*, *Hemiaster rotundus*, *H. kalksburgensis*, *Schizaster leithanus*, *S. Karreri*, *Brissomorpha Fuchsi*, *Spatangus euglyphus*, and *S. austriacus*. The monograph contains a very valuable table showing the distribution of those species which occur elsewhere, such as Malta, Corsica, Italy, and France, with a general column for other localities (*andere Lander*); it is accompanied by four quarto lithographic plates devoted to the local and new species, which are carefully figured. The whole is an important contribution to this division of the *Annuloida*.

R. E.

Fishes of New Zealand. Catalogue, with Diagnoses of the Species. By F. W. Hutton, F.G.S., Assistant Geologist. *Notes on the Edible Fishes.* By James Hector, M.D., F.R.S., Director. With 12 plates. (New Zealand, Wellington, 1872.)

THE geologists of New Zealand are of opinion that, in order to acquire a knowledge of the inhabitants of their country in past epochs, it is necessary first to know what its present inhabitants are. This will appear rather strange to some of their European brethren, who do not seem to consider that the subjects have anything to do with one another, and who usually keep them as far apart as possible. But there can be no doubt, we believe, that our Antipodal friends are right, and that a knowledge of the extinct fauna of any country must be preceded by a study of its existent fauna.

We have lately given our readers an account of two lately-published works on the Birds of New Zealand. We have now the pleasure of introducing to their notice an excellent *résumé* of the present state of our knowledge of the fishes of the same country. This has been prepared by Captain Hutton, author of one of the previously mentioned treatises, under the direction of the enlightened Chief of the Geological Survey of New Zealand, who himself contributes additional information to the volume of a highly important nature, in shape of Notes on the Edible Fishes of the Colony.

The arrangements and nomenclature adopted by Capt. Hutton are that of Dr. Gunther's "Catalogue of Fishes," the most recent and by far the best authority on the subject. A few new species are introduced, discovered since the issue of Dr. Günther's work, and some imperfectly known fishes mentioned, which Dr. Gunther does not appear to have noticed. Thus the total number of New Zealand fishes included in Captain Hutton's work amounts to 141. There can be no doubt, however, that this number will be considerably augmented when more attention has been devoted to the subject. Indeed, we believe that even within the past few months Dr. Gunther has described some rather remarkable additions to the Ichthyological Fauna of New Zealand, which are not included in the present synopsis.

The greater number of New Zealand fishes are marine species. The freshwater fish fauna is poor in the extreme, although it included two remarkable forms belonging, one to the true *Salmonidæ*, and the other to a nearly allied group, which are the "Smelt" and "Grayling" of the native fishermen. The former fish, Dr. Hector tells us, is "delicious food," but does not attain sufficient dimensions to make it of great importance as an article of diet. But the native "grayling," which probably reaches 6 lbs. or 8 lbs. in weight, ought to be valuable, and we cannot quite understand why, with such a fish available, it was thought necessary to spend large sums in the endeavour to introduce European *Salmonidæ* into the freshwater of New Zealand.

In concluding the brief notice of a most useful work, we should add that the twelve plates which accompany it, and which give the outline of about fifty of the most characteristic species, even if not of great scientific value, will, we are sure, be of great assistance to the unlearned colonist in his attempt to make out the correct names of the native fishes.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Radial Polarisation of the Corona

DURING the recent meeting of the British Association at Brighton, it was casually remarked by Sir William Thomson that during the eclipse of last December, it had been observed that the radial polarisation of the corona was greater at a small distance from the sun than close to it. This observation he thought was a very important one.

My observations on this subject were published in the *Philosophical Magazine* for last March, but I think Sir William Thomson's opinion may be taken as sufficient reason for thinking that they would not be uninteresting to the readers of NATURE.

I have twice accompanied Mr. Pogson, the Government Astronomer at Madras, as polariscopic observer, in his eclipse expeditions; first, in August 1868, and secondly, last December.

In August 1868 I used simply a Savart's polariscope, attached to a telescope, employing a diaphragm to limit the field of view. On this occasion I found that the corona was radially polarised. I shall enter, however, more fully into a description of the method adopted last December, as the results then obtained, while confirming those obtained in 1868, are, I think, of much greater importance.

The Savart was so adjusted as to show a white central band between two dark ones, when the bands corresponded in direction with the plane of polarisation. This polariscope was, last December, converted into a polarimeter, by causing the light to be examined to pass first through four plates of crown glass, mounted in a frame, moveable on an axis at right angles to the direction of the bands. This instrument was fixed in front of the eye-piece of a small equatorially-mounted telescope.

When the glass plates were perpendicular to the optical axis of the instrument, they had, of course, no effect whatever upon the polarisation of the light passing through them; but if the frame carrying the plates were turned on its axis, the light passing through them would be polarised in a plane parallel with the axis, and hence at right angles to the direction of the bands, and the amount of polarisation so caused may be calculated for any angle the glass plates may make with their normal or zero position by means of formulæ in Prof. W. G. Adams's paper, published in the *Philosophical Magazine* for April 1871.

It is evident that if we are examining common light by this instrument, turning the glass plates will cause black centred bands to appear; that is, the central band will be dark, and between two white ones, exactly the reverse of what takes place when light polarised in the plane of the bands is examined by the polariscope. If then while examining light polarised in the plane of the bands, we turn the glass plates, we prevent that polarised light from passing to the polariscope, and the bands gradually disappear as the angle of the plates increases, and they completely disappear when the plates are at such an angle as would produce on ordinary light the same amount of polarisation as that existing in the light under examination.

By knowing the angle through which the plates have to be turned in order to extinguish the bands, we are able to measure the amount of polarisation in any polarised light; the instrument forming what may be called a differential polarimeter. Further, it is evident that if we place our bands at right angles to the plane of polarisation, we shall get black centred bands, which would only be increased in intensity by turning the glass plates; hence we can only measure the polarisation of light with this instrument when the direction of the bands coincides with the plane of polarisation.

My own observations of the eclipse of 1868 led me to expect to find radial polarisation.

Prof. Pickering thought that the polarisation he observed in 1869 was caused by the reflection of sunlight from the unobserved portions of the earth into the atmosphere, and thence back to the eye. The plane of polarisation would, in that case, be vertical near the sun. I took the portion of the limb at about 90° from the vertex, towards the south (that is near the southern limb) for my observations, and carefully placed my instrument so that the bands were radial to the sun before totality. The bands were therefore horizontal, or nearly so. This was then a crucial experiment as to the accuracy of Prof. Pickering's view on the one hand, or of my own previous determination on the other.

Directly totality commenced the bands appeared, and they were white centred. The plane of polarisation was, therefore, horizontal and not vertical; it was radial, and could not be caused in the way imagined by Prof. Pickering.

I next turned the glass plates till the bands disappeared, and by this means measured the amount of polarisation, which as I have before explained, could not be done had the plane of polarisation been at right angles to the plane of the bands.

On examining the light from a portion of the corona at about ten minutes distance from the limb, I found the polarisation greater, in about the ratio of ·275 to ·158, than it was close to the limb; these figures representing approximately the amount of polarisation respectively at ten minutes from the limb, and close to it, the total light being unity.

We may therefore conclude that

1. The corona is radially polarised.
2. This polarisation increases as we recede from the limb.

The bright lines seen in the spectrum of the corona inform us that *part* of the light we receive from it is intrinsic, that is, that the gases composing it are incandescent, and from their proximity to the sun we should scarcely expect anything else; yet this fact in no way renders it impossible that much of the light we receive from the corona should be reflected or scattered by minute particles of, perhaps, denser matter, probably incipient cloud, suspended within it, as such particles are supposed to exist in the earth's atmosphere, in order to account for the polarisation and blue colour of the sky. This supposition, when we remember that the temperature, and hence the amount of intrinsic light, must decrease as we recede from the sun, would amply account for the increase of polarisation with distance from the limb. There are, however, two other causes which may be named as adequate to produce this effect. Probably in reality the increase is due to the three causes combined.

When the plates were inclined so as to neutralise the corona polarisation, I saw faint dark centred bands on the portion of the moon's disc in the field. I did not observe any when the plates were at right angles to the axis of the telescope, but I think I should have noticed them had they existed; so that although there was a sensible amount of light on the moon's disc sufficient to show bands when polarised by the glass plates, I do not think it was perceptibly polarised itself. This would tend to show that the light was reflected from the moon itself, and not from the intervening atmosphere.

G. K. WINTER

Erratum of the Errata, or, "A Few Millions".

I AM indebted to Mr. A. Cowper Ranyard, of London, for calling public attention to errors existing in the illustrative appendix to a research entitled Acoustical Experiments, &c., which article of mine the Editor of NATURE honoured with a republication in his journal on May 9, 1872.

The existence of these errors has been known to me since a few weeks after the original publication of my paper; but as they did not affect in the least the subject proper of the research, and would be apparent to any one who might take the trouble to review the calculations, I allowed them to pass unnoticed, and even now would not pursue the subject further had Mr. Ranyard really corrected my errors; but he has *himself* committed the error of "A Few Millions" (the title of his communication*) which he would attribute to *me* when, in these words, he undertakes the correction of my figures. "Taking the velocity of light as 185,300 miles per second, and the wave-length of D_1, as given by Angstrom, at 0·00058950 millimetres, gives 5,058,700,000,000,000 vibrations per second, or a little more than *five thousand millions of millions,* instead of a little less than *six hundred millions of millions* vibrations per second, as given by Dr. Mayer." The following is the correct calculation:—

$$185,300 \text{ miles} = \frac{298,212,000,000 \text{ mm.}}{\cdot 0005895 \text{ mm.}} = 505,870,000,000,000$$

and 5,058,700,000,000,000 (Mr. A. C. Ranyard's result) minus 505,870,000,000,000 (Mr. Mayer's result) gives Mr. Ranyard 4,552,830,000,000,000 tremors.

Thus it appears that both Mr. Ranyard and myself commit errors in simple arithmetic, but I am sure that our mutual friends will not attribute them to want of sufficient mathematical culture to accomplish "a simple rule-of-three sum." (A. C. R.) He that is without sin let him first cast a stone. I, however, do not wish Mr Ranyard's errors in any way to extenuate my own greater negligence which has disfigured the appendix of my paper, containing, as it does, "some strange numerical errors, which perhaps it will be well to point out, lest some of your readers should make use of the numbers given at the end of the paper without previously testing them." (A. C. R.) I will therefore ask my readers to substitute for the last paragraph under the heading of "Quantitative Relations in the Experiments and Analogical Facts in the Phenomena of Light," the following:—

"We will now examine the analogical phenomena in the case of light. Let fork No. 1, giving 256 vibrations a second stand for 508,730,000,000,000 vibrations a second, which will be the number of vibrations made by the ray D_1 of the spectrum, if we adopt 300,000 kilometres per second as the velocity of light Then fork No. 3 will represent 504,750,000,000,000 vibrations per second, which latter give a wave-length ·0000048 millimetre longer than that of D_1, and belongs to a ray removed from D_1, towards the red end of the spectrum, by eight times the distance which separates D_1 from D_2. We saw that fork No. 3, giving 254 vibrations a second, had to move towards the ear with a velocity of 8·734 feet to give the note produced by 256 vibrations per second, emanating from a fixed fork; so, if a star, which only sends forth those rays which vibrate 504,750,000,000,000 times a second, should move towards the eye with a velocity of 2,442 kilometres, or 1,517 miles, its colour would change to that given when D_1 emanates from a stationary soda-flame."

ALFRED M. MAYER

Rev. John Ward on Atmospheric Germs

THIS worthy was Vicar of Stratford-upon-Avon, from 1662 till his death in 1681. He was a man of general knowledge, and was specially skilled in the diseases of women and children. It is not known that he obtained the archiepiscopal licence to practise physic, but he certainly practised the healing art, and he records his intention "to inquire whether a man may get of the archbishop a licence to practise *per totam Angliam!*" His diary, 1648-1679, is sensible and entertaining. It is chiefly known as containing a notice of Shakespeare, with the only extant account of the cause of his death, viz., "a feavour" caused by a carouse with Drayton and Ben Jonson. The Diary is in the Library of the Medical Society of London. It was edited by Dr. Charles Severn, and published by Colburn, in 1839.

The following extract is remarkable:—

"Venenum pestilens est congeries minimarum animalcularum per aerem volitantium, quæ corpora humana per respirationem aut poros subeuntes, eorum partes corrodunt et corrumpunt, ex iisque ad alia corpora volitantes, seu ad alia quocunque modo delatæ, et quasi contagio propagatæ, etiam illa inficiunt, corrodunt, corrumpunt, sicut priora, e quibus evenerunt. . . . Supra fœnum cubare noxium multis fecit, non solum in peste, sed etiam in aliis morbis."

Of course *in peste* means "in the case of the plague."

C. M. INGLEBY

Coefficients of the Linear Expansion of Solids

AT the British Association which met last year in Edinburgh I suggested a *thermometer of translation* which should record the amount of the successive rises of temperature during the year. For this purpose a body possessing great expansibility with a fine needle point at its upper end, was proposed to be placed on a sloping frame, and made of a material possessing small expansibility, and protected from the changes of temperature, and having its upper surface finely serrated. When the body expanded, its upper end bearing the needle point would extend higher up on the frame, and when contraction commenced the projecting needle point would continue its hold of the teeth on the frame, preventing shortening at its upper end, so that the centre of gravity of the mass would be raised. In this way the successive increments of heat would be registered by successive *creeps* of the body upwards on the frame.

It has occurred to me that the same principle might be advantageously adopted for measuring the linear expansion of different solids.

In order to double the readings for expansion, clamping screws attached to upright rods fixed at the ends of the body would be better than the needle point for detention during contraction, which was proposed for the thermometer of translation. The bar to be experimented on would be placed on rollers in a vessel containing water or steam of different temperatures. The screws would be tightened at the lower end of the bar, and slackened at the upper before expansion, and tightened at the upper and slackened at the lower before contraction. After the contraction had fully taken place, the bar would be again heated and again cooled, and this process would be repeated until the total amount of translation became easily measurable. Although the amount of translation produced in any case by a single experiment might be scarcely appreciable, yet we can by cumulative repetition increase the amount of translation to any extent without increasing the errors of observation, for a single final reading is sufficient for the whole series of expansions however numerous they may be. This single reading of the total amount of *creep* being divided by the number of times the experiment was repeated, and by the given number of degrees of heat through which it had been each time successively raised, and by the original length of the bar, will give the desired coefficient of expansion.

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* See NATURE, June 20.

I think that results obtained by this mode of translation will, for minute expansions, be more reliable than those obtained either by Lavoisier and Laplace's method, or by that of Roy and Ramsden.

THOMAS STEVENSON

Baden-Baden, August 26

Origin of Insects

IN NATURE of December 7, 1871, there is an interesting letter from Mr. B. T. Lowne, on the Origin of Insects, in which the writer refers to Fritz Muller's "Facts for Darwin" in favour of the opinion that "the larval forms of insects are probably derived from imaginal" or perfect forms. I have not at present any opinion to offer on this subject; but, though I estimate very highly indeed the light which Fritz Muller has thrown on the Crustacea, I think nothing can be more unsatisfactory than his remarks on insects. He concludes that the earliest insects resembled the wingless Blattidæ, overlooking, what is obvious enough, that any theory of the origin of insects ought to account for, or at least show the origin of, those most characteristic organs of the class, the wings. I quite agree with Mr. Lowne that "it is extremely probable that insects first emerged from the water with fully-formed wings." I think it scarcely possible to doubt that the wings were originally organs of aquatic respiration. But this does not answer the question of the origin of insect metamorphoses, which, though an evolutionist, I think one of the greatest difficulties of the theory of evolution; it does not answer the question whether the perfect forms with wings and legs have been derived from the larval forms without either, or the converse.

Mr. Lowne goes on to say, "We have still relics of an aquatic winged insect fauna in the hymenopterous genus, discovered by Sir John Lubbock." I cannot think this brings us any nearer to the origin of insects. It could not do so unless the Hymenoptera were at or near the origin of the class, and this will scarcely be maintained. The Hymenoptera are probably the highest of all insects—certainly so if instinct is the criterion. The aquatic Hymenopteron (I do not know its name) is no more a relic of the origin of insects than are the water-beetles; and no one will say that the Coleoptera are near the origin of the class. It is true that the water-beetles are wingless, while the Hymenopteron in question is winged; but the beetles are a winged order, and those which have no wings have lost them. Indeed, it is only in a functional sense that any beetle is wingless, for they all retain the wing-covers, which are modified wings. It is probably true that the origin of all animals whatever was aquatic, but it does not follow that the aquatic members of any class denote the origin of the class. The aquatic habits and structure may be only adaptive. No one would look to the seal or the hippopotamus for the origin of the Mammalia.

JOSEPH JOHN MURPHY

THE LAW WHICH REGULATES THE FREQUENCY OF THE PULSE

FROM a pamphlet on this subject, recently published by Mr. A. H. Garrod, we extract the following summary of the main features of the circulation:—

"The circulation of the blood is maintained by the repeated contraction of the heart. Each cardiac revolution is divided into three parts—the systole, the diaspasis, and the diastole. The following laws hold with regard to the length of these intervals:—

"I. The systole, together with the diaspasis—or, in other words, the first cardiac interval—varies as the square root of the whole revolution.

"II. The systole varies as the square root of the diastole.

"III. The diaspasis is constant.

"The amount of work that the heart has to perform in maintaining the circulation depends on two sets of changes which may occur in the system: 1. Variations in the blood pressure; 2. Variations in the resistance to the outflow of that fluid from the arteries.

"As the capacity of the arteries, including the ventricles, varies directly as the blood pressure, and as the flow of blood from the capillaries does the same, the frequency of the heart's beats is dependent on the resistance to the capillary outflow, and not at all on the blood pressure; in other words, the heart always recommences to beat when the blood pressure in the systematic arteries has fallen a certain invariable proportion.

"Variations in blood pressure result from: 1. Absorption into, and excretion from, the vascular system of fluids; 2. Changes in the capacity of the arterial system, which occur on the contraction or relaxation of the muscular arteries; 3. Changes in the amount of available blood, which result from the hæmastatic dilitation of some of the yielding vessels on altering the position of the body. As changes in the first of these cannot be very sudden, and those in the latter are never very considerable, the mean blood pressure in health varies but little during short intervals.

"Variations in peripheral resistance result from: 1. Different degrees of tonicity or patency of the muscular arteries; 2. Different resistances in the venous system. The former may occur independently in one or other system of vessels, as the cutaneous or the alimentary; also mechanically from pressure on a part of the body. The latter are insignificant in health.

"The heart depends for its power of doing work on chemical properties in the blood it pumps into the systemic vessels, and as the blood reaches it direct from those vessels, the cardiac intramural circulation varies with the changes in the former; and the length of the systole varying only as the square root of the time of diastole, the degree of cardiac nutrition varies directly as the systematic blood pressure, and as the square root of the diastolic time. The coronary arteries supplying the whole heart, the work done by the right ventricle is governed by that done in the left; thus the supply of blood in the left auricle is always rendered sufficient for the requirements of the systemic circulation; though, as there is no reason for believing that the resistance in the pulmonary vessels varies with that of the systemic, there must be some peculiarities in the former circulation (which may explain the variations in the ratio of the number of pulse beats to respirations in some cases).

"The auricular contraction is a very small force, and its function is most probably to close the tricuspid and mitral valve.

"The heart commencing its systole as a whole, it is highly probable that the impulse for action is given by a force which affects both ventricles; such is found in the coronary circulation and the active diastole produced by means of it."

THE CONGRESS OF PREHISTORIC ARCHÆOLOGY

THE meeting of the International Congress of Archæology at Brussels was brought to a close last Friday, August 30. On the previous Tuesday General Faidherbe spoke on the Dolmens, of which he had made a special study in Algeria. He believes them to be the work of some people whose traces can be found from Pomerania to the coast of Africa, and of whose migrations they indicate the halts. Mr. Franks, of the British Museum, presided on the afternoon of the same day, when the discussion turned chiefly on the primitive races of Belgium. On Wednesday the last expedition of the Congress took place, Namur and the Camp of Hastedon, distant about two kilometres from Namur, being the places selected. The establishment of this camp, covering an area of from eleven to twelve hectares, is attributed to the men of the Polished-Stone period. It is situated on a high plateau, and the cuttings made through the ancient enclosure were explained by M. Dupont, Director of the Brussels Museum.

M. Dupont dwelt chiefly on the fact that at the epoch of the Mammoth there were two perfectly distinct populations in Belgium, one using the flint cut at Spiennes, near Mons, and the other that of the Somme. The highly interesting Museum of Archæology belonging to the town of Namur was afterwards examined by the Congress. At the meeting of Thursday, M. Dupont traced the connection of the various populations in Belgium among each other at the different ages of stone; after which a lively debate arose on the question of the descent of the present race of men from the troglodytes, and on the causes of the difference of types. In the afternoon the problems of the Tertiary age and the age of Bronze occupied the Congress. On Friday morning the question of determining the relative remoteness of the ages of bronze and of iron led to many valuable disquisitions; and Mr. Hyde Clarke gave a summary of the recent anthropological discussions at Brighton.

M. de Quatrefages summarised the results of the present Congress, and stated as the principal ones that the elements of the prehistoric populations—even of the age of stone—are discernible in the present population, and that even in the most remote ages the migrations of races took place on a much more extensive scale, and with more frequency than was believed by any one till recently. M. Vervoort, one of the Belgian vice-presidents, next presented to the Congress, in the name of M. Geefs, the well-known sculptor, a bust of M. d'Omalius d'Halloy, who presided in person at this the last meeting of the Congress. This bust is a most striking likeness of the venerable *savant*, who was congratulated by M. de Quatrefages, speaking for the Congress, on the homage rendered to him by his colleagues. M. d'Halloy's services to ethnography have been long and valuable, and his vigour of mind and youngness of heart are astonishing in a man on the verge of ninety.

The proceedings terminated with the distribution to the members of a commemorative bronze medal on the part of the Belgian Government. This medal, having a diameter of nearly seven centimetres, bears on one side, within a laurel wreath, the inscription, "Congrès International d'Anthropologie et d'Archéologie Préhistoriques à Bruxelles. 6e Session, sous la Présidence de M. T. T. d'Omalius d'Halloy, 1872." The obverse represents the Genius of Science as a female figure, seated, and pointing with her left hand to the entrance of a cavern, bearing the inscription "Furfooz," and a mammoth's skull; while on the other side of the figure the geologist's shovel and pickaxe are displayed. The medal is by M. J. Geerts, of Brussels, and is very finely executed. M. d'Omalius then formally declared the Congress terminated, congratulating the members on the scientific progress achieved, and the harmony which had characterised the meetings.

In accordance with the invitation of the Swedish Government, the next assembly of the Congress will take place at Stockholm in 1874; the proposition to confer the presidency on that occasion on Prince Oscar of Sweden was carried by acclamation. On Saturday the members of the Committee, Belgian and foreign, dined with the King.

THE FRENCH SCIENTIFIC ASSOCIATION

AMONG the subjects expected to be discussed at the general meetings of the French Association at Bordeaux are one on Fermentation, by M. Pasteur, and another on Aërial Navigation, by M. Dupuy de Lôme. In the Mathematical Section, M. d'Abbadie is expected to read a paper entitled "Expériences pour constater les variations de la verticale." In the Physics Section, M. Cornu reads a paper on the "Velocity of Light;" M. Mercadier, one on "Musical Intervals;" M. Poter, on the "Theory of Light." In the Chemical Section, M. Berthelot is to speak on "Questions of Chemical Philosophy," and M. Wurtz on "The Densities of certain Vapours, and particularly on the Density of the Vapour of Perchloride of Phosphorus." In the Meteorological Section, M. Marié Davy is expected to read a paper on "The Organisation of Meteorological Observatories."

Other expected papers are—In Geology, M. Daubrée, "Beds of Phosphates in the South of France;" M. des Cloiseaux, "Amblygonite and Martebrasite." In Botany, M. Baillon, on "*Rheum* and on the Botanic Origin of Officinal Rhubarb;" M. Chatin, "Study of the Development of the Ovule and the Grain in the Scrophularias." In Zoology, MM. de Follin, Fischer, and Périer, contribute a paper on "Recent Submarine Explorations; M. Chatin, "Researches on the Odorous Glands of certain Mammifers;" Dr. Pouchet, on "Animal Pigments;" M. de Quatrefages, on "Some Species of Inferior Animals of the Basin of Arcachon." Anthropology, M. Broca, on "The Occipital Angles." Geography, M. Gustave Lambert, on "An Expedition to the North Pole."

The Excursions are:—1. To the embouchure of the Gironde, to inspect the encroachment on the coast. 2. To Arcachon, to visit the oyster-beds, dredge the sea for molluscs, &c. 3. To Les Eyzies, to inspect pre-historic remains and bone-caves. 4. To Roueyre, to inspect the iron of the Landes, &c. 5. To Bidassoa, on the Spanish frontier, to inspect a rich bed of iron ore. 6. To Medoc, on a visit to the celebrated vineyards of Château-Margaux and Château-Montrose. Besides these excursions, visits will be made in Bordeaux itself, to M. Gintrao's establishment for rearing silkworms in the open air; to the docks, dockyards, &c., and the artesian wells of Vigan In our last number we gave a list of the public lectures.

TEMPERATURE OF THE SEA BETWEEN GREENLAND, NORTHERN EUROPE, AND SPITZBERGEN

PROF. H. MOHN, Director of the Norwegian Meteorological Institute at Christiania, publishes in *Petermann's Mittheilungen* some important facts regarding the variations of temperature in the North Atlantic. The yearly variation of temperature of the surface stratum amounts to 9° Fah. and more; it becomes less as we go down, the decline, however, being not everywhere the same. Deep-sea strata reach their lowest and highest temperatures a little later than the surface stratum; the changes offering two very distinct aspects for summer and winter. Deep-sea observations in several of the deep fjords along the Norwegian coast, which are protected against the great depth of the Atlantic by submarine ridges lying before them, show that the water in them is derived from the Gulf Stream, and that they are filled with it from top to bottom, even if the latter lies deeper than the icy bed of the Gulf Stream outside the coast region; were this not so, the temperature of the water in the fjords would be a much lower one, and Norway would not enjoy such a happy union of land and sea climate. In summer, near the coast of Norway, and in its fjords, at a depth of from 100 to 300 fathoms, we find a uniform temperature of about 44° Fah.; farther out to sea, however, at the same depth, only about 39° Fah. The deep-sea temperatures in winter are less known, but it is almost certain that at great depths the same temperature reigns all the year round, although a continual cooling from the surface downwards necessarily takes place in winter. In the north-western part of the Greenland Sea, and below the depth of the Gulf Stream, exclusively icy water is found, which somewhat compresses the latter on that side, at any rate on the surface, where the water cooled during the winter nights remains over the warmer waters beneath. Along the coast of Norway the cold from the land acts on the surface and the upper strata of the sea, increasing with the nearness of the land, so that here the temperature of the sea rises with its depth, and the axis of warmth of the Gulf Stream is moved away from the coast towards the open sea. Taking the form of the Gulf Stream as that of its surfaces having the same temperature—isothermal surfaces—we can compare it with the shape of one of the small boats called prams, which are broadest at the stern, deeper in the centre than behind, and possessing a somewhat rounded stem. The stern of this Gulf Stream pram is formed by a vertical section from Iceland to Scotland; the longitudinal section forms the axis of warmth, running along the coast of Norway. The side nearest the Polar Ocean (the larboard side) is much more considerable than the starboard side, which leans against the Norwegian coast. In summer the starboard side is pushed quite close to the Norwegian coast, and hangs strongly over, while the larboard side is perpendicular, or only slightly inclined outward; the keel near Spitzbergen sitting deep in the water. In winter the starboard side is thirty (geog.) miles broad, and has in the parts lying nearest to the coast sides strongly inclining inward, while the strata in the centre and those bordering on the Polar Ocean rise nearly perpendicularly, the keel in the fore part raising itself almost into the position of the stem, which ends in the same point as that formed by the isotherms of the surface at this season. Generally this aspect is only presented by the part of the sea which lies westward from Norway and partly from Spitzbergen.

The warm waters of the Novaja Semlja Sea are like a wedge placed horizontally, with its base between Spitzbergen and Norway, and its horizontal sharp edge turned towards the north and east.

THE ROYAL SOCIETY OF VICTORIA

We are glad to notice the progress of Science in Victoria as exhibited by the address of the President, Mr. Ellery, on the occasion of the annual *conversazione* held on July 8. We reprint the following extracts from the *Argus* of the following day:—

"We have now entered upon our fifteenth session, and as you have done me the great honour to again choose me as your President, it devolves on me, in accordance with our rules, to address you on the past year's history and progress of the Society; and also to call your attention to some of the more noteworthy facts which mark the last year's history of general scientific progress. First, then, in reference to our own business, I regret to have once more to inform you that, since the last publication of the *Transactions* of the Society, the funds have not been in a sufficiently flourishing condition to enable the council to resume the printing. For many years past the only revenue of the Society has been that derived from entrance fees and subscriptions of members. From this not only the current expenses but the interest on money borrowed for carrying out the alterations and additions to our buildings has to be paid; and although our income will amply meet these demands if the annual subscriptions of members are regularly paid, there has hitherto been an insufficient sum left to print our *Transactions* without other aid. The Government have been solicited for help every year since 1867, when the last aid was granted to us by Parliament. The council hope, however, that this year their request will be acceded to. I am happy to state, moreover, that (many arrears of subscriptions having been received of late) the financial condition of the Society is just now better than it has been for years. It is intended, therefore, at all events to at once print the *Transactions*, and the council trust that they may be able henceforward to publish promptly and regularly the proceedings of our meetings, which they will be quite able to do if the Parliament resumes its small annual grant-in-aid. Our last anniversary meeting was held on August 14, 1871. Since that time the Society has held eight ordinary meetings. On September 11 a valuable paper 'On Ocean Waves and their Action on Floating Bodies, was contributed by Mr. Deverill. Mr. Macgeorge also read a paper, contributed by Mr. Horne, of Adelaide, 'On a Linear Method of Finding the Stability of Ships;' and Mr. Pain, on 'Aboriginal Art and its Decadence in Australasia, Polynesia, and Oceanica.' The meeting of October 9 was occupied with Mr. Macgeorge's account of 'Changes in η Argus,' and Mr. G. Foord's 'Aërometer for Measuring Specific Gravities.' On November 13, our next meeting, the Rev. W. Kelly and Mr. Bosisto contributed papers, the former on 'On a Method of Combining Marsh's Test for Arsenic with Reinche's, so as to secure very reliable results;' the latter 'On the Cultivation of *Mentha piperita* in Victoria.' On November 22 it will be remembered the Australian Eclipse Expedition started from Melbourne. Our next meeting was a special one held on January 22, and was devoted to matters connected with the Eclipse Expedition, and to the approaching elections of council and office-bearers, which took place on our next meeting, on March 11. In April Mr. Harrison read a paper 'On Patents and their Utilisation.' Mr. Caldwell contributed one on 'Meat-preserving,' and Mr. P. F. Foord 'On Bi-angular Co-ordinates.' On May 13, Mr. F. Poolman read a description of his 'Self-Acting Safety Regulator and Coal Economiser for Steam Engines,' and Mr. A. K. Smith exhibited and described 'An Improved Valve for the Fire Plugs in Water Pipes,' the object being to prevent the entry of sewage water into the pipes when the pressure was off—a thing that might occur with the ordinary fire-plug valves. At our last meeting, on June 10, Mr. Macgeorge contributed the 'Results of Observations on Sirius and its Companions' with the great Melbourne telescope. Mr. White exhibited some new five-figure card logarithms which he had arranged, and Mr. Gibbons read a few 'Notes on M. Berthelot's Analysis of the Cranbourne Meteorite.'"

After alluding to the reports received from the Austra- Expedition for observing the total eclipse of Dec. 12, with which our readers are already acquainted, the President continued:—

"I have but little of more than ordinary interest to record of the past year's history of our several science or art institutions. The Technological Museum attached to the Public Library has made considerable progress; not only have the Commissioners established classes of chemistry, mineralogy, and practical mining, but have organised evening courses of lectures on popularised science and art. These have always been so well attended that it is to be regretted that a larger lecture-room than the one which was built for class teaching has not been erected. The earlier courses of these lectures have been printed and circulated by the Commissioners. They appear to have attracted considerable attention in England and on the Continent, especially those by Baron von Mueller on Forest Culture, and the purely technological series of Mr. George Foord. Prof. Negri, president of the Royal Geographical Society of Italy, in referring to Baron von Mueller's lecture, said he wished the Italian Government would have it translated into Italian and circulated throughout the country. A telegraph class for ladies has also been established in connection with the museum, at which pupils are instructed in the manipulation and ordinary use of the Morse telegraph instruments. It is intended, I believe, to hold periodic examinations of the pupils, and to grant certificates to such as prove themselves to be competent. The most recent step in the right direction the Commissioners have taken is the appointment of a gentleman of undoubted ability to conduct classes in geometry and mathematics. The additions that have been made from time to time to our national gallery of pictures now form a most valuable and beautiful collection, and it seems well adapted to fulfil one of its principal objects—the foundation of a school of painting. Already we have seen in Melbourne copies after pictures in our national collection of no small merit, and giving good promises of future excellence. For so young a colony as ours the number of students is even now large, and is, I hear, increasing. The National Museum at the University, under the care of Prof. M'Coy, is becoming more complete and perfect every year; the space that has become available, through the removal of many of the mining and machinery models to the Technological Museum will be most advantageously bestowed upon numerous beautiful specimens and collections which hitherto have appeared somewhat too crowded. Botanical science in the colony, represented by our fellow member, Baron von Mueller, has made considerable progress during the past year. I have already referred to his lectures on forest culture, in which he clearly set forth the more important and lasting objects of a botanical department in a new country, and, to quote his own words, 'A botanic garden has not merely to gratify the passing hour, but has to fulfil great objects of the whole community, as well for this as for the coming generation.' Baron von Mueller, I am glad to say, intends shortly to issue some popular works on Australian botany; the first, I believe, is to be devoted to the ferns, and it is intended to illustrate it by photo-lithography. I have been also informed that Count Castelneau, the well-known zoologist, has prepared a descriptive essay on the fishes of Victoria, which is to be issued with the report of the Acclimatisation Society. I refer to these points, although they do not belong to our past year's history, because they indicate scientific vitality and progress; and although the fruition may belong to another year of this Society, the work evidently belongs to this. In our Observatory one of the most interesting results of the past year's work is the establishment of the fact that the nebula in η Argus has not only undergone marked change since the time it was observed and drawn by the late Sir John Herschel at the Cape of Good Hope, but has also exhibited notable change since the erection of the great telescope in Melbourne. Drawings of it made at intervals of only a few months, as was pointed out by Mr. Macgeorge in the paper he read before you at our October meeting, present such differences that we can now hardly escape from the impression that observable changes in this nebula take place very rapidly. Several observers in the southern hemisphere have devoted a good deal of attention and observation to this celestial object—notably, Mr. F. Abbott, of Hobart Town; Mr. H. C. Russell, director of the Sydney Observatory; Mr. Tebbutt, of Windsor, New South Wales; and Lieut. Herschel, in India. Mr. Abbott, I believe, was the first to draw attention to the fact that it no longer appeared as drawn by Sir John Herschel. Mr. Russell made a very careful drawing of the nebula as seen with the Sydney 7¼-in. refractor. Mr. Abbott also made some drawings from observations from a 4½-in. refractor, and Lieut. Herschel by the aid of a 12-in. reflector. They have all indicated that the general appearance of the nebulæ differed considerably from that represented by Sir John Herschel's drawings, although none of the apertures used could in any way pretend to reach the more minute details grasped by Herschel's 2-ft. reflector. Several of the drawings which reached home had evidently not been executed with that precision which is so necessary to establish a fact of this kind in the minds of astronomers who are unable to see for themselves. There has arisen, therefore, in the minds of many of our most renowned observers in England and elsewhere, doubts as to the real existence of these changes. For it must be remembered that the immense distance of the nebulæ from us—probably far beyond the most distant stars—makes it necessary that changes such as these described, to be visible to us even with the aid of such light-gathering apertures and optical power as is possessed by our large telescope, must be stupendous in the highest degree, and almost beyond comparison with the most ordinary cosmical changes with which we are familiar. Now, since the great telescope has been erected, special attention has been given to this object. Mr. Le Sueur devoted a great deal of time, extending over long periods, to examination and drawings, repeating his observations again after the lapse of many months. He constantly referred to me to establish or throw doubt on his observations, so that I often observed with an unbiassed eye for this purpose. The stars down to the 16th magnitude were carefully plotted (those to the 12th magnitude with the micrometer), to form an unchanging groundwork for the mapping. He announced on several occasions in this society and elsewhere that there were unmistakable changes since Sir John Herschel's drawings. Mr. Macgeorge, who succeeded Mr. Le Sueur, and who has also observed and drawn the nebulæ constantly, pointed out to you in his paper in October last the progressive changes that had been noted. The diagram he then exhibited I had photographed and sent home to Dr. Robinson (one of the Great Telescope Committee), with a copy of Mr. Macgeorge's paper. The paper got home first, and Dr Robinson says, in a letter I received last mail: 'I lose no time in forwarding the paper to Sir E. Sabine, Mr. Lassell, and Mr. Warren De La Rue. . . . . . Mr. Lassell seems to cling to the idea which he published some time ago, that there was no change whatever in η Argus. He justifies this suspense of opinion by not being able to refer to the drawings, a difficulty which I hope you will soon be able to remove. I think his real difficulty is an opinion that nebulæ must be at a distance much greater than that even of small stars, and hence an incapability of conceiving the possibility of such changes as could be visible to us.' The photographs reached him by next mail, and he then writes:—'The photographs are very remarkable, and I think it is impossible to look at them and doubt the reality of the immense changes that have taken place. Are these changes periodical? I send one of the photographs by this post to Sir Edward Sabine, with request to forward it to Messrs. Lassell and De La Rue.' It is to be regretted that the minute and careful drawings made by Messrs. Le Sueur and Macgeorge have not yet been engraved, as they establish the facts beyond all doubt, as the photographs sent home were from a somewhat rough diagram intended only to show the principal features of the observed changes. No one accustomed to observing could fail to be convinced of change going on, if he only saw the nebulæ with a power of six or seven hundred on the great telescope on good nights at at intervals of three or four months. Mr. Macgeorge reports from observations made only two months since that still further changes were evident. The full significance of these changes can hardly yet be estimated; but they overthrow many of our hitherto-received notions of the condition of these tenants of space. It is a subject of the highest interest in physical astronomy, and one that will demand unremitting observation and drawing for its further elucidation. I am glad to inform you that the Government has given me authority to publish every month the results of our observations in meteorology, terrestrial magnetism, and of other phenomena; the numbers from the commencement of the year till the end of May are already before the public. By this means all the useful information derivable from the Observatory work in these branches of investigation is made quickly and generally available. Photography of celestial objects has been commenced with the great telescope, and some exceedingly fine and promising negatives of the moon were taken, enlargements from which have already been exhibited at one of our meetings. Since April the weather has been too unfavourable to proceed with this work. Attempts to obtain photographs of planets and nebulæ have since been made; but while those of the planets promise well, no impression whatever of the brightest nebulæ could be secured. Among the many subjects which have occupied a large share of the attention of scientific circles in the older world, and which mark indelibly the progress of scientific research during the past year, there is one which appears to me of surpassing importance. The Royal Astronomical Society awarded its gold medal this year to Prof. Schiaparelli, director of the Observatory at Milan, principally on account of his researches on the relations that exist between comets and shooting stars. You will remember in a former address I had the honour of delivering to you, I spoke of the 'meteor shower' which fell in Europe in November 186—, and that it had been established beyond a doubt that these bodies travelled in orbits intersecting that of the earth at different points; that one coterie intersected it in November, another in August, and so forth. Since then, however, it has been found from observation that the number of these meteor rings is very large, and that they intersect the earth's orbit at numerous points. And it may be stated generally, that all falling or shooting stars, at the time we see them, are, or have been very recently, members of groups travelling in true orbits, and not merely stray wanderers in space. Prof. Schiaparelli has concluded from his researches that 'celestial matter may be divided into the following classes:—1. Fixed stars. 2. Agglomeration of small stars (resolvable nebulæ). 3. Similar bodies, invisible except when approaching the sun (comets). 4. Small particles, composing a cosmical cloud.' He thinks the last occupy a large portion of space, and have motions similar to fixed stars. The latter are the sources of falling stars. Brought by the motion of our system in space within the sphere of our sun's attraction, they become in a measure part of his family and subject to him. If, while making their sun journey, they approach a planet—the earth for instance—they get disturbed in their orbits, and, becoming subject to the earth's mass, liable to enter the upper regions of our atmosphere, under which condition they appear to us as 'shooting stars.' 'Thus meteors and other celestial phenomena of like nature, which a century ago were regarded as atmospheric phenomena—which La Place and Olbers ventured to think came from the moon, and which were afterwards raised to the dignity of being members of the planetary system—are now proved to belong to the stellar regions, and to be in truth falling stars. They have the same relation to comets as the asteroids have to the planets; in both cases their small size is made up by their greater number. Lastly, we may presume that it is certain that falling stars, meteors, and aerolites differ in size only and not in composition; therefore we may presume that they are an example of what the universe is composed of. As in them we find no elements foreign to those of the earth, we may infer the similarity of composition of all the universe—a fact already suggested by the revelations of the spectroscope.' Professor Schiaparelli had noticed a remarkable likeness between the elements of the orbits of some of these meteor groups to those of some well-known comets, the perihelion passage occurring approximately at near dates, the direction of their motion alike, the point they intersect the ecliptic, and their inclination to it very similar, while the distances of their nearest approach to the sun, and their period of revolution, have also a marked likeness. The relations are very remarkable, and Prof. Schiaparelli concludes one of his last memoirs on this subject in these words:—'These approximations need no comment. Must we regard these falling stars as swarms of small comets, or rather as the product of the dissolution of so many great comets? I dare make no reply to such a question.' The conclusions of Prof. Schiaparelli are of the highest interest, and suggest some new and interesting questions on the constitution of the universe. Are the irresolvable nebulæ systems of these cosmical particles? If so it will add a fresh interest to our observations of the changes going on in that of η Argus.

"In a new country, such as ours, in which all are so fully engaged in business pursuits, it would be unreasonable to expect so large an annual crop of scientific facts as are realised in the older countries; but as the fields for original observations in a new country are really wider in many respects than in the older, it is of course, of the first importance that what we do obtain should be properly recorded and disseminated. I have mentioned that the Society's *Transactions*, the printing of which has been too long suspended, will be immediately resumed, and I believe I may confidently state that the present prospects of the Society are such as to warrant the belief that they will henceforth appear with regularity. It should be remembered that although this Society has how existed for so many years, its ranks are still thin, wanting both workers and supporters. It has been recently proposed in your council, as a means of strengthening and increasing the utility of the Society, that a rule should be adopted to admit of residents at a distance joining us as country life-members, on the same scale as ordinary members are now admitted, by payment of half the usual subscription. This will entitle such members to the Society's publications, and to all the privileges of membership when in Melbourne. I have chosen the earliest occasion for announcing this proposition, which will no doubt be presently adopted by the generality of the members. I also wish to remove an impression which, I believe, holds some ground, that advanced scientific attainments are indispensable qualifications for membership. The object for which our Society is founded was the promotion of literature, science, and art in the colony. Whoever can assist in this is, so far, eligible for membership."

NOTES

Mr. Stanley has forwarded to the daily papers a letter from Zanzibar, dated July 22nd, stating that communications have been received from the interior, which render it probable that Dr. Livingstone would receive his stores and letters about August 1st. In that case he would leave Unyamyembe about August 10th, and be now near Mrera, in Central Ukonougo.

The following letter has been sent by Lord Granville to Mr. H. M. Stanley, the special correspondent of the *New York Herald*, accompanied by a magnificent gold snuff-box richly set in brilliants:—"Foreign Office, Aug. 27, 1872.—Sir,—I have great satisfaction in conveying to you, by command of the Queen, Her Majesty's high appreciation of the prudence and zeal which you have displayed in opening a communication with Dr. Livingstone, and so relieving Her Majesty from the anxiety which, in common with her subjects, she had felt in regard to the fate of that distinguished traveller. The Queen desires me to express her thanks for the service you have thus rendered, together with Her Majesty's congratulations on your having so successfully carried out the mission which you so fearlessly undertook. Her Majesty also desires me to request your acceptance of the memorial which accompanies this letter."

On Monday evening the "Livingstone Search and Relief Committee of the Royal Geographical Society" met to consider its final judgment upon the conduct of the expedition under Lieutenant Dawson, which left England on the 9th of last February, and arrived at Zanzibar on the 17th of March, some months after Dr. Livingstone had been found and relieved by Mr. Stanley. The Society, however, adjourned the inquiry, and have agreed to put their questions in writing, so that Lieutenant Dawson may give them a categorical answer. The blue book containing Dr. Livingstone's despatches of 1870, has been placed before Parliament.

THE *Times of India* of Aug. 9 contains the order of the local Government on the report of the Madras Cyclone Committee. In this order Mr. Pogson, the Government Astronomer, is severely blamed for his negligence in not giving due warning of the coming storm, as it was his duty and in his power to do. "Mr. Pogson," the order says, "endeavours to explain this omission as having been caused partly by an accident which happened to his carriage at noon on May 1, but partly also by disinclination to suggest anything to the Marine Department, his advice not having of late years been sought by that department. It does not appear to have occurred to Mr. Pogson that an occasion on which a most serious loss of life was imminent was not one for the indulgence of such susceptibilities. The Governor in Council considers that Mr. Pogson has failed to justify his negligence on this occasion. It is hardly necessary that the Government should, after the disastrous consequences of Mr. Pogson's neglect, inform him that they expect from him in future the strictest observance of that portion of his instructions which require him 'to furnish notices of approaching stormy weather,' and that the most essential part of his duty is constant, and, when necessary, personal communication with the head of the Marine Department." The Marine Department and all the officials connected therewith are severely blamed; and Mr. Dalrymple, the Master-Attendant, comes in for special censure on account of his negligence, and especially "for omitting to remain at his office during the night of May 1." The Government have ordered the appliances for saving life and property to be considerably increased, and accept in their entirety the recommendations of the Committee, which they resolve to adopt and embody in a code of rules, which will be communicated for the strictest observance by all the departments concerned.

NEXT year's London International Exhibition will consist of three divisions:—1. Fine Arts; 2. Manufactures; 3. Recent Scientific Inventions and Discoveries of all kinds. We notice that of the future Exhibitions that of 1874 will embrace Artificial Illuminations by all Methods; Gas and its Manufacture. 1875.—Hydraulics and Experiments; Supply of Water. 1876.—Photographic Apparatus and Photography, and Philosophical Instruments and Processes depending upon their use. 1877.—Health; Manufactures, &c., promoting Health, with Experiments. 1880.—Chemical Substances and Products, and Experiments; Pharmaceutical Processes.

WE have received some official details concerning the Universal Exhibition to be held at Vienna from May 1 to October 31, 1873. Her Majesty has appointed a large British Commission, headed by the Prince of Wales, to look after British interests in connection with this exhibition, which is expected to be of unusual interest, on account of Vienna being in a measure half way between East and West. The exhibition is to be held in the Prater—"the Windsor Park of Vienna"—in buildings erected specially for the purpose, and in the surrounding park and gardens. At no previous International, to judge by the official programme, have so many facilities been afforded to exhibitors. Especially is every encouragement given to the bringing forward of machinery of all kinds, the cost of exhibition to the owners being reduced to a minimum. If the Exhibition does not turn out a success, the blame can hardly be attributed to the Austrians. All information will be given to those who desire it by Mr. Philip Cunliffe Owen, Secretary to the Commission, 41, Parliament Street. British exhibitors can communicate with the Austrian Commission solely through the British Commission.

A BEQUEST of 10,000 fr. has been made to the Academy of Medicine in Paris by M. Fabret, for the purpose of founding a prize in mental and nervous diseases.

THERE has recently been formed at Amiens a Society calling itself "Société Linnéenne du Nord de la France," having for its object the study of all the branches of Natural History in its wide sense, and is divided into three sections—Zoology, Botany, and Geology. The society will hold general meetings, as well as meetings of sections, and during the favourable season will make excursions for the purpose of exploring the surrounding region. It intends to publish annually a volume of memoirs, and a monthly *Bulletin des Sciences Naturelles*.

WE are glad to be able to record the addition to the Brighton Aquarium, of a specimen of Muller's Topknot (*Rhombus hirtus*.) It was netted off the Brighton coast last week. But one capture of this rare fish off the Sussex coast is recorded by Yarrell, but it is more frequently taken off the Cornish coast. The interesting event is also announced of the birth of a young cuttle-fish, which signalised its entrance into the world by an immediate discharge of the sepia fluid.

SOME little time ago we referred to the proposed aquarium for Manchester (NATURE, vol. v. p. 487). A recent report of a meeting of shareholders, shows that it is hoped the building will be completed and opened next spring, and that temporary buildings with the necessary tanks have been provided for the reception of such marine animals and plants as can only be obtained in the summer season. They will then become acclimatised, and the tanks can be afterwards removed to their permanent places in the aquarium.

THE Committee for the Recording of Earthquakes in Scotland reported at the British Association Meeting that none had occurred within the past year. However, about the very time the report was being drawn up, and a few days before it was read, Scotland was visited by quite a sharp shock, which is thus described in the Scotch papers:—On Thursday, the 8th of August, an earthquake shock was felt at ten minutes past four o'clock at the Bridge of Allan, and was also felt over a considerable extent of country. At Braco and Kinmuck panes of glass were broken; at Dunblane and Bridge of Allan a number of houses were severely shaken, and glass was broken on sideboards. At about the same time the shock was felt in Stirling, and was attended by a loud rumbling noise like thunder. It is stated that no such sharp shock has been felt since 1839. In the evening a fine display of aurora borealis was observed in South Wales for nearly two hours.

A SLIGHT shock of earthquake was felt at Chopea, in Khandeish, on the evening of Friday, July 12, at about seven o'clock. The shock lasted for about a minute, and appears to have been felt at the same time at Amalner, Dhurrangaon, Dhulia, and Julgaon. Its course was from west to east.

A SLIGHT shock of earthquake is also reported to have been felt at Tripatore, in the Salem district in India, on the morning of the 15th of July.

AN earthquake shock was felt along the Long Island coast on July 11, and also in the northern part of Missouri, but it did no damage. A good deal of fright was caused in various Long Island villages.

A VERY violent tornado passed over Philadelphia on August 6. It was accompanied by vivid lightning and loud peals of thunder, and a tremendous downpour of rain. Trees were blown down, houses and churches unroofed, and much damage done by falling bricks and tiles, &c. No loss of life is reported, but the damage to property throughout the city is immense.

MANY of our readers will be glad to hear that Mr. Robert Swinhoe, H.B.M. Consul at Ningpo, China, is recovering from the serious illness which laid him prostrate about a year ago. Letters which were received from him by the last mail announce his gradual convalescence, and that he was already resuming the zoological investigations which have rendered his name so well known to naturalists. Mr. Swinhoe has now succeeded in obtaining living specimens of the singular hornless deer which he lately drescibed, and figured in the "Proceedings" of the Zoological Society as *Hydropotes inermis*, and will forward them to that Society's gardens by the first opportunity. He has also obtained specimens of several mammals and birds new to the Chinese fauna.

THE *Madras Athenæum* of July 19 records the death of Capt. Mitchell, the Superintendent of the Central Museum, Madras, well-known as an accomplished naturalist and a gentleman of most engaging personal character.

DR. SCHWEINFURTH, the renowned German explorer in Central Africa, is about to return with the object of continuing his explorations, chiefly in the interests of botany. His brother, a merchant at Riga, has come forward with a handsome sum of money, the interest of which will be given to aid Dr. Schweinfurth in his undertaking, and will afterwards be handed to the Polytechnic School of Riga, to found a prize to defray the travelling expenses of future explorers who may have studied there with success.

WE learn from the *Madras Times* of July 18 that the pioneering party which was sent to Cummum some five or six months ago to bore for coal have been very successful in their operations, notwithstanding the difficulties and hardships they had to contend against. Three distinct and promising seams of coal have been struck, and the quality of the coal is spoken of as being superior to that of Sasti. The party will return to renew operations at the fall of the year. The attention of Government is also being drawn to the discovery of copper in certain districts where hitherto its presence was unknown. In the Nagur Kurnool the existence of this ore has recently been discovered, and samples of it are, with a piece of copper wire manufactured in the district, transmitted to Government, who have forwarded them to scientific men for examination and report. It is also said that copper ore has been discovered near Yedlabad, in the Indoor district; but doubts are entertained as to whether the ore is indigenous to the locality where it was found, or whether it has been washed down from the hills to the west of Yedlabad. This doubt is to be cleared up as soon as opportunity offers. The Government have also been recently directing their attention to the iron ore of the Cummum district. This is no late discovery, but the Government were not before aware of the abundance and excellence of the iron to be obtained in that part of their territories. This, it will be remembered, is also a promising coal district. So here we have side by side those two mighty engines of civilisation to which the "old country" is greatly indebted. It is much to be regretted that the resources of the territories are so imperfectly known. But the vigorous and earnest measures the Minister is taking to develop them will do much to bring them to light.

WE would recommend to those interested in scientific education and the establishment of colleges of science in this country the report of the Massachusetts Institute of Technology for 1871-72. The subjects embraced in the course necessary to qualify as a graduate of the Institute are varied, and the curriculum in each department is comprehensive and thorough. For example, the course in the department of Geology and Mining Engineering extends over four years, and is so arranged as to secure to the student a liberal mental development and general culture as well as the more strictly technical education which is his chief object. The space devoted to laboratories, and the prominence given to laboratory work, in Physics, Chemistry, Assaying, Blowpipe Analysis, Metallurgy, and Ore-dressing, is a marked feature of the scheme of instruction of the Institute. The Institute seems to flourish as it deserves.

WE have received a reprint from the "Proceedings of the Liverpool Geological Society" of the address of the President, Dr. Ricketts, on valleys, deltas, bays, and estuaries. In a thoroughly praiseworthy and fair spirit he considers the opinions that have been held from time to time by different geologists on the subject of denudation, and especially the formation of valleys. His own interpretation of the facts deserve consideration, and the address as a whole is worthy of perusal.

THE *Journal de Physique* for August contains the third part of M. Cornu's paper, "Sur les Mesures Electrostatiques," and a paper on the Electric Chromoscope of MM. F. Lucas and A. Cazin.

WE take the following from the *Chemical News*:—There are no handbooks on chemistry in the Italian language; but a work has been in course of publication since 1867 somewhat similar to the first edition of the celebrated German "Handworterbuch der reinen und angewandten Chemie," viz., "Enciclopedia Chemica," edited by F. Selmi, of Bologna, with the co-operation of Arnaudon, of Turin, for technical chemistry, and of Sestini, Paterno, and others, for pure chemistry. There are 2,000 subscribers to this work, which is highly valued in Italy. The Italian Government has appointed Dr. Cannizzaro, Professor of Chemistry at Rome; and 20,000*l.* has been voted by the Italian Parliament for the establishment of a chemical laboratory in Rome. The Florentine Institute (*Istituto Superiore*) receives, in addition to its present subsidies, an annual subsidy of 25,000*l.*, and will be converted into a kind of Polytechnic University.

THE following is from the *British Medical Journal*:—M. Lindeman continues his investigation of the parasitic bodies (Gregarinidæ) found on the false tresses and chignons commonly worn by ladies. They are to be found at the extremity of the hairs, and form there little nodosities, visible, on careful examination, to the naked eye. Each of these nodosities represents a colony of about fifty psorosperms. Each psorosperm is spherical; but, by the reciprocal pressure of its neighbours, it is flattened, and becomes discoid. Under the influence of heat and moisture, it swells; its granular contents are transformed into little spheres, and then into pseudo-navicellæ—little fusiform corpuscles, with a persistent external membrane, and enclosing one or two nuclei. These pseudo-navicellæ become free, float in the air, penetrate into the interior of the human organism, reach the circulatory apparatus, and produce, according to this author, various maladies—"cardiac affections, especially valvular affections, Bright's disease, pulmonary affections." M. Lindeman calculates that, in a ballroom containing fifty ladies, forty-five millions of navicellæ are set free; and he concludes that it is necessary to abolish false hair, which often proceeds from unclean persons.

WE have two small blue-books issued by the Government Meteorological Committee. They are both translations, specially intended for the use of seamen. The one, a paper issued by the Royal Meteorological Institute of the Netherlands, is Lieutenant Cornelissen's "Notes for the Navigation of the Indian Ocean between Aden and the Straits of Sunda," which gives details concerning the winds that affect that ocean, and contains four charts, having the outward and homeward routes for the four quarters of the year clearly indicated. The other is translated from No. III. of the *Mittheilungen aus der Norddeutschen Seewarte*, being a paper on "The Winds, &c., of the North Atlantic along the tracks of steamers to New York," and contains some carefully-constructed tables, showing the distribution, force, &c., of winds and storms in the North Atlantic throughout the year.

THE BRITISH ASSOCIATION

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE

On the Duty of the British Association with respect to the Distribution of its Funds, by Lieut.-Col. A. Strange, F.R.S.

It is probably well known to most men of science in England that the British Association succeeded, in 1870, in inducing Her Majesty's Government to appoint a Royal Commission to inquire into the whole question of scientific instruction and the advancement of science. The Commission is composed of the Duke of Devonshire (chairman), the Marquis of Lansdowne, Profs. Stokes, Sharpey, Huxley, H. Smith (of Oxford), Sir John Lubbock, Sir J. Kay Shuttleworth, Mr. Samuelson (members), and Mr. J. Norman Lockyer (secretary). This powerful body has, since its constitution, been sedulously engaged in taking evidence. That which relates to "Scientific Instruction" has been already published, and it is now engaged in the second branch of its inquiry, namely, the "Advancement of Science." It is to this latter branch that the present communication refers.

Since the movement was begun at the Norwich Meeting of the Association in 1868, great progress has been made towards the formation of definite views as to the duty of the State with respect to Science. As having been myself examined by the Commission, and as having been in communication with many of the witnesses who have appeared before it, I am able, without intending to anticipate the publication of the proceedings, to say that the following specific points have been forcibly and extensively brought under their consideration:—(1) That the objects of scientific teaching and of scientific investigation are distinct, and require for their respective attainment distinct machinery; (2) That the State is bound, in the interests of the community, to maintain institutions—such as laboratories and observatories—for scientific research, apart from teaching; (3) That all State scientific institutions and action of every kind should be subject to the direction of a single Minister of State; and (4) That such Minister of State should have the assistance of a permanent, paid, Consultative Council composed of eminent men of science.

Of these measures, unquestionably the most important are the two last—a Minister and a Council. I believe I am justified in saying that the Commission are giving their most earnest attention to those fundamental steps on which the whole fabric of a consistent administrative system for science must be based.

The question having reached this point, it appears to me that the British Association may well now consider how they may still further advance it; and I beg to tender a suggestion to that end.

Of all difficulties in the way of science reform which, in the course of my study of the question, has appeared to me the most obstructive, I should pick out the confusion of thought which prevails as to what scientific objects should properly be undertaken, as a duty, by the State, and what objects may be safely left to be worked out by private enterprise. The confusion of thought exists even amongst scientific men; and it is characteristic in a much more marked degree of the occasional references to science made by the Government and by politicians generally. It appears to me that the British Association has it in its power to clear up this most prejudicial obscurity, and to contribute powerfully to the much-needed scientific education of the Government.

I will here briefly allude to a few recent examples of the confusion of thought to which I allude.

Mr. Gladstone, on two late occasions—namely, at the anniversary dinners respectively of the Civil Engineers' Institute and of the Royal Society—expressed the opinion that the more science was left to itself the better for it. He termed the intervention of the State as "interference" with science, calculated to discourage individual exertion, and so obstruct discovery and progress. If this opinion be sound, let us see what its consistent application must lead to. It must logically mean that the Royal Observatory, the British Museum, the Ordnance and Geological Survey, and our various botanical gardens and other scientific institutions, should forthwith be abolished as "interferences." No one has yet ventured to recommend this.

Such a recommendation would no doubt at once be met, even by Mr. Gladstone, by a clear exposition of the great importance to the nation of such institutions, and of the reasons why they cannot be maintained in efficiency but by the State. At once his general and sweeping proposition would thus be shown to be liable to so many and such extensive exceptions as utterly to destroy it as the basis of the argument. He would have, therefore, to deal with each example of proposed State intervention on its merits, and, before resisting it, to show that either it was not needed by the community, or that, being within the fair scope of private action, Government aid could be dispensed with. And this, in fact, is the only way in which we can possibly test the claims of science on the State. It seems to me that to substitute the term "interfering with science" for the more correct one of "aiding science" is as fair and rational as it would be to term our Post Office an interference with freedom of correspondence, our Railways an interference with freedom of communication, or our Police an interference with the good order of community. Yet many persons, who will not give themselves the trouble to think, will accept a word falling from an eminent man like Mr. Gladstone, even when so grievously misapplied as this, and will found upon it the most mischievous conclusions. I do not doubt for a moment that Mr. Gladstone used the word in all good faith, but I am also forced to believe that he cannot have applied his powerful and logical mind to this subject with the same energy and earnestness which have given him the mastery of so many others of equal or greater difficulty. When he has given it his full consideration, as he will shortly no doubt have to do, I am confident he will withdraw from the indefensible position he lately assumed.

Another recent example of confusion of thought as to the duties of the State is afforded by the refusal of the Government to aid in tidal researches. A committee of the British Association—of which its late president, Sir William Thomson, is chairman—has been for several years engaged on this subject. The funds provided by the Association being exhausted, application was made to Government for 150*l*. in aid. Her Majesty's Lords of the Treasury replied in these words:—"That they are fully sensible of the interesting nature of such investigations, but that they feel that if they acceded to this request it would be impossible to refuse to contribute towards the numerous other objects which men of eminence may desire to treat scientifically. Their Lordships must, therefore, though with regret, decline to make a promise of assistance towards the present object out of public funds."

If we contrast this refusal of aid to tidal researches with the aid afforded to the two last expeditions to observe solar eclipses, we shall be forced to conclude that mere parsimony and indifference to science cannot have dictated it, but that our statesmen have as yet arrived at no principle whatever on which such questions should be dealt with. The importance of eclipse observations is very great, but such researches partake, in the present state of our knowledge, more or less of a speculative character, whilst tidal researches, though bearing on various high cosmical problems, contribute in the most direct and practical manner—obvious to the least scientific person—to the welfare of our commerce and navy, and to many other branches of national activity. Yet several thousands of pounds, with the use of ships, were freely accorded by Government in the one case, and 150*l*. refused in the other. Such extraordinary inconsistency can only arise from the absence of due knowledge on the part of Government as to what are duties of the State in science and what are objects fairly devolving on private exertion.

The question I would now ask is—How can the Association help to impart this indispensable knowledge to Government?

In my opinion this can be done by the adoption on the part of the Association of some more settled rule for aiding science than those which at present prevail. So far as I know, the Association is guided, in making grants, by two main considerations only—first, the total sum at their disposal; second, the number and relative importance of the objects proposed. Acting principally on these considerations the Association has, in my opinion, whilst prompted by the most sincere desire to advance science to the utmost, contributed somewhat to the confusion of thought to which I wish to draw attention. By aiding numerous objects which, under a systematic administration of science, would unquestionably devolve on the State, they have, I fear, helped to justify that undue reliance on the all-sufficiency of private enterprise which Mr. Gladstone expressed, and on which the Government, in the case of the tides and in many others, have acted.

The remedy for this evil—which is every day becoming greater—that I would now propose is that the Association, in making their grants, shall discriminate more than they have hitherto done between objects which are national and those which are not national; and that they should give the preference to the latter. I would further recommend that a list of national scientific researches requiring immediate attention should be forwarded yearly by the Association to the Government, with such advice as to the best mode of conducting them as may seem necessary; and that previous to each annual meeting the Association should request the Government to state what had been done with respect to such researches, the result being published in their annual volume of Proceedings.

It is not by any means my object nor my wish to bring the Association into collision with the Government. Such a result is strongly to be deprecated. But I do not believe it would follow from the course I suggest, whilst I see no other mode of putting in a practical form before the administration those urgent requirements of science on which so much of the material economy of the State rests.

The Association has undoubtedly the right to distribute funds entrusted to it by private individuals as may seem best for science. And being the most powerful scientific body in the kingdom, both intellectually and numerically, the duty properly devolves on it of endeavouring to remedy evils arising from imperfect knowledge of science wherever discernible. These two considerations afford an ample justification for the course I suggest, should it seem otherwise judicious.

It may be objected that no definite test exists by which we may discriminate between the two classes of scientific objects which for brevity I will call Public and Private. I therefore propose the following:—

Public science should be characterised by three principal features—(1) Continuity; (2) Probability of Expansion; (3) Unremunerativeness to the individual cultivating it, combined with profit or advantage to the community generally; (4) Costliness. Each individual research, properly Public, may not present all these three features in equal prominence, but with the great mass of questions which come before the Association there will be no difficulty, in most cases, in arranging each under the proper category. Cases of doubtful character must be classified according to the discretion of the Association, than whom no one can be more competent for the task.

I would here, in order to illustrate my meaning, mention a few typical cases of Public Science which the Association and other private bodies and persons have attempted to deal with. The Kew Observatory was one such case. An institution of that kind satisfies exactly all the three conditions constituting it, according to my definition, Public Science. It was discontinued chiefly on the ground that the cost of its maintenance, 600*l.* per annum, absorbed an undue proportion of the income of the Association. I was one of those who assented to its discontinuance, but on other grounds besides the one I have named. I considered that the annual sum spent upon it, though a large one relatively, was quite insufficient for such a purpose, that it needed great expansion, and finally that as long as such an institution existed, even on so contracted a scale, Government would not found a really sufficient one, as I considered they ought to do. After its relinquishment by the Association, Mr. Gassiot came forward and undertook the cost of its continuance. While offering my humble meed of admiration of such rare liberality, I must still say, what I urged when the arrangement was first proposed, that, though supplying to a certain extent a temporary want, it could not do so on the requisite scale, and that its effect would be to postpone the period at which Government would be able to see that such institutions must be maintained on the most liberal and comprehensive scale at the public expense. I also instance the subjects of Sewage, of Rainfall, of the Map of the Moon, and of the Tides, as bearing all the three characteristics of Public Science—and many others could be added. As to the Sewage question, the committee which undertook it no sooner began their labours than they perceived the utter inefficiency of the funds allowed by the Association. They sought to supplement these by appeals for help to the large towns, and questions of a delicate nature respecting the personal expenses of the members of the committee led to unpleasant consequences—the whole showing clearly that the subject was far too extensive, costly, and arduous for the powers and resources of private enterprise, and that it was, in fact, a national question which could be grappled with by national agency only.

As to the Rainfall question, it is well known that for several years Mr. Symons has devoted the greatest energy and skill to it, and that he has established a considerable number of rain-gauges all over the kingdom. He has received annual grants of money from the Association, but, unless I am misinformed, he considers the system which he has created still incomplete, and I believe he has incurred very considerable sacrifices in bringing it to its present condition. Now this case strongly illustrates the evil on which I have dwelt.

When, on the death of Admiral FitzRoy, the present Meteorological Office was established, no provision whatever was made for ascertaining the general rainfall of the kingdom. This, it was found, was being done by private enterprise, and the excuse for excluding one of the most important of meteorological elements from the programme of the State Meteorological Department was eagerly seized, although a little reflection would have shown that the very existence of the private system depended on the zeal and life of a single individual; indeed, it was threatened with total collapse that winter, in consequence of Mr. Symons' health failing from overwork.

The Map of the Moon was another large subject to which many eminent astronomers—of whom the late Sir John Herschel was one—attached much importance. The funds which the Association was able to furnish for the purpose being utterly inadequate, the project was abandoned. It is still kept alive by the zeal of Mr. Birt, assisted by a few friends; but its complete realisation by such means may be considered as indefinitely postponed. I am justified in assuming that the fact that the Association did make the attempt will be used, when the subject is pressed on the Government, as a proof that it is one properly devolving on private enterprise. I have already described the position of the Tidal question, which constitutes the most conclusive demonstration of the effect on the Government of such undiscriminating application of private funds and private enterprise.

I trust I shall not be considered, in consequence of what I have said on the subject, to depreciate or undervalue private enterprise. It is one of the just boasts of Englishmen that in no nation are there to be found such wonderful examples of individual zeal for high and unremunerative objects as here. Our colonisation, libraries, museums, hospitals and charities, the missionary agencies—all supported by voluntary means—surpass those of all the world put together. In some instances—as in that of our charities—private beneficence, for want of due direction, has actually gone too far and done harm. In science, also, England is pre-eminent for the number of observatories, laboratories, and other forms of activity maintained by individuals; whilst no civilised nation is so backward in its State organisation for science. The question I wish to raise is whether our private science, exuberant as it is, suffices for the national wants in the present age; and if not, whether the indiscriminateness of private scientific enterprise in England has not tended to induce the feeling that science is already sufficiently provided for, and does not need what Mr. Gladstone calls "the interference of the State."

I would strongly urge that no State organisation of science can possibly chill the zeal of private enterprise. The love of individual exertion, the pride of persona prowess, and the liberality of private wealth, are sentiments too deeply imbedded in the genius of Englishmen to be capable of such easy eradication. Nor will the Association find, that by the classification of scientific objects which I have recommended, there will remain of those which may properly devolve on individuals a number insufficient to absorb the moderate income which it has at its disposal.

The only other effect of such classification that appears to me open to possible objection is that some of the scientific objects which would otherwise be, however imperfectly, advanced by the help of the Association, may, by being referred to the Government, suffer total neglect. I at once admit that this effect will at first very probably take place in some instances. But for my own part I should be prepared to incur the risk of that sacrifice, in order to expedite a full consideration of the great question of the duty of Government towards science. The measure I propose is avowedly not intended to meet at once pressing temporary want, but to contribute to the creation of a great permanent system. It is my wish, not to ease, by small local applications, the sufferings of the patient, but to effect a radical cure. The treatment has as yet consisted of small expedients which, though they have given occasional relief, have not touched the seat of the disorder. I do not shrink from recommending, instead, a bold, if painful, operation that shall strike at the very root of the malady.

Report of the Committee for Discussing Observations of Lunar Objects Suspected of Change, by W. R. Birt.

The Committee have pleasure in presenting their Second Report on the above subject. It will be remembered that the Report of last year was confined principally to the discussion of the possible variations of visibility of the numerous spots and craterlets upon the floor of Plato under the same conditions of illumination. That now presented is directed chiefly to the discussion of the various streaks and bright patches which interlace the spots and craterlets. One interesting and important change has been fairly shown, the floor of Plato becomes darker with the increase of the sun's altitude, Mr. Birt has offered an explanation of the phenomenon; whatever be the true cause of this change, it is very difficult to account for it by the ordinary laws of reflection; when we consider the varying aspect of the streaks at the same time of the luni-solar day, we cannot but think that, with careful observations made with powerful instruments such as the Newall refractor and many others, we may be able to confirm or otherwise a physical explanation of these curious changes involving the existence of certain gases and vapours upon the surface of the moon. The Committee can only look upon the study of Lunar physics as in its infancy, and they trust that in future years the Association will not overlook this important branch of astronomical inquiry.

After reading this Report of the Committee, Mr. Birt read extracts from his report of the discussion of the streaks on the floor of the lunar crater Plato which had been entrusted to him, the opening portion was to the following effect:—

"In completing the task assigned to me of discussing the observations of the streaks on the floor of Plato, I have been desirous of including every, even the most minute circumstance bearing on the exhibition of phenomena that may possibly illustrate the condition of a small portion of the moon's surface from April 1869, to April 1871. Drawing my conclusions from the experience of twelve years, I feel that I may confidently say that it may be some years before another series of observations of a particular region will be undertaken with the view of so closely examining the spots and streaks characterising it, unless a staff of efficient observers be organised, with the provision of a fund sufficiently ample to defray all the necessary expenses. The work is a difficult one, the staff should consist of not less than six devoted observers who would independently and most probably, as in the present case, work with instruments of varying aperture, and carefully record all their observations. The principal qualification is a keen eye for the appreciation of delicate variations of tint and the detection of minute spots of light, with a readiness of referring them by estimation and alignment to the respective localities of the region on which they are seen. The observations should not be allowed to accumulate, but should be forwarded at once to an experienced selenographer charged with the work of arranging and discussing them. Taking into consideration the results of the discussions of the present and previous years embodied in the two reports, it appears that in order to confirm these results and to open up new investigations the requisite time cannot well be fixed at less than three years—five would most probably afford the best results.

"The results of the present work may be briefly characterised as confirming by a direct reference to the sun's altitude above the horizon of Plato the supposition that variations of tint in some measure depend on increasing and decreasing altitudes. The ascending and descending branches of the curve obtained from independent estimations of tint by the several observers, are sufficiently near those of the sun's altitude to enable me to delineate a normal curve representative of the sun's influence in darkening the floor of Plato, or else in overspreading it with something of the nature of a dark covering as his rays strike the surface at the increased angle of about forty degrees. While this darkening influence comes out most unmistakeably, there are variations in the lighter and darker portions of the floor which are quite irreconcileable with solar influence of a gradual character. The treatment of the observations under intervals of the luni-solar day fails to bring out any regularity in these variations, and it is only by treating the observations *chronologically* that the true sequence of the changes can be detected."

Mr. Birt proceeded to notice that in order to assist in showing more distinctly the changes observed on the floor of Plato and their connection with certain supposed agencies, he had introduced the *hypothesis* of a dark obscuring medium overlying the surfaces of the lower parts of the moon; he did not insist upon this hypothesis further than its utility in connecting the observations. The Report, which was rather voluminous, referred especially to the influence of the sun on the floor of Plato; an examination of the changes recorded in August 1869, formed a separate portion of the Report as well as the history of a single streak from its first detection in September 1869, to the close of the observations. A considerable portion of the Report consisted of "Notes" furnished by the several observers.

SECTION B—CHEMICAL SCIENCE

On Filiform Native Silver, by J. H. Gladstone, F.R.S.

The object of this communication was to show that metallic silver might be obtained artificially in the same filiform condition in which it frequently occurs in a mineral, and thus to throw light on the origin of this native variety. Specimens of the metal were exhibited, from Kongsberg in Norway, associated with calc-spar, and from Chili, associated with greenstone, and in each case the silver resembled twisted threads or wires non-crystalline, but often bending at sharp angles. Under the microscope were exhibited precisely similar threads of silver produced by the decomposition of nitrate of silver by sub oxide of copper. The latter substance is partly dissolved and partly converted into the black oxide, while filaments of the white metal shoot forth and bend in every direction. Most of these are extremely fine, perhaps $\frac{1}{25000}$ of an inch in thickness, so that, as was said, a gramme of such wire would stretch from London to Brighton. Since sub-oxide of copper is no rare metal, it seems probable that filiform native silver may often, if not always, originate from it.

SECTION C.—GEOLOGY

On the Occurrence of a remarkable Group of Graptolites in the Arenig Rocks of St. David's, South Wales, by John Hopkinson, F.G.S., F.R.M.S.

In a series of black, iron-stained shales, about 1,000 feet in thickness, which form the lowest beds of the Silurian rocks in the immediate vicinity of St. David's, the author noticed the occurrence of about twenty species of graptolite, which, he considered, furnished conclusive evidence of the equivalency of these beds with the Quebec group of Canada, the Skiddaw slates of Cumberland, and the Arenig rocks of Shelve.

The Graptolites, of which there are more than twenty species, were collected in the lower beds of the series at Ramsay Island and Whitesand Bay. Of the true Graptolites, or *Rhabdophora*, the only genera of undoubted occurrence are *Didymograptus*, *Tetragraptus*, and *Phyllograptus*. *Didymograptus* is represented by five species, three of which—*D. extensus* Hall, *D. patulus*, Hall, and *D. pennatulus* Hall—are characteristic of the Quebec group and the Skiddaw slates, *D. patulus* also occurring in the Arenig rocks at Shelve; the other species are new. Of *Tetragraptus* but one species, *T. serrus* Brong., also a Quebec and Skiddaw form, has been found. *Phyllograptus* also is only represented by a single species, which is new. There is also another new species—a very peculiar branching form referred provisionally to *Loganograptus*. The absence of any specimens undoubtedly referable to *Dictyograptus* is remarkable, as this is a common Quebec genus. *Diplograptus* and *Climacograptus*, genera of very rare occurrence in the Quebec group, have not as yet been found here.

Of the allied forms, all the genera of the so called "Dendroid" Graptolites, so characteristic of the Quebec group, are present in the St. David's beds. *Phyllograptus* is represented by two new species, and *Dendrograptus* by five species, three of which—*D. divergens* Hall, *D. flexuosus* Hall, and *D. striatus* Hall—are at present only known to occur elsewhere in the Quebec group, the other not being new. *Cailograptus* is also represented by five species, three—*C. elegans* Hall, *C. diffusus* Hall, and *C. Salteri* Hall—being Quebec forms, and two being new; and, lastly, of *Dictyonema* but one species, which is new, has been found. Many obscure impressions referred to the genus *Retiolites* also occur, one species seeming to agree perfectly, as far as its state of preservation allows of comparison with Prof. Hall's figures, with his *R. ensiformis* of the Quebec group. Another appears to be distinct from any species yet figured.

The Graptolites and their allies are now thus known to be represented in the Arenig rocks of St. David's by nine genera and about twenty-two species. Of the true Graptolites three genera—namely, *Tetragraptus*, *Loganograptus*, and *Phyllograptus*, are exclusively confined to the horizon of the Quebec and Skiddaw groups. The remaining genus, *Didymograptus*, is represented in higher rocks but by one species, *D. Murchisoni*. With this exception, *Didymograptus* is exclusively an Arenig genus occurring in rocks of this age in Canada, Cumberland and Shropshire. The four genera of dendroid Graptolites have a more extensive range, but until now they were only known to occur together and in any abundance in the Quebec group of Canada.

The author then stated that he could now give another locality for these genera. During a recent visit of the Geologists' Association to Ludlow and the Longmynds he had found, at Shelve, in the lower part of the Arenig rocks, underlying the great mass of the Llandeilo, a Graptolite zone in which these four genera are represented by species, some of which are identical with, and others nearly allied to, those in the St. David's beds and in the Quebec group of Canada; these beds, and also the Skiddaw slates of Cumberland, being therefore of Lower Arenig age.

Prof. Harkness, after referring to the labours of Mr. Hicks, remarked upon the occurrence at so early a period, of so many new forms of life, alluding especially to the discovery of a star-fish. He thought the name Skiddaw would be more appropriate for the beds immediately underlying the Llandeilo flags, than Arenig, although he acknowledged that the latter name was one which had been a long time in use.—Prof. James Hall, of Canada, on being called upon, said that he had examined the specimens on the table and was much pleased to find the Graptolites from St. David's so intimately allied to those he had described from the Quebec group; indeed had he not known where these specimens had been obtained he should have thought that they had really come from some of the beds in Canada. Allowing for differences caused by pressure and cleavage, the resemblance between these and the Canadian forms was truly remarkable.—Dr. Nicholson said that *Didymograptus* had a wider range than Mr. Hopkinson had given, several species of this genus being found in the Llandeilo flags in the south of Scotland and elsewhere.—Mr. Hicks, in his reply, said that he was pleased to find that Prof. Hall so thoroughly agreed with his ideas of these beds in regard to their equivalents in Canada. In reference to the remarks of Prof. Harkness as to the name of Skiddaw being preferable to that of Arenig, he said that as some of the rocks in the Shelve district which had for some time gone under the name Arenig, were now proved to be the equivalent of these beds, he thought it best to adopt this name.—Mr. Hopkinson, in reply to Dr. Nicholson's observation on the genus *Didymograptus*, stated that the species referred to by Dr Nicholson, in his opinion belonged to a distinct genus to which he had given the name *Dicellograptus*, and which differed entirely in structure from *Didymograptus*. He was very glad to find his views of the equivalence of the St. David's beds, as shown by their Graptolites, with the Quebec group of Canada so decisively confirmed by Prof. Hall.

Saturday, August 17.—*Sur les divisions de la Craie en France, leurs limites et leur faune, l'identité de ces divisions des cotés du détroit*, by Prof. E. Hébert.

The author objected to the divisions of the chalk commonly adopted in England, into chalk with flints and chalk without flints. He proposed to subdivide the chalk according to the characteristic fossils of certain horizons, affirming that the divisions thus adopted were constant, and could be applied as well to the English chalk as to that of France. Taking the Gault as the natural base of the chalk, he classed the overlying beds, in ascending order, as follows:—1. *Craie glauconieuse* (Upper Greensand and Grey Chalk); 2. *Craie à Inoceramus labiatus* (Chalk marl, chalk without flint, and part of the chalk with flint); 3. *Craie à Micraster cor-testudinarium* (part of the chalk with flints); 4. *Craie à Micraster cor-anguinum* (chalk with flints); 5. *Craie à Belemnitella mucronata* (Norwich chalk).

Between the first and second divisions comes the great series of Sandstones of the Maine; and between the second and third division comes the Hippurite limestone. These beds are not represented on the coasts of the English Channel, but at the points where the "breaks" occur there are hardened beds of chalk, often pierced with holes.

The author showed that the chalk area of the North West of France is traversed by five well-marked anticlinal folds, which run in a general south-westerly direction, but converging somewhat towards the coast. These folds the author identified with some on the English coast.

Mr. Davidson then made some remarks upon Prof. Hébert's paper, pointing out that the Upper Greensand in parts of England assumes a much more important character than that given to it by Prof. Hébert. Prof. Phillips, Mr. Godwin-Austen, and Mr. Seeley also took part in the discussion, Prof. Phillips remarking upon the great good that results from meetings of this kind, at which geologists of different countries could meet and personally discuss their views.

Monday, August 19.—Three Reports were read at the commencement of the meeting. That by J. Thomson, *On the Continued Investigation of Mountain Limestone Corals*, dwelt upon the great difficulty which had been experienced in determining the species and genera of corals. The author showed that all systems of classification founded upon the arrangement of special parts of the coral were artificial and misleading. He had prepared careful drawings tracing the coral through the whole of its stages of growth, and he believed that only in this way could we arrive at satisfactory results.

Prof. Duncan and Prof. Jas. Hall both insisted upon the great difficulties that were encountered in classifying corals. It was clear from this discussion that in the corals at least there is no lack of the "intermediate forms" which Mr. Darwin assumes to have existed in all groups of animal and vegetable life.

Dr. Bryce, in his *Report on Earthquakes in Scotland*, said, that nothing of importance had occurred during the past year, no disturbance of the earth's crust or oscillation of the lakes had been observed. The attention of the committee had been turned to the remedying of those defects which from time to time are apt to occur with instruments long in use. It was stated that the Seismometer belonging to the Association, which now occupies the lower part of the parish church of Comrie, is of too complex construction for general use. Simpler and cheaper instruments have been constructed by Mr. Geo. Forbes, which will be distributed amongst the stations of the Scottish Meteorological Society, and the results obtained will be detailed in a future report.

Mr. W. Jolly's *Report on the Discovery of Fossils in certain remote parts of the North West Highlands*, was likewise richer in promise than performance. Certain work had been done in investigating the limestones occurring with the Laurentian Quartzites and Sandstones, but the best part of the report was the announcement that the clergy and schoolmasters of the district had entered warmly into the projects, it was therefore hoped that before the next meeting of the Association a good deal of valuable information as to fossiliferous localities would be obtained.

On the Geology of the Thunder Bay and Shabendowan Mining Districts on the North Shore of Lake Superior, by H. Alleyne Nicholson, M.D., D.Sc.

Having recently had an opportunity of accompanying an exploring party to the north of Lake Superior, the author had been able to examine geologically the silver-mining district of Thunder Bay, and the gold-bearing district of Shabendowan (sixty miles to the north-west of Thunder Bay). Having described the chief geographical features of Thunder Bay, the author gave an account of the series of rocks known as the "Lower and Upper copper-bearing series." The chief argentiferous lodes were also described, and the more important mines were shortly noticed. The leading geological features of the country between Thunder Bay and Lake Shabendowan were next glanced at, and a detailed account was given of the geology of Lake Shabendowan itself The most interesting rocks described are the so-called "talcose" slates, in which the auriferous lodes are situated. These slates are of Huronian age, and they occupy, along with interstratified and intrusive igneous rocks, a vast area, which extends to an unknown distance north of Lake Shabendowan. Having described their mineral characters, the author expressed his opinion that these "talcose slates" are truly of the nature of bedded felspathic ashes, and that the talc which they often contain is a secondary product developed in them as the result of the metamorphic action to which the whole series has evidently been subjected. It was shown also that these Huronian slates, with their interstratified traps, presented the most striking resemblance to the Borrowdale series of green slates and porphyries of Cumberland and Westmoreland. The paper concluded with a description of the chief auriferous veins which have hitherto been found traversing these rocks.

On Ortonia, a new genus of Fossil Tubicolar Annelides, with Notes on the Genus Tentaculites, by H. Alleyne Nicholson, M.D.

Having recently had the opportunity of carefully investigating the genus *Tentaculites*, the author was led to the conclusion that fossils of diverse zoological affinities had been included under this head (Amer. Jour. Science and Arts, vol. iii. 1872). The author showed that some fossils formerly referred to *Tentaculites* were truly Pteropods, whilst others were genuine tubicolar annelides. For some of the latter he had proposed the genus *Conchicolites*; and he restricted *Tentaculites* to straight unattached conical tubes. With this restriction the genus may safely be regarded as Pteropodous, since no Pteropod has an irregularly bent or twisted shell, and none can possibly have a shell attached parasitically to foreign bodies. In the present communication the author founded a new genus for the reception of a fossli which had been kindly sent him by Mr. Edward Orton, of the Geological Survey of Ohio, after whom he proposed to name the genus *Ortonia*. This fossil had formerly been referred to *Tentaculites*, and is of common occurrence in the Lower Silurian (Hudson River group) of south-western Ohio. No doubt can possibly be entertained as the proper reference of this fossil to the tubicolar annelides. Only a single species is known, which the author named *O. conica*, and this occurs in the form of conical tubes attached by the whole of one surface to the shells of Brachiopods and other molluscs, *Strophonema alternata* being the form which is most commonly infested in this way. The sides of the tubes are furnished with strong annular ridges, which die away upon the dorsal surface, leaving a narrow vacant space or belt of a peculiar cellular character, exhibiting numerous small alveoli, strongly reminding one of the peculiar cellular structure of the tube of *Cornulites*. From this latter *Ortonia* is separated by the complete attachment of the tube along one side, and by its much smaller size. From *Conchicolites* it is distinguished by its mode of attachment, and by never growing socially in clustered masses.

The Rev. Canon Tristram's paper *On the Geology of Moab* was then read. After referring to the researches of M. Lartet and others, the author described the general structure of the southern end of the Jordan valley, which, he said, coincided with a great synclinal depression. The lowest rocks exposed are New Red sandstone; these occur only on the east side of the Jordan, and are there capped by tertiary limestone, resembling that of the "back-bone" of Palestine. Abundance of springs break out at the junction of the limestone and the New Red, rendering the eastern shores of the Dead Sea very fertile. On the west side only three springs occur, and, excepting near these spots, the country is barren. Great deposits of marl are heaped against the western banks, but only a little of this occurs on the eastern side. Many streams of basalt occur on the eastern side of the Dead Sea. These overlie the tertiary limestone, and are, therefore, of later age than that. The origin of the lava flows is not yet known—no craters were observed in this district.

To the north-east of the Dead Sea, on the east of the New Red plain, there is a range of hills formed of tertiary limestone. Beyond, to the east of this, the Arabs tell of a vast volcanic tract, covered with ruined cities, which is as yet wholly unexplored.

In the course of the discussion, Prof. Hull remarked that the statements of the author gave a good example of the formation of a valley by disturbance, and he thought that comparatively little was due to denudation. Mr. Topley thought that even if a fault or synclinal ran along the valley, yet the valley itself was still due to denudation. Even if this were not the case, there was the line of hill, or an escarpment of tertiary limestone, on the north-east of the Dead Sea. The westerly continuation of this had been removed by denudation. He saw no reason why the whole of this denudation should not have been subaerial, the material having been carried southwards down the continuation of the Jordan valley before the great depression was proved. All the evidence compels us to believe that the great depression is of extremely recent geological age.

Canon Tristram, without giving an opinion as to the denuding agents, thought that the valley of the Jordan was marked out, and in great part formed by disturbance. In reply to Mr. Sharp, he said that the Moabite stone was a block of basalt of the country. Many such blocks of basalt are preserved at the houses there. In reply to Mr. Scott, he observed that the great deposits of salt at the southern end of the Dead Sea were of New Red sandstone age. The great saltness of the Dead Sea is mainly due to this salt being washed down by streams. Salt occurs all along the line wherever the New Red sandstone has been brought up, as in the Sahara and elsewhere.

On the Trachyte Porphyries of Antrim and Down in the North of Ireland, by Prof. Edward Hull, F.R.S., Director of the Geological Survey of Ireland.

Trachyte is one of the rarest of the British rocks, and it is as yet uncertain whether it is to be found amongst these islands except in the North of Ireland. In this district it was discovered and identified by the late Prof. Jukes and Mr. Du Noyer during the progress of the Geological survey in the year 1867.[*] No description has as yet been published of this remarkable species of volcanic rock, and I propose to give a short account of its characters and relations to the surrounding formations as it occurs both in Antrim and Down.

Trachyte Porphyry of Antrim.—The principal mass forms a group of eminences about four miles to the north of the town of Antrim, called Tardree mountain, Carnearny Hill (1,043 ft.,) Brown Dod Hill and Scolboa Hill. The tops of three of these hills are formed of basalt in beds capping the trachyte rocks, and it is supposed that basaltic sheets enclose the whole of the trachytic district; though the survey of the district being incomplete the actual limits have not been determined in every direction.

The mineral constitution of the trachyte is generally uniform, although the relative proportions of the individual minerals occasionally vary. In general, the rock consists of a nearly white or grey felspathic base, with individual crystals of Sanidine, a triclinic felspar, blebs or grains of smoke quartz, and rarely a little mica. In some places the grains of silica are exceedingly abundant, giving the rock the appearance of Rhyolite or Perlyte as described by Cotta, minute crystalline grains of magnetite appear in a sliced section under the microscope. It is in this state that the iron mentioned in the analysis below probably occurs.

The rock is quarried as a building stone at Tardree mountain, where it sometimes assumes a columnar structure. A specimen from one of the quarries was subjected to an elaborate analysis by Mr. E. T. Hardman, of the Geological Survey of Ireland, who gives the following as the constituents:—[*]

Analysis of Trachyte Porphyry, Tardree quarry.

Silica	76·960	per cent.
Alumina	5·101	,,
Peroxide of Iron	2·344	,,
Lime	7·064	,,
Magnesia	0·294	,,
Potash	4·262	,,
Soda	1·818	,,
Loss by ignition	2·102	,,
Phosphoric acid	trace	
	99·943	
Specific gravity	2·433	

Relations of the trachytic and basaltic rocks.—During a recent visit I was enabled to ascertain with the greatest certainty the relative position of the trachytic to the basaltic rocks of the district. In the first place, there does not appear to be any passage or graduation of the two classes of volcanic rock into each other, and each having been erupted and spread out in sheets, exhibits a laminated, or bedded structure, which enables the observer to determine their relative positions without much difficulty. Both at Carnearny and Tardree Hills the trachytic porphyry may be observed to dip beneath the basaltic rocks of the surrounding country; and the observations made here and elsewhere tended to show that, of the two kinds of rock, the trachyte is the older.

On the other hand, both at Carnearny Hill and Scolboa the trachyte seems to have been penetrated by "necks" of later date filled with basalt, from which some portions of the overlying basaltic sheets may have been erupted. We are not, however, as yet in a position to say whether or not the trachyte is the oldest and lowest of all the Tertiary Volcanic rocks of County Antrim, as its base is nowhere exposed.[†]

The events which have taken place in the volcanic history of this locality appear to have been as follows:—

At some early stage of the miocene period large masses of trachytic rocks were poured forth from one or more vents, doubtless accompanied by craters as in Auvergne. After, probably, a long interval of repose new interruptions of basalt and dolerite took place through fissures and small volcanic vents breaking in some places through the trachyte. These later eruptions of basalt may have enveloped the whole of the trachytic masses which have been subsequently laid bare by denudation. The denudation of this region has been very great during postpliocene and later times; and to it is due the obliteration of the actual craters of eruption over the whole volcanic region of Antrim.

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[*] Jour. Roy. Geological Soc. Ireland. Vol. iii. part i. p. 27 (New Ser.)

[†] Messrs. Hull and J. L. Warren. Explanatory memoir to sheet 36 of the Geological Survey of Ireland (1871).

Trachyte Porphyry of Co. Down. This rock is very similar in appearance and constitution to that of Antrim, consisting of a greyish felsitic base with crystals of sanidine and blebs of quartz. It is only visible at Ballyknock, about four miles west of Hillsborough, surrounded on all sides by Lower Silurian rocks, but not very far distant from the margin of the basaltic plateau of Antrim. There can be little doubt that it is of the same age as the trachyte porphyry of Antrim; both being referable in all probability to the great volcanic outbursts of the miocene period.

Considerable uncertainty exists regarding the relations of the Downshire trachyte to the volcanic rocks of the adjoining country. It only appears in two or three spots within a small area: but the probabilities are, that it is portion of an old neck from which trachytic lava was erupted contemporaneously with that of Antrim, the higher portion of the mass as well as the original vent having been removed by denudation. The district has since been deeply buried beneath boulder clay.* The author then proceeded to show the similarity of the Antrim volcanic rocks with those of Auvergne, the Siebengebirge and Eifel districts.

SECTION D—BIOLOGY

SUB-SECTION ZOOLOGY AND BOTANY

On the Occurrence of the Supra-Condyloid Process in Man, by Prof. Struthers, of Aberdeen.

The author showed dissections of this part in several animals. An arch of bone is thrown, like a bridge, over the great nerve, and generally also the great artery of the limb, a little above the elbow, protecting them from pressure and injury. No such structure exists normally in the human arm, but it occurs occasionally as a variation. When it exists, the process grows from exactly the same spot as in animals which possess it, and the arch is completed by a ligament, the nerve and generally also the artery passing through the arch. This variety had attracted some notice lately, and is supposed to be very rare, but the author has found it often, and he exhibited a large number of specimens of it from the human arm, in its various degrees of development. He had also met with it occasionally in the living body, and had lately been able to prove the correctness of his previous supposition that it may be hereditary, having met with it in the members of a family, in the father and in two sons. The author remarked on the great interest attaching to this variation. In animals which possess it, it is what, in olden phraseology, would be called a contrivance specially designed for the protection of the nerve in them, and it looks as much a piece of contrivance as London Bridge or Temple Bar. But why should the same contrivance occur as a variety in man? The old argument from final cause, and no less its successor the theory of "type," besides being metaphysical, becomes untenable in the face of the existence of these rudimentary structures. The theory of so-called type has a great deal to answer for in obscuring the natural interpretation. If species are of independent origin, how comes it that animals have in their bodies parts of other animals, parts which are of no use to them, sometimes even dangerous to them? To those who are able to overcome the prejudices of their early education, the evidence comes with irresistible force in support of the hypothesis of the origin of species by evolution.

On the Sternum and Pelvic Bone in the Right-Whale and in Great Fin-Whales, by Prof. Struthers.

The sternum exhibited showed a very different form from that of the same species of Fin-Whale which Prof. Struthers had brought under the notice of the Association last year. Instead of a singled median cervical process, it has a deep median notch with a broad crest on each side; and the posterior process is very narrow. Two sterna of the Greenland Right-Whale exhibited were large. The author divides the sternum into three parts. The middle between the first ribs is thick, completing the thoracic girdle, and essential. The part in front of this, and the part behind it vary greatly, being more or less rudimentary. The sternum of the Finner has two joints with the first rib, that of the Right-Whale only one joint, and this difference in the thoracic adaptation, together with the great breadth of the first rib in the Right-Whale, might explain the very different forms presented by this bone in these two kinds of whales.

One of these breast-bones exhibited marks of former inflammation of the bones. The author mentioned that he had often met with this condition in whales, in some cases ankylosis of vertebræ had resulted, and in some there must have been considerable suffering to the animal. This fact might be commended to the notice of those, if there be yet any such, who have the notion that disease occurs in animals only when they come under the influence of man.

On the Occurrence of Finger Muscles in the Bottle-Nose Whale (Hyperoodon bidens), by Prof. Struthers.

This bottle-nose stranded on the Aberdeenshire coast just after the meeting of the Association last year at Edinburgh, at which the author read an account of the finger-muscles in the great Fin-whale, first noticed by Prof. Flower. It had been believed that these muscles do not exist in the toothed whales, but in this bottle-nose they were even better developed than in the Finner. The extensor muscles especially were better marked, the external extensor, corresponding to the so-called extensor of the little finger of man, being also present. An extensor *carpus radialis* was also present. Besides the muscles which were known to exist at the shoulder and arm in the Cetacea, he found a representative of the biceps present here. These muscles were mainly to be regarded as rudimentary, but they had a certain low amount of function by which their presence as muscles is maintained. In some other cetaceans they are represented entirely by fibrous tissue. Prof. Struthers exhibited also a dissection of the rudimentary teeth concealed in the gum of this bottle-nose. They are alive but useless, and their presence could be reasonably interpreted only by the hypothesis of evolution.

SUB-SECTION ANTHROPOLOGY

Exploration of some Tumuli on Dartmoor, by C. Spence Bate, F.R.S.

The author had examined several cairns of the usual kind common on Dartmoor with but little success; they apparently had been previously rummaged by unknown hands. In one, a broken urn and an implement of white slate was found; the latter was supposed to have been that with which the potter formed the rude urn.

On Hamel Down, near the centre of Dartmoor, the author explored a barrow composed of earth surrounded with small stones; in this he found, beneath five large stones which were placed horizontally one beside the other, some burnt bones on the ground, a bronze dagger blade, and an amber ornament inlaid with gold, which is supposed to have been the extremity of the handle of the dagger.

The author contends that from the character of this interment, the burnt bones not being enclosed within an urn, and the amber ornament taken together with the names associated with the locality, are evidence of an early incursion of the old Scandinavian Vikinger in search of that tin which was necessary for them to manufacture their bronze.

On the Ethnological and Philological Relations of the Caucasus, by Hyde Clarke.

This paper communicates the further researches of Mr. Hyde Clarke on the classification of the languages of the Caucasus. It identifies (1) the Ude with the ancient Egyptian and Coptic; (2) the Abkhass with the Agau, Falasha, &c., of the Upper Nile; (3) the Circassian with the Dravidian; (4) the Georgian, Lazian and Sivan with the Caucass—Tibetan. The Ude and Abkhass are connected with the statements of Herodotus (Book II.) as to the Egyptian colony established in Colchus by Sesostris. Mr. Hyde Clarke observed that the Caucasus was not a centre of population for the world, but a place of passage, and showed the relations of the Abkhass (Agau) and Circassian with the Ougeners in Europe, Africa, Asia, Australasia, and America, illustrating the common population of the new and old world, and the knowledge of America by ancient nations, dimly preserved, though not understood by the Greek and Roman geographers.

The Origin of Serpent-worship, by C. Staniland Wake.

After referring to various facts showing the existence of serpent-worship in many different parts of the world, the author proceeded to consider the several ideas associated with the serpent among ancient and modern peoples. One of its chief characteristics was its power over the wind and rain; and a second, its connection with health and good fortune, in which character it was the agathodæmon. It was also the symbol of life or immortality, as well as of wisdom. This reptile was viewed by many uncultured peoples as the re-embodiment of a deceased ancestor, and descent was actually traced by the Mexicans and various other peoples from a serpent. The superstition thus became a phase of ancestor worship, the superior wisdom and power ascribed to denizens of the invisible world being assigned also to their animal representatives When the simple idea of a spirit ancestor was transformed into that of the Great Spirit, the father of the race, the attributes of the serpent would be enlarged, and it would be thought to have power over the rain and hurricane. Being thus transferred to the atmosphere, the serpent would come to be associated with nature or solar worship. Hence, the sun was not only a serpent-god, but also the divine ancestor or benefactor of mankind. Seth, the traditional divine ancestor of the Semites, was the serpent sun-god, the agathodæmon, and various facts were cited to establish that the legendary ancestor of the peoples classed together as Adamites was thought to possess the same character. It appeared that serpent-worship, as a developed religious system, originated in Central Asia, the home of the great Scythic stock from which the civilised races of the historical period sprung, and that the descendants of the legendary founder of that stock, the Adamites, were in a special sense serpent-worshippers.

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* Mr Hardman considers that the amount of lime shown by the analysis, proves that the trachyte has undergone some amount of metamorphosis or alteration, and considers it probable that it is consequently older than the basalt of Antrim, a view which subsequent examination in the field has enabled me to verify.

Sir Walter Elliot read a paper *On Some of the earliest Weapons in Use among the older Inhabitants of India.* These he traced to a curved "throw-stick" resembling, but differing from, the Australian bomerang, in as much as it does not return to the hand when thrown. The Indian "throw-stick" is found among the rude races inhabiting the mountain and forest tracks of Central and Western India, as the Dhangars, Kolis, and Gonds, and more to the South, the Kallars, Marawars, and other low castes. In waste and jungle tracts the people turn out in great numbers during the hot season, commencing with the first day of Hindu new year in March, and continued on every succeeding Sunday till the Monsoon begins Hares, deer, hog, pea-fowls, partridges, &c., raised by this lowly race of beaters, each carrying a "throw-stick," are knocked over by showers of these weapons, thrown with great force and precision.

From the form of such sticks, which are from 1½ to 2 feet long and 3 to 6 inches broad, thrown with the concave side foremost, the author deduced the form assumed by the iron weapons subsequently formed by the same races. Specimens of these were exhibited, such as the Gurkha knives of Nipal, those of the Nairs, Moplas, and of the Malabar coast, and the common woodman's knife used everywhere, and which the late Capt. Forsyth states the Gonds, Bygas, and other tribes of the central Highlands throw at game with wonderful precision. These remarks apply more particularly to the Druoidian races, although not exclusively so. The earliest or aboriginal people now represented by the servile classes, seem to have used stone implements like people of the same condition in Europe and elsewhere, while the early Aryans passed through a bronze or copper period—specimens of the weapons of which era were exhibited.

Sir Walter observed that Prof. Huxley in classifying the varieties of the human race, exclusively for physical characters, had included under one head the people of New South Wales, of the Highlands of Central India, and of Ancient Egypt, all of whom he includes under the term Australoid. Now it is a remarkable coincidence that among these three far distant peoples the "throw-stick" was the weapon of the chase, and that examples do not occur in the intermediate countries. The pictures in the tombs of the kings at Thebes represent hunting scenes in which the curved sticks found at this day in India are extensively represented. The bomerang of Australia is precisely of the same form, but, being thinner and lighter, is so fitted to have a recoiling property.

SECTION E.—GEOGRAPHY

On the Orography of the Chain of the Great Atlas, by John Ball, F.R.S.

The representations of the chain of the Great Atlas given on the most modern maps show how very vague and incomplete our knowledge still is. They agree in very little beyond the fact that high mountains extend in a nearly direct line from the west coast, where they approach the Atlantic, near Agadir, in about 30° 30′ N. lat. for about 500 miles inland, where they subside at no great distance from the frontier of Algeria about the parallel of 33° 30′.

All but the most recent maps indicate a single range similar in general character to that of the Pyrenees, while in these we find represented two nearly parallel ranges at an average distance of sixty or seventy miles, of which the northernmost alone terminates near the Algerian frontier, its axis lying exactly in the line of the great shallow lakes, or chotts, that occupy a great part of the high plateau of southern Algeria, while the southern range, with some slight interruption, is continuous with the elevated zone that forms the northern limit of the Algerian Sahara. The details, however, as given in these recent maps, are strangely discordant, especially in regard to the region lying E. and N.E. from the city of Morocco, and connecting the main range with the mountains of North Morocco.

It is not surprising that such discrepancies should exist, when it is known that the best maps have been compiled with no better materials than the reports of natives, and that none but a very small portion of the entire region has ever been traversed by civilised men. In regard to Gerhard Rohlfs, one of the most remarkable of recent African travellers, it must be remembered that he was forced to maintain a rigid disguise, to associate constantly with natives, and to suit his movements to theirs. He was unable to make more than scanty and occasional notes, and was altogether debarred from the use of instruments. It is not surprising that, under such conditions, his contributions to the topography of a region never before visited by European traveller tend more to excite than to satisfy curiosity.

During the spring of last year the Sultan of Morocco, at the request of the British Minister, Sir John Drummond Hay, granted permission to Dr. Hooker, the eminent Director of the Royal Gardens at Kew, to explore the portion of the Great Atlas subject to the Imperial authority; and although the main object of the party, consisting of Dr. Hooker, Mr. Maw, and myself, was to investigate the Flora of the mountains, it might not unreasonably be expected that we should be able to make some considerable addition to existing geographical knowledge in regard to a region so little known.

Those who are best acquainted with Morocco will be least surprised to learn that in this respect the expedition has not borne abundant fruit. The obstacles which stood in the way were partly anticipated by us, but were in great measure insuperable.

The authority of the Sultan extends over but a small portion of the region included under the denomination Great Atlas. It is in fact limited to the northern declivity of the main chain, and only throughout the western part of this, for it extends to a distance at the utmost not more than 120 miles E. of the city of Morocco. The time at our disposal was too limited to enable us to explore even the limited field that was thrown open to use The cares and responsibilities attaching to his official duties prevented Dr. Hooker from prolonging his stay in and near the mountains beyond about three weeks, and the private engagements of Mr. Maw compelled him to separate from us and to return to England at a still earlier date. But by far the most serious obstacle which we encountered arose from the persistent though covert opposition of all the persons holding local authority, aggravated and not seldom stimulated by the chief of our escort, whose charge, as we had been assured, was to remove all impediments from our path.

But for the difficulties incessantly placed in our way, we should undoubtedly have attained several of the higher peaks, and could not fail to have learnt a good deal respecting the disposition of the greater masses and the direction of the main valleys in the territory which we could not actually traverse.

In point of fact we were able to make but two considerable ascents. On the first occasion, when we ascended the Tagherot Pass in a storm of snow and hail that completely intercepted all distant view, the cold was so severe that we willingly turned our faces from the storm when only Mr. Maw, the foremost of the party, had actually set his foot upon the summit, about 12,000 feet above the sea level. On the second occasion, after Mr. Maw had departed from us, we attained a conspicuous peak, called Djebel Tezah, about 11,500 feet in height, in a much lower part of the range than that previously visited. In addition to the very limited results of personal observation, we naturally availed ourselves of every promising opportunity for obtaining topographical information from natives. Much of the information obtained in this way appears to me utterly unreliable, especially when derived from persons holding local authority, but the particulars supplied by a very intelligent Jew residing in Morocco, so far as they rest on personal knowledge, deserve more confidence.

The following are the chief points as to which I think myself entitled to express an opinion, premising that as to some of them I may place undue confidence in my own personal conclusions:—

1. The portion of the Atlas chain that is seen from the city of Morocco is considerably higher than has generally been supposed. The higher summits approach nearly to the same elevation, and the majority of these approach very nearly, if they do not occasionally surpass, the level of 14,000 feet. Westward of the district of Glaoni, S.W. of the city of Morocco, the range subsides gradually as it approaches the coast.

2. There is a certain amount of tolerably good evidence tending to show that the interior part of the range extending from the upper valley of the Wed Tessaout to Eastern Morocco contains peaks of higher elevation than any seen by us.

3. The existence of an anti-Atlas or range parallel to the main chain, and enclosing on the south side the great valley of the Sous, was established by Rohlfs, if not by previous travellers; but we are probably the first who have looked across the wide intervening space and scanned the outline of the anti-Atlas. The portion seen by us at a distance of from 50 to 60 miles is far less bold in form than the main range. The utmost height of that portion can scarcely exceed 10,000 feet.

4. The map, compiled by Capt. Beaudouin, and published in Paris at the Depôt Général de la Guerre in 1848, which is decidedly the best that has hitherto appeared, is defective in representing the main chain as arising abruptly from the low country, scarcely indicating considerable lateral valleys. At the same time it should be remarked that the projecting ridges which divide these lateral valleys appear to be lower in comparison with the peaks of the main chain than is usual in other great mountain ranges.

5. There is a marked tendency to the formation of considerable valleys parallel to the main chain, and in such cases the remark made in the last paragraph does not apply. Some of the higher peaks, and amongst them those named Miltsin by the late Captain Washington, lie in ridges nearly parallel to the main chain.

6. It appears at least possible that the Anti-Atlas, if we may so denominate the range forming the southern boundary of the Sous Valley, is merely an example on a large scale of one of the parallel ridges just referred to, many examples of which are to be found in better known mountain regions.

7. The existence of two parallel chains so continuous as those represented in Gerhard Rohlfs' map appears to be open to reasonable doubt. In the absence of direct evidence, it appears at least equally probable that the conformation of the main chain may be best represented by a series of ridges slightly inclined to the axis of elevation of the entire mass.

8. The remarkable valley of the Beni mquald, laid down on Beaudouin's map as extending more than one hundred miles from S.E. to N.W. in a nearly direct line must be pronounced imaginary or based on false information. The details given in Rohlfs' "Reise durch Marokko," however incomplete, are manifestly inconsistent with the general plan of the mountain system laid down in that map.

SECTION G—MECHANICAL SCIENCE

Experiments on Surface Friction in Water, by W. Froude, F.R.S.

The object of investigation was to determine the laws which govern this force, in those especial relations under which it forms a portion of the resistance experienced by a ship when moving through the water at various speeds.

These are, (1) the relation of the force to the speed, (2) its relation to the quality of the surface, (3) its relation to the length of the surface along the line of motion. The necessity of investigating it under the latter of these relations, may not be at once obvious, it having been generally held that surface friction varies directly as the area surface, and will be the same for a given area whether it be long and narrow or short and broad. But a little reflection shows that this cannot be so, because the portion of surface that goes first in the line of motion, in experiencing resistance from the water, must reciprocally communicate to the water motion in the line in which itself is moving, and, consequently, the portion of surface which succeeds the first, must be rubbing, not against stationary water, but against water partially moving with it, and cannot experience as much resistance from it.

The experiments were performed with carefully made apparatus, which automatically recorded the resistance and the speed, and the errors and uncertainties of the results probably did not in any case exceed on the whole ½ per cent.

The surfaces used in the experiments were of yellow pine board, about $\frac{3}{16}$ of an inch thick, loaded at the edge with lead keels of the same thickness, fastened fair and flush with the board, the weights being such as to just neutralise the flotation, and hold the boards stably in a vertical plane. The head end (so to call it) of each board in turn was fastened into a tin sheath, or fine-edged cutwater, which formed a portion of the dynamometric apparatus, and which held the board resolutely in a vertical plane, with its length horizontal, and in the line of motion. The width of each board, including the lead keel, was 19 in., their lengths, including the cutwater, varied from 1 ft. to 50 ft. Great pains were taken, and successfully, to eliminate, and, indeed in effect to obliterate the resistance due to thickness.

It turned out that the effects of the three conditions under which the variations of the force were to be determined, could not be regarded as absolutely independent of each other, because certain variations in the quality of the surfaces were found to affect in some degree the relations of the force to the speed and to the length. The results may be approximately stated in brief, as follows:—

1. As regards the relation of resistance to speed. With the surface coated with shellac varnish, Hay's composition, or Peacock's composition, or tallow, the resistance varied very nearly as the power 1·83 of the speed; with the surface coated with tinfoil, very nearly as the power 2·05 of the speed; but the experiments with the tinfoil are not yet complete.

2. As regards the relation of resistance to quality of surface. With the surface coated with shellac varnish, Hay's composition, Peacock's composition, or tallow, the resistance differed extremely little; such variations as occurred scarcely exceeding 1 per cent., and being probably not greater than belonged to the small differences of smoothness in the laying on the composition.

With the surface coated with glue, and thus simulating the sliminess of a living fish, three successive experiments were tried at the same speed, so as to test the effect of the gradual growth of the slimy character. The first experiment showed an increase in resistance of 2 per cent., the last of 4 per cent., as compared with the shellac surface which the glue resembled before immersion, a proof that the attempted imitation of the fish's surface was not advantageous.

Comparing a tinfoiled surface with one coated with shellac, when the length is 1 ft. the resistance of the former is on the average only $\frac{2}{3}$ that of the latter, making the comparison with planes of 1·6 in length, the ratio is $\frac{3}{4}$, and with planes of 16 ft., more than $\frac{9}{10}$, instead of $\frac{2}{3}$; indeed, the total difference becomes progressively less as the planes compared are longer. At higher speeds also the difference tends to become less, in consequence of the higher power of the speed to which it is proportioned with the tinfoiled surface.

3. As regards the relation of resistance to length of surface. There plainly is a very considerable diminution of average resistance per square foot as the length of surface is increased, and this probably from the course already indicated, though the *rate* of diminution becomes gradually less as the surface becomes longer; there is, in fact, as great a diminution between 3 ft. and 4 ft. of length as between 30 and 50.

The following tabular statement gives the mean resistance per square foot on surfaces of from 1 foot to 50 feet in length, with speeds of from 200 to 800 feet per minute.

Length of plane. Feet.	Speed in feet per minute.				
	200	400	600	800	
1	0·048	·200	·460	·830	Resistance in pounds.
2	045	·188	·413	·725	
3	043	·183	·390	·673	
5	042	·166	·358	·608	
7	041	·154	·333	·563	
10	·040	·145	·312	·529	
20	·036	·131	·278	·473	
30	·035	·123	·264	·446	
50	·035	·117	·250	·417	

The table is applicable to a clean planed surface coated either with shellac varnish, Hay's or Peacock's composition, or tallow.

SCIENTIFIC SERIALS

Proceedings of the Newcastle-on-Tyne Chemical Society, 1871-72.—The number of meetings held by the society during the past year has been 6, the session only lasting from October to March with one meeting in each month. The October number opens with the address of the president, Mr. John Glover, which contains a short review of the papers read before the society during the previous year. The first communication is one which does not seem to have attracted the attention which it deserves; it is by Messrs Pattinson and Marreco "on the residual sulphur in purified coal gas," that is to say, the sulphur contained in coal gas after the removal of the sulphuretted hydrogen by means of oxide of iron or by lime. At various times violent fluctuations have taken place in the amount of the residual sulphur in the gas supplied to Newcastle, the quantity varying from 25 grains in the 100 cubic feet down to 4 grains. The authors believe that the explanation of this lies in the fact that the lime purifiers then in use were sometimes allowed to become "foul" or acted on to a great extent by the sulphuretted hydrogen, &c., contained in the crude gas. Contrary to what might have been expected, when some of the purifiers had become foul, the quantity of sulphur decreased rapidly, but that when the foul lime was replaced by clean lime, the quantity of sulphur immediately rose to some 25 grains per 100 cubic feet. An example is given of an occasion when clean lime was placed in the boxes, the amount of residual sulphur in gas was found to be 17·58 grains, and that as this lime became foul, the succeeding weekly tests gave 12·10 and 6 69 grains respectively. The method employed for the estimation of the sulphur is not stated; it is to be hoped, however, that one of the more recent methods has been adopted, and not the "Letheby sulphur test," which, as is well known, gives at the best, most inaccurate results. According to the authors it would seem a simple matter for our large gas companies to considerably reduce the quantity of sulphur present in gas, at comparatively no expense to themselves. The probable chemical action which appears to take place is the formation of a sulphocarbonate from the combined action of the carbon disulphide in the gas and the foul lime. Most of the remaining papers possess principally a technical interest. Amongst these we notice one "on the action in the black salt-pan and calcining furnace in alkali manufacture," by Mr. Moorhouse, and "on a new mechanical calcining-furnace" by Mr. Gibb, who also contributes a paper "on the formation of sodic carbonate by the action of carbonic anhydride on solutions of sodic sulphide." Dr. Lunge also contributes the abstracts of two papers by Fresenius, the first "on the quantitative estimation of sulphuretted hydrogen in presence of carbonic anhydride," and the second "on the best method of analysing artificial manures." This latter paper should be read by all interested in artificial manures, and will well repay a careful study. One feature to be noticed at the meetings of this society is that there is almost always a spirited discussion on the papers read, which shows very well the interest taken by the members in promoting the advance of chemistry, and also the welfare of their society.

Annali di Chimica applicata alla Medicina compilati del Dottor Giovanni Polli, No. 4, 1872. The contents of this number is of varied interest, the papers being grouped together under various heads, such as pharmacy, toxicology, therapeutics, &c., are very easy of reference. Under the head of pharmacy there are several papers given, which are abstracts of papers already published in other countries, such as "On the determination of the value of chloral hydrate," "On a new reaction of alcohol," by Berthelot, and others. Amongst them there is a note on the adulteration of essence of peppermint, which is now adulterated with the essential oil of copaiba; there is a second note on the adulteration of argentic nitrate with zincic nitrate, the amount of which, in some specimens, leaves no doubt of its having been introduced with fraudulent intention.

SOCIETIES AND ACADEMIES

PHILADELPHIA

Academy of Natural Sciences.—October 3, 1871.—Dr. Ruschenberger, president, in the chair.—Mr. Thomas Meehan referred to some observations made by him last spring before the Academy in regard to the office of bud scales and involucral bracts. The general impression was that they were formed for the purpose of protecting the tender parts beneath. At that time he exhibited branches of *Fraxinus excelsior* on which some of the buds were entirely naked, and others clothed with scales in the usual manner. They could scarcely be for protection in this instance, as both were equally hardy. He now had to exhibit an ear of corn which had been produced without the usual involucral bracts or husks, and yet was as perfect as if clothed in the usual way, showing that the husk was of not much importance as a protecting agent. An interesting point was that this ear had been formed on the end of a male panicle or tassel. It was not uncommon to find scattered grains of corn amongst male flowers, but a perfect ear like this he had never before seen. The ear was eight-rowed, and contained two hundred perfect grains. It was the variety known as "popcorn."

November 7.—Dr. Ruschenberger, president, in the chair.—Prof. Cope exhibited a specimen of a *Galeodes*, probably *G. pallipes* of Say, taken in the town of Denver, Colorado, by Dr. Gehrung. According to that gentleman, it was common in that place in houses, and was an enemy and destroyer of the *Cimex lectularius* (bedbug). In captivity, it showed a preference for them as food, and crushed them in its short cheles, preliminary to sucking their juices.—Mr. Thomas Meehan said that while travelling through a wood recently he was struck in the face by some seeds of *Hamamelis virginica*, the common Witch Hazel, with as much force as if they were spent shot from a gun. Not aware before that these capsules possessed any projecting power, he gathered a quantity in order to ascertain the cause of the projecting force, and the measure of its power. Laying the capsules on the floor, he found the seeds were thrown generally four or six feet, and in one instance as much as twelve feet away. The cause of this immense projecting power he found to be simply in the contraction of the horny albumen which surrounded the seed. The seeds were oval, and in a smooth bony envelope, and when the albumen had burst and expanded enough to get just beyond the middle where the seed narrowed again, the contraction of the albumen caused the seed to slip out with force, just as we would squeeze out a smooth tapering stone between the finger and thumb.

BOOKS RECEIVED

English.—Memoirs of the Geological Survey of England and Wales. Vol IV.—The Geology of the London Basin: T. McK Hughes (Whittaker).—Autumns on the Spey: A. C. Knox (Van Voorst).—A Handbook of British Birds: J. E. Harting (Van Voorst).—A Handbook of the Birds of Egypt: G. E. Shelley (Van Voorst).—Thoughts and Meditations on the Mysteries of Life Book I: John Frith (Trubner).—Physical Geography. S. P. J. Skertchley (Murphy).—The Sea-weed Collector. Shirley Hibberd (Groombridge and Sons).—Flora of Liverpool (the Liverpool Naturalists' Field Club).—Human Physiology: W. T. Pilter (J. Kempeter).

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Errata.—Vol. vi, p. 361, 1st col, line 17, for "meeting" read "heating," line 19, for "illuminated" read "eliminated."

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, SEPTEMBER 12, 1872

THE POTATO DISEASE

THERE seems little doubt that the present season will prove one of the most unfavourable within this generation as regards the yield of the fruits of the earth. The steady rise in the price of corn indicates a widely-spread fear that the harvest will turn out to be considerably below the average, both in quantity and quality. The crop of fruits of nearly every kind may be described as all but a complete failure. The potatoes are estimated as irredeemably bad, to the extent of three-fourths of the yield. Hops are in many parts scarcely worth the pulling. The grass and root-crops have alone benefited by the wet and ungenial summer. The cattle are, moreover, suffering from the foot-and-mouth disease on almost every farm in some counties, and we hear of the outbreak of rinderpest in Yorkshire.

In this dismal list the palm of failure must be given to the potato, with the exception, perhaps, of the apple crop, which has been destroyed by causes not affecting the health of the tree. It is generally admitted that the potato crop is, taken as a whole, the worst since 1845 or 1846. The cause of failure is the same—one, in fact, that has been more or less in existence ever since—the attack of a parasitic fungus, *Botrytis* or *Peronospora infestans*, peculiar to plants belonging to the same natural order as the potato, and unknown before 1845, or some say 1842. The mycelium of this fungus eats into and completely destroys the tissue of the leaf and stem, and when once its ravages have commenced it is almost impossible to arrest them. When the disease made its first great onslaught in 1845, innumerable remedies were suggested, some of which have again cropped up during the present season. Unfortunately, no sooner does one experimenter announce in the *Times* a mode which he has found effective of preventing or arresting the disease, than another grower replies that he has tried the same plan, and with him it has utterly failed. The exact mode of action of the parasite, and the operation of the proposed remedies, we intend glancing at on another occasion. It is satisfactory, at all events, that Dr. Hooker has given in public the weight of his authority in favour of the statement that the starch of the potato is not affected by the complaint, if only some economical mode can be found of separating it from the diseased ingredients. This is some alleviation of a calamity which, according to a statement in the *Times*, threatens the country with a loss of between twenty and thirty millions sterling.

The point to which we specially desire to call attention at the present time, is the enormous material loss which the country is now suffering, and has suffered year after year, from causes which are unquestionably within the range of scientific means to prevent, or at all events materially to alleviate. We are satisfied that we are within the mark when we say that the increased expenditure in most middle-class families within the past eight years, caused by the enhanced price of butchers' meat, milk, and potatoes, represents an income-tax of from a shilling to eighteen-pence in the pound. A portion of this rise is no doubt due to increased consumption, caused by the general prosperity of the country; but the greater part is owing to the prevalence of epidemic diseases in our crops and our herds. Surely Science can find no worthier object than in an earnest attempt to find a remedy for this. And yet what is English Science doing? It was cogently asked a few days since in the *Times*:—"What are we doing, or what have we done, to obviate the recurrence of a disease which is always impending? Probably all we can remember is that there is always a talk of the potato rot, and that some years it has been worse than others. We can only say that this is a disgraceful confession. There is no matter in which Science could interfere with more advantage; and we seem to have all the conditions of the subject under control." We fear that the rebuke here given to English Science is not wholly undeserved.

This brings us to the question which has so often been debated in these columns:—Where are we to find the proper individual or body to start and to carry on scientific investigations of this nature—in private individuals, in societies like the Agricultural or the Horticultural Society, or in the Government? Few will probably contend in favour of the first alternative. Individuals, no doubt, have been found, and will be found, to spend their lives and lavish their fortunes in investigations in which they have no or only a remote pecuniary interest. But it is surely unwise in the extreme to subject our national prosperity to the hazard of private generosity. The societies we have named, and others of a more local character, such as the Highland Society, have done eminent service in promoting sounder views and practices in agriculture and horticulture; but it is questionable whether inquiries of this nature are not beyond their scope, or whether any conclusions at which they might arrive would obtain the universal acceptance which would be desirable. We are, therefore, driven once more to the third alternative; and compelled to inquire whether we have not a right to look to the Government of the country to "interfere" in the matter, as Mr. Gladstone would term it, that is to institute and to promote an investigation into the Origin, Cause, and Remedies for the Potato Disease.

Little objection can be anticipated to the course we advocate on the ground of the money value at stake in the question. We are at the present time spending a large sum of money and employing the highest talent in the country in the settlement of a claim for a few millions; to save the country several times as much per annum cannot be objected to as a matter unworthy the attention of our rulers. And yet, because the one infliction will fall upon us in the form of an additional twopence to our Income-tax for a single year, the other in the form of a much heavier addition to our butchers' and greengrocers' bills for many years in succession, we are content in the latter case to grumble and bear it, without making any serious efforts to relieve ourselves from it. Science is often charged with being "unpractical;" indeed, in the minds of perhaps the majority of people there is a kind of hazy feeling of a necessary antagonism between what is scientific and what is practical. It is time for Science to redeem herself from this imputation, and no better opportunity could be found than in discovering a remedy for the Potato Disease. The questions which would present themselves for solution in such an inquiry are numerous. It would not be difficult to collect the facts; but they have never yet been tabulated or presented to the public in such a form that any conclusions can be drawn unquestionably from them. A competent authority on these subjects, the *Gardener's Chronicle*, recently remarked:—"Though for nearly a quarter of a century, more or less, cultivators have had to wince under the losses inflicted by the enemy, they have not yet learnt either the mode of invasion or the method of destruction." The Commission would have to inquire whether the disease is most prevalent on any particular soil; whether, as some assert, seed left in the ground through the winter enjoys comparative immunity as contrasted with that sown in the spring; whether seed introduced from a distance is safer than that grown in the neighbourhood; whether old varieties are dying out and new ones comparatively healthy; whether, if the disease can by any means be warded off till August 10, the crop is then comparatively safe, and very many others, on which every diversity of opinion exists at present? On one point almost all authorities are agreed, viz., that the disease generally makes its first decided appearance during thundery weather. The exceptional amount of electrical disturbance which extended over almost the whole country during July of the present year appears to have been most unfavourable to the potato crop; while a clergyman, writing from a district where thunderstorms are remarkably rare, in the portion of the county of Devon to the south of Dartmoor, averaging about six in twelve years, states that it is there almost free from disease.

It is worthy of note that an unusual development of the potato blight has been hitherto accompanied by murrain or epidemic diseases in animals and in other crops, and that a certain periodicity appears to be manifested. The present year has witnessed the most virulent outbreak since 1846; the worst of the intermediate years were nearly midway, from 1859 to 1861, showing an approximate recurring interval of about twelve years. A writer in the *Gardener's Chronicle* thus describes the crops in the latter year:—"My potatoes are in as bad a state as I ever remember to have seen them; my turnips are rapidly rotting, and many are filled with a semi-fluid offensive matter; the lettuces in various parts of the kitchen-garden are nearly all rotten; the roots are found generally diseased; the cabbages, savoys, and others of the *Brassica* are what gardeners term blind; the beans are spoiled by the black fly; the peas are all more or less blighted or mildewed; many of the plum and cherry trees are destroyed; I never witnessed anything more lamentable and disheartening." Other accounts agree in the main with this, at least as regards the potatoes in that year. Now, it is very remarkable that an interval of from eleven to twelve years coincides with the period of maximum sun-spots. The present time is near the maximum of sun-spots, so was 1860, so was 1848, the curve showing but little decline for one or two years on each side of the actual maximum. Now, if it can be shown that epidemics like the potato blight are connected with great cosmical cycles, an important step is gained. Physicists are now nearly of accord that a connection exists between the sun-spot period and the recurrence of electrical and other disturbances in the earth's atmosphere. It may be urged that such a conclusion as this would make cure hopeless, and paralyse, instead of stimulating, energy, by inducing a kind of hopeless fatalism. Not at all. An evil which cannot be avoided may, nevertheless, be greatly mitigated by scientific knowledge and skill. To be forewarned is to be forearmed, and a knowledge of the cause of a disease is already halfway towards its cure. If we were certain that in another twelve years we should be liable to a recurrence of the blight with unusual severity, the farmers might be persuaded to plant only so much as would be likely to yield seed for the next year, and that only under the most favourable circumstances, where comparative immunity might be predicted; and large breadths might be devoted to turnips, beet, or other root-crops which experience showed to be likely to yield good results, and which would furnish some substitute for the lost potato.

We have endeavoured to sketch out only a few of the questions which would present themselves for solution were we in earnest to institute a thorough scientific investigation of the cause and cure of the potato blight, and to point out that few subjects are more worthy the attention of a commercial and practical nation.

SHARPE & DRESSER'S BIRDS OF EUROPE

The Birds of Europe. By R. B. Sharpe and H. E. Dresser. Parts xi. and xii. (Published privately.)

THE completion of the first volume of this important work by the issue of Parts xi. and xii., affords the authors an opportunity of expressing their determination to continue the monthly issue with as much punctuality as is compatible with the fulness and accuracy at which they aim. This volume has occupied eighteen months in its publication; but as it contains 101 coloured plates and about 800 or 900 pages of letterpress of large quarto size, the wonder is rather that so near an approach to regularity has been attained in a work which is taking so much larger dimensions than was at first anticipated.

The present parts show no lack of the energy and care hitherto exhibited. In addition to the seventeen species figured and copiously described, we have three additional plates with eight figures of the Sparrow Hawk in various states of plumage, and two others with additional figures of the Ring Ouzel and the Rock Thrush. As an example of the great care bestowed by the authors in the accumulation and critical comparison of specimens from all parts of Europe, and from other quarters of the world where necessary, we may state that the present part discriminates between several birds that have hitherto been confounded, and thus adds two species to the list of European birds, and one to that of Britain. A fine Woodpecker (*Picus lilfordi*), found in Greece and Turkey, has been separated from *Picus leuconotus* which inhabits the more northern parts of Europe; while the British form of the Cole Tit (*Parus ater*) is found to be so constantly different from that which inhabits the Continent as to require a distinct specific name, and it has accordingly been called *Parus brittanicus*. To illustrate these minute specific differences the excellent plan is adopted of giving figures of the allied species on the same plate.

We cannot, however, equally praise the system of including American and other stragglers as European birds. It needlessly encumbers an already very bulky work, and leads to misconception, and it will also have the effect of making the book apparently imperfect whenever fresh stragglers reach our shores. Is it not absurd in a book of European birds to have seven pages devoted to the American Stint, with full details of its distribution over North America, and the statement that it has occurred "twice in Britain" as the sole justification for including it? Another seven pages is devoted to the American Hawk Owl on the strength of its occurrence four times in Britain. Such birds should be rigidly excluded from the body of the work, and only described in notes or an appendix when it is necessary to do so in order to avoid confusion with the allied European species.

It is a pity that the temporary paging of the letterpress to each species had not been altogether omitted, as it is of no use whatever, and occupies the prominent position which should have been left for the permanent paging. As the only means of remedying the evil, we would suggest that when the work is completed a series of numbers be printed in squares reaching to the highest number of pages in a volume, and be issued with the last part on gummed paper, so as to be cut out and fastened in the proper position over the temporary numbers.

The figures by Mr. Keulemans continue to be as spirited and lifelike as ever, and the authors devote the same attention as heretofore to giving the fullest and most reliable information obtainable. The work will thus satisfy the requirements both of the scientific naturalist and of the general reader and amateur. The former requires accurate descriptions and figures, careful measurements, and precise indications of distribution and habits. The latter wants to determine readily any bird he may meet with at home or on the Continent, with an intelligible and interesting account of its habits and distribution, and other topics of general interest. To both these classes of readers we can cordially recommend this book, and we believe that it is calculated at once to take a high position as a scientific work, and at the same time to popularise the delightful branch of natural history of which it treats. A. R. W.

GEOMETRICAL CONIC SECTIONS

Geometrical Conic Sections: an Elementary Treatise, in which the Conic Sections are defined as the Plane Sections of a Cone, and treated by the Method of Projections. By J. Stewart Jackson, M.A. (Macmillan and Co., 1872.)

The Geometry of Conics. Part I. By C. Taylor, M.A. (Deighton, Bell, and Co, 1872.)

MR. TAYLOR'S present work is by no means a second edition of his "Geometrical Conics" (1863). His object in this volume is a highly laudable one; from more than one quarter has recently come the complaint that the subject of geometrical conic sections is in an unsatisfactory state. The work under consideration is stated to be "the result of an attempt to reduce the chaos of geometrical conics to order, the subject having suffered not a little from desultory treatment." As in the earlier treatise, our author does not define the conics in question to be sections of a cone; and here he is at direct issue with Mr. Jackson:—"I am unable, despite his skilful advocacy, to acquiesce in the primary definition of conics from the solid."

This feud among writers on the conic sections is of old date. Simson, in his preface, stated that Wallis (1655) treated of these curves not as being sections of a solid (*nullâ coni habitâ ratione*), and that he was followed by De Witt and De la Hire. T. Newton, in his "Treatise" (1794), remarks that in the University of Cambridge the preference seems to have been given to that method which begins with a description of the curves *in plano;* whereas in the sister University, the Savilian professor, Abram Robertson, in a nearly contemporary work (1802), adopts the more ancient definition, and bases on it a very interesting exposition of the principal properties of conics. This latter method is the one we are inclined to prefer in a school book, though it is not that adopted by our standard writers, as Drew, Besant, and Taylor. Mr. Wilson, we were glad to see, has adopted it in his very handy though concise introduction to the study of these curves.

Putting on one side the numerous typographical errors in Mr. Jackson's work, and some few inelegancies, as we think, in the proofs—the results, doubtless, of too great haste in bringing it out—we have much pleasure in commending this volume, and hope that he will soon have an opportunity of removing these slight blemishes. If he has this opportunity, we are sure it will not be the result of luck ("in case this work should be so fortunate as to reach a second edition"), but the reward of genuine merit.

It is hardly needful to enter into any details respecting Mr. Taylor's mode of treatment of his subject. He is too well known and approved a writer upon it to need our commendation. Suffice it to say that many waifs and strays which he has previously communicated to the mathematical journals here find a fitting place. His leading principle, and that which tends so much to the clearness of his exposition, is that "Chord properties should take precedence of the Tangent properties, the latter being deduced from the former and not the former from the latter." A noteworthy feature is the prominence assigned to the treatment of a curve usually hurriedly passed over —the rectangular hyperbola. To this curve he devotes pp. 61—77. He very fully acknowledges his indebtedness to Prof. Wolstenholme's investigations of the properties of the curve. He has himself elsewhere (*Messenger of Mathematics,* vol. i. pp. 121—127) treated of the curve in question.

The book is a valuable contribution to the literature of this branch of pure geometry; and though it may not take the place of Besant's fuller treatise, as it does not go over the same extent of ground, yet it is worthy of being ranked side by side with it. We shall hail with pleasure the remaining part or parts of the work.

OUR BOOK SHELF

An Introduction to the Practical and Theoretical Study of Nautical Surveying. By J. K. Laughton, M.A. (London: Longmans and Co., 1872.)

THIS work is intended to supply a want that has long been felt by young officers of the navy who have not had an opportunity of gaining a knowledge of the methods of conducting a coast survey used on board vessels regularly employed on such work. We are accustomed to quote with pride the old saying, that wherever wood would float an English pendant was to be seen, and it is true at the present time, that every sea, well known or slightly known, is visited more or less frequently by our men-of-war. But unfortunately many of these places are roughly surveyed, the coast lines inaccurately laid down, and the positions of the principal dangers doubtful. Here much valuable work can be done by those not surveyors, who have time and are willing to take the opportunities often given them, to improve our knowledge by making a correct survey or verifying the charts of the coasts they may visit. To these Mr. Laughton's book will be exceedingly valuable, and though in his introductory chapter the author modestly says that he writes "not for the guidance of surveyors, but for those who know little or nothing at all about it," we are sure that many old surveyors will find their work easier from having the principles so clearly brought before them. The work is the more valuable in showing what can be done with the means at hand on board every vessel, though not especially equipped for the work. The second chapter describes the choice and measurement of the base line, and the methods for determining the exact latitude and longitude of the first position. The rules given are exceedingly clear and simple, and can be readily followed by anyone in the habit of using the sextant and artificial horizon. The hints on the choice and adjustment of the sextant will be found very useful to every navigator. Mr. Laughton's practical way of dealing with the subject is shown in his suggestion to get an old sextant and let it fall on the deck in order to acquire a thorough acquaintance with its mechanism in putting it to rights again. But it is a pity that the description of the instruments should be placed between the rules for the choice and measurement of the base line, indeed it would be better if this chapter were re-arranged. A great deal more might be said on the selection of objects for triangulation, this is a point on which the beginner encounters his greatest difficulty; some hints also as to the best way of noting angles would be useful, this want is supplied when levelling is treated of. Chapter III. is devoted to the construction of charts and various projections of the sphere, it is very clear and concise, and will prove valuable not only to the chart-maker, but also to the navigator, who will here gain a clearer knowledge of the plan on which the chart he uses is laid down, than is to be found in most books on navigation. We recommend, especially to young surveyors, the method advocated for graduation of charts according to the gnomonic projection, it is more comprehensive and certainly more mathematically correct than the methods usually employed, and we do not remember to have met it described in any other work. The part of Chapter IV. relating to the determination of positions is perplexing, and would with difficulty be understood by an inexperienced surveyor, without examples. The author might have well supplemented this part by showing graphically the way of protracting the angles, and finding the points of intersection. It is not clear why he has omitted from the description of instruments all mention of the protractor, an instrument as essential to the surveyor and navigator as his pair of compasses. The remarks in the last few pages on the "danger angle," or as it was called by old surveyors the "approximative angle," will be found very useful by all navigators, especially those who since the introduction of iron ships have experienced the difficulty and uncertainty of determining positions quickly by the compass when the course is changing rapidly. The running survey is ably treated, but we regret there is no illustration of the mode of surveying a harbour; this is a serious omission, but notwithstanding this, and the want of illustration of the choice of stations and selection of objects, we can recommend the book as the best out on the subject.

The Lepidopterist's Guide, intended for the Use of the Young Collector. By H. Guard Knaggs, M.D., F.L.S. Second Edition, illustrated. (London: Van Voorst)

THE want of such a "Guide" as the present was long felt by "young collectors" before the appearance of this book; but now this want is so well supplied that a second edition has not only been issued, but nearly exhausted, and we call attention to it with confidence, because, although eminently popular in its style and treatment, it is the work of a practical hand, and is as reliable as it is full and complete. In these days of cheap books it is marvellously cheap, and we are led to wonder how a scientific manual of upwards of 120 pages, closely printed, and copiously illustrated, can be produced for one shilling. Everything that the young collector is likely to require information upon will be found by a little searching, which a copious index would greatly facilitate, and which we hope to see appended to a future edition. We have no doubt this Guide will continue to receive the support it so well deserves. C.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Variation in Outline of American "Flint" Arrow-heads

I FIND, after a careful perusal of the sixteenth chapter of Mr. Evans's magnificent work on British Stone Implements—on javelin and arrow-heads—that he has considered the American forms far more uniform, less varied in outline than they really are. I am familiar with many collections made at distant parts of the Union, and have an immense assortment of my own collecting now in the Museum of the Peabody Academy, at Salem, Mass. In these several collections is every form that Mr. Evans has figured, and several that he has not mentioned.

On page 362 of Mr. Evans's work, I find the author asserting as a prevailing type, "that with a notch at the base on either side." While this form unquestionably is very common, it cannot be considered as the "prevailing" one, inasmuch as several distinct patterns equal it in numbers found, and some exceed it, as those that "have merely a central tang, with little or no attempt at barbs." A second statement of Mr. Evans strikes me as very remarkable. He says, "the leaf-shaped form is very rare." This is a very great error. In any locality where arrow-points are to be found at all, they are always to be met with; and I have gathered scores of them that for symmetry far excel any of the figures given by Mr. Evans.

Nor can I admit the correctness of Mr. Evans's assertion concerning arrow-points, that "for the most part the chipping is but rough, as the material, which is usually chert, hornstone, or even quartz, does not readily lend itself to fine work." I believe no arrow-points have ever been discovered that can exceed, in beauty of finish, those I have myself gathered from the fields and meadows of central New Jersey. And I am the more surprised at Mr. Evans's remarks, inasmuch as the specimens I have found, that have been wrought from white or rose quartz, are remarkable for the smooth surfaces and sharp edges they present. So, too, of our jasper and hornstone specimens. Remarkably diversified in form, uniformly well finished, they strike the beholder with astonishment, when the "intractability" of the material is recognised. I have seen but few specimens of arrow-points not found in the United States; but judging from them and the illustrations of Mr. Evans's work, I unhesitatingly assert, that although we have no convenient flint in New Jersey or near it—Prof. T. A. Conrad has discovered true flint and chalk in Colorado—we have, in the jasper, chert, hornstone, quartz, and slate arrow-heads, examples of such weapons, as are in no wise inferior to those of Europe in beauty of finish, or less diversified in the various so-called "types."

Mr. Evans seems to have based most of his impressions concerning our antiquities upon Schoolcroft's ponderous tomes, which present little or nothing of value, of our "antiquities," whatever may be their reliability as concerning the "Indians," at the time when the volumes were compiled.

In conclusion, I will give a list of the arrow-points gathered on Wednesday, July 31, during the course of a three hours' search, and over fields that have yielded hundreds during the past and present summers.

Nos 1—5, genuine leaf-shaped arrow-heads; four of black jasper or hornstone; all symmetrical, perfect. The largest specimen measures five-eighths of an inch in width at widest part, which is near the base, which is a beautifully wrought half-circle. The length is one inch and eleven-sixteenths. The other four specimens are somewhat smaller; one a little broader; and none can be considered as rough or badly-made examples.

Nos. 6—8 are what may be called triangular arrow-points, but are different from the ordinary examples of that pattern, in that they have very concave sides and base, and a rounded rather than pointed tip. Nothing in any way similar is figured by Mr. Evans, nor was I acquainted with this pattern when I wrote of our arrow-heads in the April No. of the *American Naturalist.* These arrow-points vary little in length and width; being about an inch and a quarter to an inch in length, by about one in width. The main portion of the specimen and the projecting barbs are nearly the same length, and have all the same degree of finish. One specimen is of brown jasper, one of hornstone, and the third, we suppose, is what Mr. Evans means by "chert."

Nos. 9—14 are triangular arrow-points, with straight or very slightly convex sides, and well-marked concave bases. None measure over an inch in length, by three-quarters of an inch in width. One specimen is of "chert," three of hornstone, one of green, and one of chocolate jasper. The finish of all is good, and of two in particular very fine. The chocolate-coloured jasper example has a row of uniformly shaped notches or serrations, throughout the greater part of one side.

Nos. 15 and 16 are triangular arrows, both of which have straight bases, and one with straight sides, the other with very convex sides. Both are well finished, and the smaller quartz specimen with the convex sides is as smooth, well-edged and pointed, as though it had been "rubbed" down.

No. 17 is a yellowish "flint-like" stone, chipped into an arrow-point of the triangular pattern, but with a notch in the base, and also at each side. This form I have figured in the *American Naturalist.* It is but sparingly met with, and is there called a stemmed arrow-head, or one with a projecting base, which I think now is scarcely correct; the notches at the sides and base give it a "stemmed" appearance only.

Nos. 18—20 are three fine specimens, having projecting stems, which are narrower than the body of the specimens, and are not notched, but taper to a blunt point. Mr. Evans's figures 300 and 301 and the base of 302 well represent the specimens now lying before me.

Nos. 21—25 are stemmed arrow-points, with notches, that is, the "base" projects beyond the base of the body of the specimen, which gives "barbs" to the weapon—a style not given by Mr. Evans; or the base or "tang" is narrower than the body of the specimen, and flaring at its termination, produces the notches, by which the shaft was attached. A poor example of this pattern is Mr. Evans's figure 303. One of these "tanged" arrow-points has a projecting "tip," like that figured by Mr. Evans, as a peculiar feature of his leaf-shaped arrow-head, figure 283.

Nos. 26—33 are plain "tanged" arrow-heads, very similar to Mr. Evans's figure 304, which he refers to as "a magnificent specimen." The only marked difference in the little series before me and the illustration mentioned is that the tangs are all broader and a little shorter. The specimens themselves are not much smaller. They are of slate, jasper, hornstone, and "chert."

No. 34 is a lozenge-shaped arrow-head, very similar to Mr. Evans's figure 277, but is somewhat smaller. It varies from everything I have found as yet, and is a reproduction of those lozenge-shaped specimens, only of handsomer outline, that Mr. Evans has found on the Yorkshire wolds. The finding of this specimen lessens the number of forms found in Europe, that have not occurred here.

Besides these thirty-four specimens, which are all perfect, I gathered innumerable "chips" and broken specimens, some of them being of patterns not enumerated in my list. I have here briefly referred to nine distinct patterns, numbers of which, save two, were found during one day's hunting; and the result in numbers and varieties was nothing more than "a good average." I cannot therefore admit any one form to be a "prevailing" type, and the idea of inferior finish and of general elegance of appearance, is, we respectfully assert, a very great mistake. CHAS. C. ABBOTT

Trenton, New Jersey, Aug. 5

Millions of Millions

THE fact that I have myself slipped into an error by writing eleven noughts instead of ten in setting out a number expressed by a row of sixteen integers, only serves to confirm my former remark that millions of millions are awkward numbers to deal with, and that it will be well to avoid them by making use of the very simple rule-of-three sum indicated at the end of my list of errata to Professor Mayer's paper.

A COWPER RANYARD

Fertilisation by Moths

IT has recently been suggested to me that the following note on the readiness with which moths wander, and their efficiency in fertilising orchids, is worth publication; I therefore forward it to you.

In the summer of 1869 I caught here on an island of less than six acres, in the middle of Derwentwater, twenty specimens of the common 'shark' moth (*Cucullia umbratica*); of these, seven had the pollinia of the butterfly orchis (*Habenaria chlorantha*) sticking to their eyes. I know for certain that there were no plants of *H. chlorantha* growing on the island, and all the moths must have come from places separated from the island by half a mile of water. W. C. MARSHALL

Derwent Island, Sep. 9

Origin of Insects

MR. J. J. MURPHY, in writing "that it is true that the water-beetles are wingless" (NATURE, No. 140, p. 373), has surely made a *lapsus calami*, since many waterbeetles are not only winged but use their wings. Other orders furnish examples of an aquatic winged insect fauna. The hemipterous genera *Notonecta*, *Corixa*, &c., are well-winged, and use their wings (especially *Notonecta*). *Corixa* affords an example of the elytra (*i.e.* the front wings) assisting in respiration, but probably not in the way that Mr. Murphy means. At the base of the anterior margin of the elytron there is a channel which retains a supply of air. Of course everybody knows the use of the elytra in *Dytiscus* to catch and retain air.

The Lepidopterous genus *Acentropus* affords another instance. The perfect winged insects frequently descend into the water. The females are sometimes winged and sometimes apterous, and the winged male has been seen entering the water in pursuit of —it is supposed—the apterous virgin female.

I think that it is possible that these apterous females exhibit the same kind of "parthenogenesis" as occurs in the *Psychidæ*. It would be well if those observers who have an opportunity would try to ascertain if parthenogenesis ever occurs in *Acentropus.* F. BUCHANAN WHITE

Solar Spots

BY an observation of the sun this morning at 11h. 25m., I find that several parts of his surface are in a disturbed condition, and that several largish spots (*maculæ*), surrounded with penumbræ, are visible. In the north-west quadrant of the disc, near the west limb, there was a group seen, in which two rather conspicuous spots were situated, and below these, in the southern hemisphere, there were three others of somewhat considerable dimensions. In the same hemisphere there was an irregular train of spots of various forms and sizes, extending almost to the margin of the south-eastern part of the disc. In the north-east quadrant I could discern no spots at all. Light clouds were continually passing over the sun during the time of observation. I used a 4-inch metallic-mirror reflector, with the aperture contracted to three inches. WILLIAM F. DENNING

Bristol, Sept. 8

Correlation of Colour and Music

A VERY brilliant rainbow, which occurred on the evening of September 6th, recalled to my mind the note on the correlation of colour and music by Mr. W. F. Barrett, which appeared in

NATURE of January 13, 1870, and the subsequent correspondence. The violet of the primary bow passed into red at its concave edge, and within this violet-red arc there was a faint appearance of prismatic colours, blue or green (and I think yellow), and then a distinct red arc, and within this again yet another very faint red arc. Between these last two the other colours of the spectrum, if they existed, were too faint to be seen; but the impression given by a *coup d'œil* was that of three complete series of colours. There was nothing beyond the red on the outside of the primary bow, except, of course, the secondary bow, at some distance.

This is the phenomenon alluded to by Mr. Justice Grove, in his letter to NATURE of January 20, 1870, in which he queries whether these colours are repetitions of the spectrum, such as are suggested by Sir John Herschel. Your correspondent, Mr. C. J. Munro (NATURE, February 3, 1870), appears to regard them as analogous to "Newton's Rings." I should much like to see the point more fully elucidated. Is it established that under no circumstances can the spectroscope show visible rays beyond the violet?

GEORGE C. THOMPSON

Cardiff, Sept. 8

Cat's Teeth

I HAVE in my collection the skull of a cat, which has the peculiarity of possessing an extra molar tooth on the left maxilla; this tooth is tricuspid, and is situated between the last premolar and the carnassial tooth, on their interior side, so that it does not disturb their normal position. Will some of your readers inform me whether this is not very unusual? and whether from its position it does not overthrow Professor Owen's theory, that the two premolars are respectively third and fourth?

R. LYDEKKER

Harpenden, Sept. 2

DANISH EXPEDITION TO THE FAROES

THE United Steamers Company (*forenede Dampskibselskab*) in Copenhagen, having got a grant from the Government for the exploration of the Faroe coal-fields, is about to send an expedition to these islands, for the purpose of scientifically examining into the extent of the coal-fields in the north of Suderoe, and discovering in what manner coals may be best transported from that island to Copenhagen.

Besides having in view commercial purposes, the expedition will be accompanied by men of science, who will investigate the natural history of these little-known islands. The Government has asked Prof. Johnstrup to visit the different coal-fields on the southern island, and to investigate the geological features. The managers of the steam company, represented by Consul Koch, have also kindly allowed the writer of these lines to accompany the expedition for zoological purposes.

The geological features of the islands are best known from Forchhammer's researches, published in the "Transactions of the Danish Society of Sciences" (1828). The rocks of the Faroes are for the greatest part of volcanic origin, dolerite-porphyry being found in large masses in all the islands. Coal sediments are only to be seen in the south (Süderoe), and in the little islands of Myggenäs and Tindholm. To what formation these beds belong has not been cleared up, as fossils have hitherto not been discovered. But as the coal-fields of Iceland and Greenland, in which fossil plants have been found, belong to the miocene-tertiary period, it is very probable that those of the Faroes belong to the same formation. The researches which now are to be made by Prof. Johnstrup and his assistant, Cand. Geisler, will, we hope, throw further light upon the nature of these deposits.

The fauna of the islands, as far as the vertebrates are considered, was already tolerably well known at the beginning of this century, as may be seen from Landt's "Beskrivelse over Faeröerne," published in 1800. The only wild mammals inhabiting the interior of the islands are a few species of the genus *Mus*, which follow man's steps wherever he goes. But the shores of the Faroes are visited by a large number of *Pinnipedia* and *Cetacea*, from the capture of which the inhabitants have every year a good profit. The birds—those inhabiting the rocks of Store and Lille Dimon, as well as those of some of the other islands—have been made known by Graba, and, so far as they also occur in Iceland, by Faber. Later publications, especially by Swedish authors, are well known to have thrown much light on the natural history of these inhabitants of the north. Reptilia and Amphibia do not occur at all in the Faroes; but fishes of various species come to the shores and ascend the rivers in considerable numbers. They have been collected with great zeal by Sysselman Müller, of Torshavu, who has sent a list and specimens of all the species known to him to the zoological museums of Copenhagen. The lower animals are less known; we have lists of echinoderms and molluscs by Lütken and Morch, and we know something about the worms from the investigations made there by Prof. Oscar Schmidt, who for a short time visited the Faroes. The writer of these lines hopes to gather further information about the lower animals by dredging on the shores of the islands; and, while collecting the fishes for the Munich Museum, he will continue his researches into the natural history of their parasites.

The expedition will leave Copenhagen early in September, and, when returning from the Faroes, may perhaps pay a visit to a Scottish port.

RUD. V. WILLEMOES-SUHM

Copenhagen, Sept. 4

NATURAL HISTORY EDUCATION AT HARVARD UNIVERSITY

WE reprint the following interesting article on the scientific instruction given in Harvard University from the pages of the *American Naturalist*:—

The changes which have been made in the departments of Natural History at Cambridge within the last two years have been very great, greater perhaps than in any other school within the same time. As there are many persons of both sexes who are seeking opportunities for study such as the University now offers, we give a sketch of the plans of education in the different schools as far as they concern the student of natural history. There are five schools in the University where natural history is taught: the College, the Museum of Comparative Zoology, the Botanic Garden, the Scientific School, and the Bussey Institution. Let us trace in a general way the course of a student in these departments.

The student who enters the college, to-day is no longer compelled to follow the one uniform road over which the boy of twenty years past had to go; after his first or freshman year, he may begin to turn himself into the paths of natural science. At the commencement of his second year he may begin his studies by courses which lay the foundations of a knowledge of chemistry, taught in the laboratory; of physical geography, geology, and meteorology, taught by text-books, lectures, and excursions in the field. The time allowed for these studies during the year is estimated at twelve hours per week. It is expected that the student will in this year lay the foundations for the work he may wish to do during the following years, by getting that general idea of the physics of the globe which forms the necessary basis for the work of the naturalist in any department of labour.

With the junior year the studies of a strictly biological character begin. One course includes the elements of comparative zoology, with elementary teaching in microscopy, another the elements of botany, a third the elements of comparative anatomy. The principle on which the teaching of zoology is based is that the student should at the very beginning be put into the position of an investigator. With this object in view the student is at first required to do all his work upon natural objects. Beginning with the solid part of a Fungia, or some other object of equal simplicity, the student is then required to draw and describe the specimen, aided only by such questions and suggestions as may be necessary to get him over the worst obstacles. As soon as he has done the little he can do in the way of close observation, he is given a Manacena or Agaricea, which he proceeds to compare with the Fungia, and so making at least diagrammatic drawings with a dozen other specimens of Polyps, Halcynoid and Actinoid. Thus the student gets some idea of the general relations which exist among the members of that group. When, say, in thirty hours of labour he has got through this work, a few lectures serve to supplement and connect the knowledge he has obtained from the personal study of the dry parts, illustrated by a sufficient series of alcoholic preparations, and helped out by such individual teaching as can be given without weakening the habit of self-reliance. In this way he goes through group after group, until, from a study of about one hundred species, he has gotten a general idea of the organic forms above the Protozoa. In this stage of the student's work care is taken to avoid the use of diagrams; this avoidance being dictated by the conviction that the student remembers the diagram and not the object. During this year botany is also taught, with the same object and by much the same method. In connection with the zoological instruction the students are taught the elements of microscopy, the development of the subject being left to the next year.

The second-year courses are advanced zoology, palæontology, historical geology, geography, and advanced botany. The first two have one common feature; three lectures or readings are given each week to the discussion of the history of zoology and palæontology, with special reference to modern opinions concerning the relations of animals. An effort is made to acquaint the students with the character of the greater works in the science, by giving them constant opportunities for consulting them in their studies, and by showing them the methods of the masters in the several departments. Besides this, each student is required to pursue some special line of work. In the choice of subject the largest liberty is allowed, but the student is, however, recommended during a half-year to study advanced microscopy; in this work the aid of an instructor is given for four hours a week. In this four months he should acquire a sufficient knowledge of the practical management of the instrument in all ordinary investigations. The laboratory is well supplied with instruments of instruction in this branch of work.

Besides the course in the history of the science, the student who takes the elective in palæontology is required to traverse the ground covered in that part of "Dana's Manual" which is entitled historical geology, acquainting himself in a practical way with the most important characteristic fossils of the several periods.

The greatest value in this work is set upon the keeping of full and accurate note-books in both the last described courses. The rank of the student turns upon the condition of his note-books as much as upon the quarterly examinations which he is required to pass.

Those students who desire to contend for honours at the graduation in zoology or in palæontology are required to have taken, besides their junior election in natural history, one election in physical science, and at least three natural history elections in the senior year, in all of which they must have attained excellence. They are moreover required to write an acceptable thesis, which must contain an original discussion of some question in biological science. Hereafter the junior electives will consist of a course in anatomy and physiology, one in zoology, and one in botany; and the students in this as well as in the last year will be allowed to substitute for the themes required in other branches theses upon scientific subjects prepared under the direction of their instructor.

The natural history education of the scientific school has undergone a great change within a year; hitherto the students have worked with the professors of the several departments, giving their whole time to any speciality which they might select. This plan, admirably suited as it was to the needs of the trained student who had fitted himself in other schools for the work of a special department, was not adapted to the needs of those to whom this teaching was to fill the whole office of higher education. With the introduction of the doctor's degree into the plan of the school, it became necessary to make a change which has long been desirable, by fixing a definite scheme of general scientific instruction in place of the imperfect system which had hitherto prevailed. A three years' course has been arranged which secures to the student a broad view over the whole field of science, and the advantage which comes from a knowledge of the methods of research in use in its several branches. It gives to those persons who may not have the desire or the means to go through a regular college course a systematic training which will occupy their full time for three years, and give the best results of culture which can be attained in any scientific course. Students who can pass the required examinations are admitted to the degree of bachelor of science. Graduates of colleges where science is taught in an effective way should be able to enter this course in advanced standing. Students of the college, graduating with honours in the departments of natural history, should be able to obtain the degree in this course in a year of study. The student is trained in the important art of expressing himself clearly on the matters which he is studying, by requiring him to keep carefully planned note-books; and he is urged to the preparation of theses which may embody the results of some research. Ample opportunities are given for the prosecution of studies in the field, by excursions during term time and vacation, led by the instructors in zoology, botany, and geology.

After two years' further study, one of which must be spent in Cambridge, the student may apply for the degree of doctor of science, which is given after an examination conducted by a committee appointed by the Academic Council of the University.

The study done, the preparation for the degree must be in some special department, when the student will generally become the private pupil of some one professor. The degree will be a certificate of capacity as an investigator or teacher in the science which the student has made his specialty.

The resources of the University for teaching science are, it is believed, not only unrivalled in this country, but unsurpassed in Europe. The scientific departments have a list of twenty-four instructors, and the material resources which they afford have cost in the aggregate over a million and a half of dollars. There are six museums in the University—the Museum of Comparative Zoology, the Botanical Museum, the Museum of Comparative Anatomy, the Museum of Morbid Anatomy, the Museum of Mineralogy, and that of Ethnology. These collections are unsurpassed by those of any educational institution in this country; and, taken together, they furnish an efficient basis for the acquisition of the wide ranging knowledge on which a scientific career must be based. The opportunities for contact and intercourse in scientific societies are excellent. There is a working society of natural history in the University, and the Boston Society of Natural History, one of the largest and most efficient of the American institutions of this nature, is also open to all students of the science.

MELTING AND REGELATION OF ICE

IN NATURE of January 4th of this year, there is a most interesting account of some experiments on melting and regelation of ice by Mr. James T. Bottomley. These experiments of Mr. Bottomley's suggested the possibility of passing large bodies through ice in the same way as he caused the wires to pass. I accordingly placed a sixpence on a block of ice, and applied pressure to it by means of a fine steel wire about one-sixteenth of an inch in diameter. On examining the block of ice sometime afterwards, I found the sixpence had passed into the centre of the block, and that the space through which it had passed, except the small part occupied by the steel wire, was again solid ice. I tried the same experiment with a shilling, and found that it also easily passed through the ice, the experiment was then repeated with a half-crown with the same result. I did not attempt anything larger, but have no doubt much larger discs of metal might be made to pass through ice if sufficient pressure were applied. The ice in the parts of the blocks through which the coins had passed did not look very solid, but was rather full of air-bubbles; on breaking the block, however, it did not seem much weaker than the rest of the ice. Another form of the experiment was then made, a block of ice was supported on two boards placed near each other. A loop of fine wire was passed over the ice, and hung down between the two boards and a weight attached to it, as in Mr. Bottomley's experiments, pieces of wood were placed so as to stop the wire when it had passed half way though the ice. After the wire had passed into the centre of the block, the weight was removed, the wire cut, and a disc of metal half an inch in diameter was attached to one end of the wire, and a weight to the other end. In this manner the disc was drawn through the ice, leaving apparently perfect solid ice behind. The path of the disc could only be traced by its slightly cloudy appearance, it looked as if the few air-bubbles passed through by the disc had been broken up into a great number of small ones. On breaking the ice afterwards it seemed quite as strong where the disc had passed as elsewhere.

The explanation of these experiments is of course the same as for the experiments with the wires; Professor James Thomson showed that the freezing point of water is lowered by pressure, and also that ice has a tendency to melt, when forces are applied which tend to change its form. So that the ice under the coins has a tendency to melt, and has its freezing point lowered by the pressure. The under side of the coin will thus have a lower temperature than the upper; there will therefore be a transference of heat from the upper to the under side of the coin, this heat melts the ice under the coin, the water so formed passes round the edges of the coin to the upper side. This water being at a slightly lower temperature than the freezing point at ordinary pressure, a very small proportion of it will freeze and raise the temperature of the rest to the freezing point. The water arrived at the upper side of the coin, the coin being at a temperature a little below the freezing point, the water will be frozen, giving out its latent heat, which will pass through the coin and melt an equal quantity of ice on the under side, this having absorbed its latent heat of liquefaction will in turn pass to the upper side, and will there be converted into ice, giving out its latent heat to melt another quantity, and so on.

A slightly different form of the experiment was then made, a small metal cup was filled with water and laid on a piece of ice, and a heavy weight placed on the cup. After some time the water in the cup was frozen. The freezing point of the ice under the cup being, owing to the pressure, lower than that of the water in the cup, the water in the cup parted with its heat to the ice outside. A quantity of ice outside the cup was thus melted equal to the quantity of ice formed in the cup.

At first sight these experiments might seem to have an important bearing on the motion of glaciers. It might be thought, that if large bodies flowed thus easily through ice, why should not ice flow easily in its channel? But when we consider the circumstances, we find they are not so similar as might at first appear. When a body flows in this way through ice, there is not only a displacement of matter but also a displacement of heat, and the displacement of the matter cannot take place till there has been a displacement of the heat. In the preceding experiments, circumstances were most favourable for both displacements taking place quickly. The heat easily flowed through the very small thickness of the good conducting silver discs, and the water had only to flow from the one face to the other round the edges of the coins, whereas in glaciers, the ice and the rocks over which it moves are bad conductors of heat, and the distance to which the heat has to be conducted is so much greater than in the above experiments, that the exchange of heat can take place but very slowly; and when we further remember the very small difference of temperature between the freezing point of ice under pressure and not under pressure—if the lowering of the freezing point is the result of hydrostatic pressure alone, a pressure of one hundred atmospheres not lowering the temperature one degree centigrade—we can easily see that there will not be sufficient difference in temperature between the different parts of the glacier to cause the heat to flow quickly from one part to another, through such bad conductors.

In the explanation given of the passage of the coins through the ice, it has been assumed that the passage depends on the exchange of heat from the freezing ice on the one side of the coin to the melting ice on the other side. If this explanation is correct, then, if the coins had been non-conductors of heat, they would not have passed through the ice. The test was put. A shilling was placed on a block of ice, and over it a disc of a non-conductor (india-rubber), the same size as the shilling and over that another shilling; a weight of 90lbs. was applied by means of a small steel rod. After four hours it was found that the shillings had only sunk about an eighth of an inch into the ice, most of the heat to sink it this short distance being, in all probability, got by radiation from surrounding objects; as other two shillings and non-conducting disc placed on a block of ice and similarly situated, but not under pressure, had sunk to nearly the same depth.

There is another point in these experiments in their relation to glacier motion, which requires to be noticed. In all the experiments referred to, ice at the melting point was used. Sir William Thomson showed that the freezing-point of water was lowered 0°·13 C. by a pressure of 16·8 atmospheres. We should therefore expect that, if we lowered the temperature of the ice by half a degree or a degree below the freezing-point, a much greater pressure would be required to cause the coins to pass through the ice. In order to test this, a block of ice was surrounded with ice, salt, and water. After it was cooled about a degree below the freezing-point, a shilling was placed on the block of ice, and a pressure of 90 lbs. applied. On examining it three and a half hours afterwards, the shilling was found not to have entered in the slightest degree into the ice. The freezing mixture was then removed, and within an hour the shilling had passed some distance into the ice. It would therefore appear, considering the enormous resistance offered by ice at a temperature of even one degree below the freezing-point to change of state, that the motion of glaciers at the higher parts, where their temperature is below the freezing-point, is, in all probability, not caused by the melting and regelation of the ice in the same manner as in the experiments.

Darroch, Falkirk JOHN AITKEN

A GIGANTIC "PLEASURING GROUND": THE YELLOWSTONE NATIONAL PARK OF THE UNITED STATES

THE Americans do their pleasuring as gigantically as they do everything else. This is the first and strongest impression made upon one by an Act of Congress of March 1872, to which we alluded in a recent number of NATURE, enacting that a district about half the size of Wales, and 1,000 square miles larger than the largest Swiss Canton, be "dedicated and set apart as a public park or pleasuring ground for the benefit and enjoyment of the people." It is forbidden that anyone shall hereafter settle upon or enclose any part of the immense area thus set apart, and only such buildings can be erected upon it as the Secretary of the Interior, who has the exclusive control of this "park" (what an inadequate name!), may deem conducive to the accommodation and comfort of the visitors.

The estimated extent of the district thus set apart is 3,575 square miles, and coincides to a large extent with the area contained between the 110th and 111th degree of W. long., and the 44th and 45th parallels of N. lat. The southern boundary, however, is about eight miles farther north than and parallel with the 44th degree, and about seven miles westward of and parallel with the 111th degree, the whole district forming very nearly a square, and looking on the map like a huge slice out of one of the most mountainous parts of Switzerland; one of the heights, Mount Washburne, is 10,575 feet above sea level. Even the lowest part toward the south, containing the basin of Yellowstone Lake (330 square miles), "one of the most beautiful lakes of the world," is about 7,000 feet above the sea. Besides the huge mountains that form the most prominent features of this pleasure ground, the beautiful lake just mentioned, and a large part of the upper courses of the picturesque Yellowstone and other rivers, the district embraces some of the most remarkable natural phenomena that are to be seen in any part of the world—wonderful falls, multitudes of hot springs, steam springs, mud geysers, mud puffs, water geysers, some of them rising to a height of 200 feet, and other objects of interest. This whole region was in comparatively modern geological times the scene of wonderful volcanic activity. The hot springs, geysers, &c., represent the last stages, the vents or escape pipes, of these remarkable volcanic manifestations. All these hot springs are adorned with decorations more beautiful than human art ever conceived, and which have required thousands of years for the cunning hand of Nature to form. The geysers of Iceland sink into insignificance in comparison with the hot springs of the Yellowstone and Fire-hole basin. No portion of this tract could ever be made available for mining or agricultural purposes. The mountains that wall it in on every side form one of the most remarkable watersheds on the continent. From whatever point of view we survey this remarkable region, it is unsurpassed in interest, and the Act is one that should cause universal satisfaction throughout the States. "This noble deed," Mr. F. V. Hayden truly says, "may be regarded as a tribute from our legislators to science, and the gratitude of the nation, and of men of science in all parts of the world, is due to them for this munificent donation."

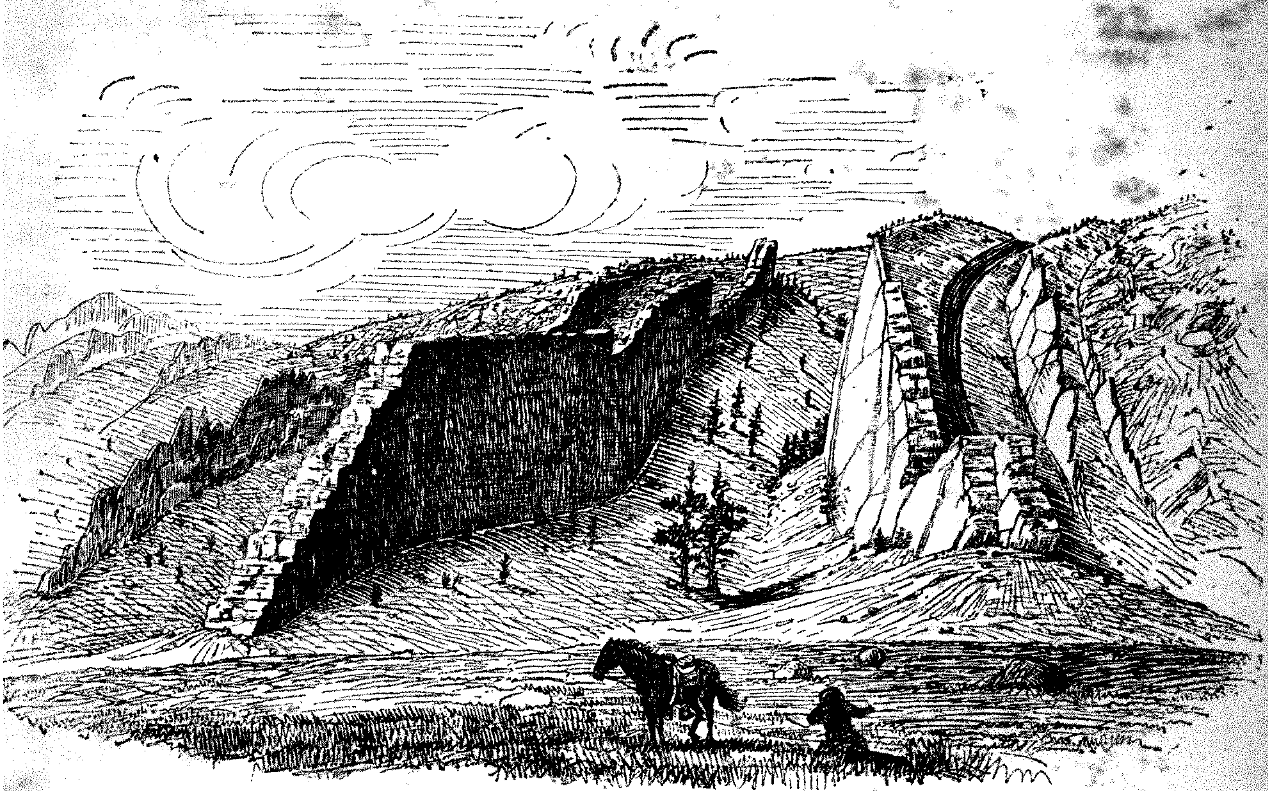

FIG. 1.—Natural Walls in Yellowstone Park.

Several exploring parties have lately visited the district, and from an account of one of these, under Mr. F. V. Hayden, U.S. Geologist, to whose courtesy we are indebted for the accompanying woodcuts, we condense the following description of some of the most remarkable phenomena to be witnessed.

Nine-tenths of the area is covered with volcanic material in some form. The base rocks are the usual metamorphic granitoid series of the country, with basalts and basaltic conglomerates in every variety. The sedimentary rocks belong to the Carboniferous, Jurassic, Cretaceous, and Tertiary ages. The Triassic is probably wanting. The sedimentary rocks occur in patches, covering very restricted areas, yet presenting evidence that, up to the period of the Eocene Tertiary inclusive, they were extended uninterruptedly over the whole country. In the Yellowstone valley, as in the valleys of all the streams of the West, there is a chain of lake basins that must have existed during the Pliocene period. There was a continuous chain of these lakes of greater or less size to the source of the river; thence it expanded into an immense double lake, of which only a remnant, Yellowstone Lake, now remains. This lake was once much larger than at present, and it was partially connected with another lake about 30 miles long and 20 wide, which terminated at the Grand Cañon, at the upper falls of the Yellowstone.

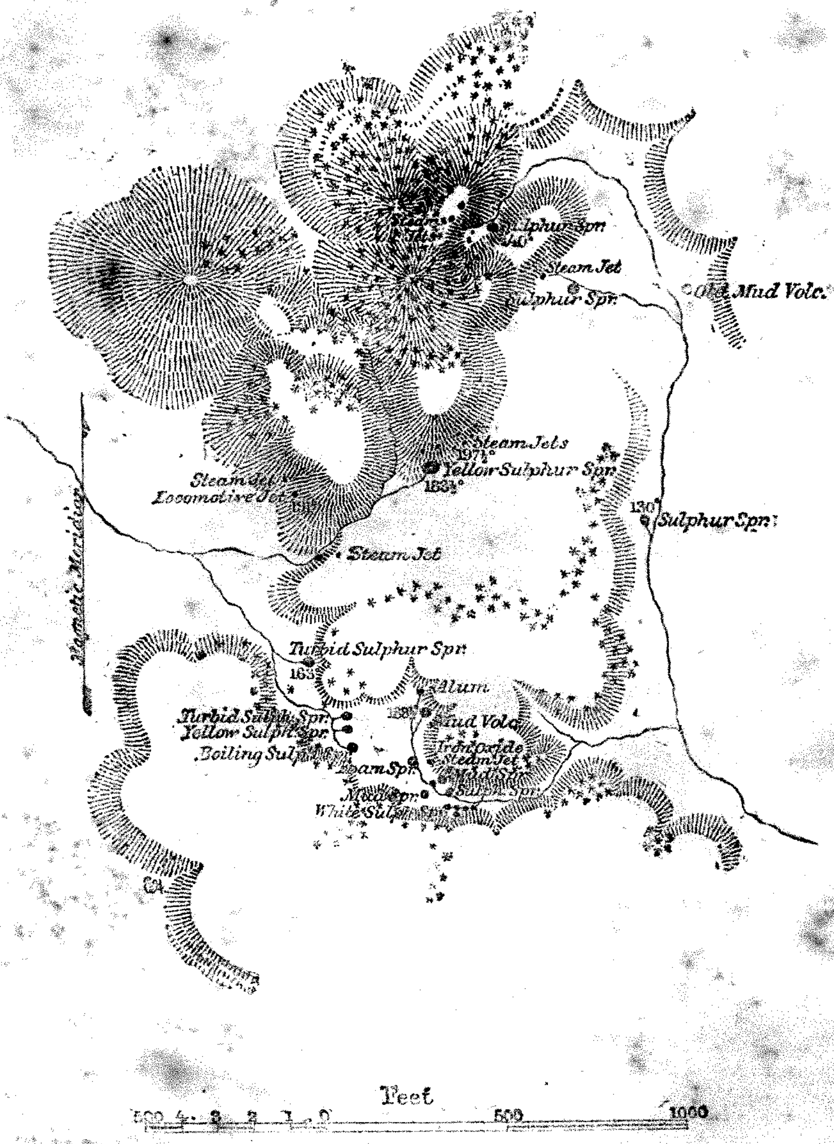

FIG. 2.—Sulphur and Mud Springs, Eight Miles below Yellowstone Lake.

The term Yellowstone Basin is sometimes applied to the entire valley; but the basin proper comprises only that portion enclosed within the remarkable ranges of mountains which give origin to the waters of the Yellowstone, south of Mount Washburne and the Grand Cañon. This basin is about 40 miles in length from north to south, and on an average 30 miles in width from east to west. It might be called the vast crater of an ancient volcano made up of thousands of smaller rents and fissures, out of which the fluid interior of the earth, fragments of rocks, and volcanic dust have been erupted in unlimited quantities. Hundreds of the nuclei or cones of these volcanic rents are now remaining, some of them rising to a height of nearly 11,000 feet above the sea. Indeed, as has been said, the hot springs and geysers of this region are only the closing stages of that wonderful period of volcanic action which began in Tertiary times. Even at the present time there are connected with these manifestations of internal heat earthquake phenomena which are well worthy of attention. Earthquake shocks are not uncommon, and are at some seasons of the year very severe.

Yellowstone Lake itself has at all seasons the temperature nearly of cold spring water, and its area is gradually but very slowly diminishing. Mr. Hayden estimates that since the period of volcanic activity the depth of the lake has been about 500 feet greater than at present, the shore line having then been high up on the sides of the surrounding mountains.

Warm springs are not uncommon in the valley of the lower Yellowstone, but the temperature is seldom higher than 60° or 80°. It is not until we reach Gardiner's River, a small branch flowing into the Yellowstone on the left side, opposite the third cañon, that the true hot springs commence in their full force. About three miles above its junction with the Yellowstone, the valley bottom is covered with a thick calcareous crust, the deposits of hot springs which are now extinct; but flowing swiftly from beneath this crust is a stream of hot water six feet wide and two feet deep, with a temperature of 132°. A little distance farther up is a high hill, on the slope of which has been formed a system of terraces, each from 200 ft. to 300 ft. in height, and covered with a thick deposit of lime. On the ascent of the hill, about three-fourths of a mile from the river bottom, there is to be met with one of the most remarkable exhibitions of hot spring deposits that is to be seen in this land of wonders. In the distance it looks like a vast glacier of snow and ice, on which account it has been named the White Mountain. Indeed, the different terraces can be compared, for their wonderful beauty, only to a frozen cascade. The remains of once active springs are plainly visible; old chimneys, irregular openings, like entrances to caverns, which extend beneath the crust, are numerous. This crust is probably from 20 ft. to 50 ft. in thickness, and underneath it is supposed that the surplus water from the active springs above flows down to the river. A little farther up is a series of basin-like pools, from 4 ft. to 8 ft. in diameter, and on a terrace about 200 ft. farther still are numbers of active springs, with basins 20 ft. to 50 ft. in diameter, some of them with several centres of violent ebullition in the same basin. The temperature at the outflow varies from 150° to 162°.

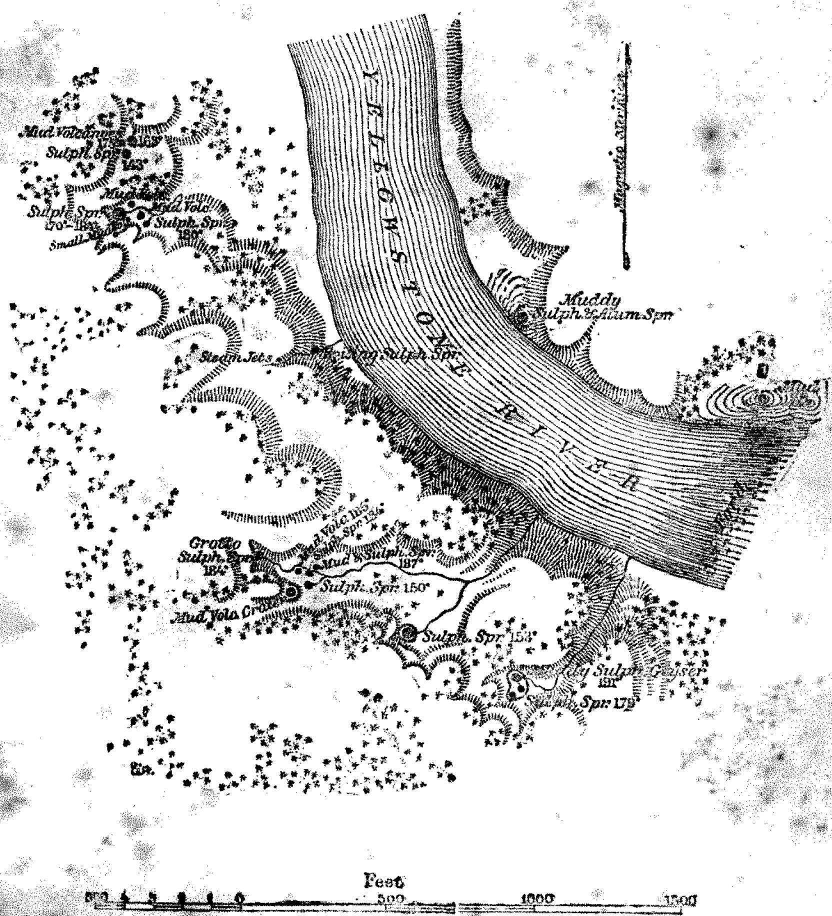

FIG. 3.—Sulphur and Mud Springs, Six Miles below the Lake.

Upon the terrace, down about midway on the side of the mountain covered with this deposit, the principal active springs are now located, and here is presented another picture to the eye which transcends any description in words. The water is perfectly transparent, and down in the clear depths can be seen distinctly the minutest ornament upon the inner sides of the basin; and the exquisite beauty of the colouring and the variety of forms baffle any attempt to portray them, either with pen or pencil; various shades of red, from the most brilliant scarlet to light purple; yellow, from deep-bright sulphur through all the shades to light cream colour; and green of various shades. These springs also are full of a kind of vegetation, which under the microscope proves to be composed of diatoms, among which Dr. Billings discovers *Palmella* and *Oscillaria*. There are also in the quiet springs, and in the little springs that flow from the boiling springs, great quantities of a fibrous, silky substance, apparently vegetable, which vibrates at the slightest movement of the water, and has the appearance of the finest quality of cashmere wool. A qualitative analysis made at the springs shows that the water contains sulphuretted hydrogen, lime, soda, alumina, and a slight amount of magnesia. Carbonate of lime predominates over all other elements in the deposits, and they may therefore be called calcareous springs.

There are two classes of springs in the Yellowstone valley, one in which lime predominates, in the other silica. With the exception of the White Mountain Spring in Gardiner's River, and one or two of not much importance, the other springs of the Yellowstone and Firehole basins are siliceous. They may be divided again into intermittent, boiling and spouting, and quiet springs. Those of the first class are always above boiling point during the period of action, but during the interval the temperature lowers to 150°. Those of the second are always at the boiling point, and some of them throw the water up two to six feet by regular pulsations. The springs of the third class may have once been geysers, but are now quiet, and have a wide range of temperature, from 188° to 80°. Where the temperature is reduced below 150° great quantities of the sesquioxide of iron are deposited by the water, lining the inside of the funnel, and covering the surface where the water flows. Taken in the aggregate, these springs have been in constant operation during our present period, and Mr. Hayden estimates that under favourable circumstances, at least six feet of this deposit have been precipitated within the space of one century.

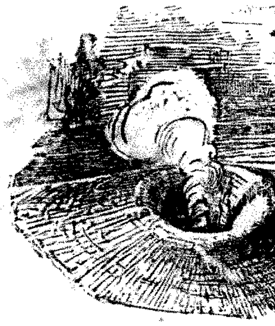

FIG. 4.—Hot Spring.

We must omit an account of the basaltic columns in the cañons of the Yellowstone and Gardiner's river, and of the great cañons, falls, cascades, and other wonders of this unique region, and pass to the hot springs of the upper basin. A few springs are seen at the mouth of Tower Creek, at the lower end of the Grand Cañon; but it is not until we pass the range of mountains which forms the north wall of the upper basin, about twenty miles above the lake, that the great hot spring district of the Yellowstone commences. There is here an area, within the drainage of the Yellowstone, forty miles in length, and on an average fifteen miles in width, that either is at the present time, or has been in the past, occupied by hot springs. The old deposits cover the region, and here and there are groups of active springs—mere remnants of what formerly existed. The Grand Cañon is a deep channel 1,000 to 1,500 ft. in depth, carved out of the basaltic rocks, and hot spring deposits, and on the sides of the walls may be seen the irregular fissures which communicate from the surface with the heated interior. Resting upon an irregular surface of basalt are immense deposits of silica of all colours, every shade of red, yellow, and white. Much of the deposit is as white as snow.

On the west flank of Mount Washburne, in the north of the area, there is a remarkable group of springs, in a constant state of action at the present time. Alum, sulphur, soda, and common salt, are found upon the surface in considerable quantities. Sulphuretted hydrogen is emitted from the spring in such quantities as to fill the air, rendering it oppressive with sulphurous odour. This group extends across the Yellowstone to the eastward for several miles. The springs, which are now in active operation, are only a few out of hundreds which once covered the entire area, but which are now dead or dying out.

Two remarkable groups deserve particular mention, the sulphur and mud springs, shown in Figs. 2 and 3. The largest group (see Fig. 2) is found on the east side of the Yellowstone, at Crater Hills, eight miles below the lake. This district covers an area of about half a mile square, and is sometimes called the "Seven Hills," from the fact that there are here several mounds of siliceous deposits from extinct springs, varying in height from 50 ft. to 150 ft. The old craters of dead and dying springs, and the immense quantity of the siliceous deposits, show that the present active springs represent only the last stages of what must have been at some period in the past a magnificent group. There are still numerous steam-jets, one of which, on the west side, produces a sound like that of a locomotive, which can be heard for a long distance. The surface is fairly riddled with little steam vents, and the crust sends forth a hollow sound beneath the tread; and on removing this shelly covering at any point, hot vapours come forth, while its under surface is encrusted with the most beautiful crystals of sulphur.

The springs at this point are either boiling, mud, or quiet springs. The principal boiling spring, which is in a constant state of ebullition, sends up a column of water 2 ft. to 4 ft.; has a basin about 15 ft. in diameter; and gives forth such a column of steam that it cannot be approached except on the windward side. But perhaps the most interesting objects here are the mud springs, which are of every size, from an inch in diameter to 20 ft. One of the largest is filled with fine light brown mud, which is in a constant state of agitation, the surface covered all over with puffs like hasty pudding. Others send forth a thud-like noise every second, with an impulse at long intervals that throws the mud up several feet. The water in the vicinity, as well as the mud, seems to be thoroughly impregnated with alum. In an adjoining valley are little mud or turbid water vents, which keep up a simmering noise, showing the nature of the earth beneath the crust.

Two miles above, on the same side of the Yellowstone, is the other group of springs, similar to those just noticed. Besides these are the geysers, to be alluded to presently. One of these is a true intermittent spring, and throws up a column of water 10 ft. in diameter, from 15 to 30 ft. high. The crater becomes filled with boiling water; suddenly immense columns of steam shoot up with a rumbling noise, the water overflows the basin, another column of water is thrown up for the space of 10 or 15 minutes, when it quiets down, and the basin is nearly empty. This operation seems to be performed about eight times in 26 hours. Upon the side of the hill bordering the river is one of the most terrific mud-cauldrons seen by Mr. Hayden during his visit. A large column of steam is constantly ascending, 500 ft. or more, from a deep funnel-shaped basin, 25 ft. in diameter; when the wind carries away the steam for a moment, the thin, black mud may be seen 25 ft. below the rim in the most violent state of agitation, with a noise like distant thunder.

On the shore of the south-west arm of the lake is an interesting group of hot springs, which extend along the margin, covering a belt about three miles long and nearly a mile in width. Many of these, which might be called pulsatory springs, are in a constant state of quite violent ebullition, but rise and fall every second or two, and with each pulsation throw out a quantity of water. Quite a pretty, symmetrical, funnel-shaped crater is formed with a circular rim, varying from a few inches to several feet in diameter. Some of these funnel-shaped chimneys (see Fig. 4) extend out into the lake several feet, and the hot spring deposits may be seen through the clear depths for fifty yards. The same variety of colours, quiet springs, mud springs, old ruins, &c., that have before been described, occur here. No geysers have been observed, but the group of mud springs keep up a constant thud-like noise, which can be heard with great distinctness for half a mile. At Steamboat Point are two vents, which keep up a constant pulsatory noise like a high-pressure engine on a river steamboat; columns of steam are thrown out at each pulsation to the height of 100 feet or more.

(*To be continued.*)

NOTES

WE may hope that we and the public have now heard the last of the unfortunate Hooker and Ayrton dispute. We learn that Mr. Ayrton has expressed himself satisfied with Dr. Hooker's explanation of the "offensive" matter in his letter to Mr. West, and here the matter will probably rest. It would be more satisfactory to know that all probability of similar unpleasantness for the future had been removed, and that the Government recognises the principle that a servant selected to control a great scientific establishment must necessarily be entrusted with all the details of its management.

THE rejection, by the Committee of Recommendations of the British Association, of the resolution of Section D respecting the treatment of Dr. Hooker as Director of Kew Gardens, resulted in the sending up to a subsequent meeting of the committee of a more strongly worded resolution to the same effect, which was then passed, not only by the Committee of Recommendations, but by the General Committee.

AT the meeting of the French Academy on the 2nd instant, the President presented to M. Chevreul a medal which had been procured by a subscription among his *confrères*. M. Dumas in a speech "à la manière anglaise," as the President expressed it, touched upon the chief services rendered by M. Chevreul to science in acknowledgment of which this medal was presented to him. Each subscriber is to receive a copy of the medal, and according to a slip inserted in the *Comptes Rendus*, the subscription list is still open.

IT is with great regret that we have to record the death, at the early age of 38, after a long and painful illness, of Mr. John Cargill Brough, F.C.S. He was a man of most accomplished mind and great general culture, and had personally endeared himself to all his acquaintances. Mr. Brough had filled, for about two years before his death, the office of secretary and librarian to the London Institution in Finsbury Circus, and had brought new life into its management.

WE hear from Paris of the death, at the age of 42, of one of the most promising of the younger generation of French botanists, M. Gris. He had written largely on both systematic and physiological botany, and held the post of assistant in the botanical department of the *Jardin des Plantes*.

THE inaugural address of the winter session of the Birmingham and Midland Institute will be delivered on Oct. 7, by Canon Kingsley, who is the president for the year.

MR. G. F. RODWELL, F.R.A.S., &c., has just been appointed Lecturer on Natural Philosophy at Guy's Hospital, still retaining his position as science teacher in Marlborough College.

THE Royal Polytechnic Institution appears, under its new management, to be assiduously encouraging the cultivation of Science. We have received a prospectus of classes held in the Institution in quite a number of branches of Natural and Physical Science.

WE notice from the *School Laboratory of Physical Science*, published in Iowa, U.S., that the total number of pupils who have attended one or more courses of lectures at the physical laboratory of the State University during the school-year of 1871-2, has been 340, representing all the departments of the University. Of these students, 270 have practised at the stands of the laboratory from two to ten hours a week each, and from the reports of the work done and the results, carefully calculated, of the examinations, it would appear that the teaching is varied and thorough. The high importance of laboratory work in the teaching of students is becoming more and more widely recognised in practice in the United States, and from this Report we learn that the great majority of the students themselves like the laboratory practice very much; those who do not, we are told, are those who are not fond of any serious mental work. As the opinion of the writer of the *Laboratory News* in the above work we quote the following note:—"One of the principal drawbacks to the perfect success of the Laboratory has been the admission of some students of advanced standing in the dead languages; we find these students almost invariably less careful in their work, and more hasty and illogical in their conclusions, than the regular beginners; they also have made the greatest blunders in calculation. As a result, such juniors have failed in competition with common sub-freshmen. I should not refer to these facts, if it were not so frequently asserted that the study of the dead languages constituted an auxiliary to the study of science."

THE Marlborough College Natural History Society has issued its Report for the half-year ending Midsummer 1872. In the Preface praise and blame are impartially dealt out to the members of the Society; the zeal and industry of individual members are commended in not undeserved terms; while the apathy and want of energy of the majority of the Society come in for severe censure. In the Botanical department some good work appears to have been done, and the president, the Rev. T. A. Preston, continues his Flora of Marlborough, the present instalment embrace the Calycifloræ. In Geology nothing has been done; in Ornithology a few of the members have shown active interest in "*genuine* Ornithology, not merely the taking of eggs, but observations of the birds themselves and their habits." The greatest triumph has been, however, in Entomology, where, thanks to the energy of two or three individuals, upwards of thirty moths have been added to the Society's list. Of the papers printed we may notice a useful one on Shells, by F. J. H. Jenkinson, and one on Thermo-Electricity and a new Thermo-Electric Battery, by Mr. Rodwell; others, however, are alluded to in the Preface. We cordially encourage the Marlborough College Natural History Society to continue its work.

MR. GOULD is now engaged on the preface to his great work on the "Birds of Great Britain," and will issue the last two parts, completing the whole, in 1873.

A VERY good "Flora of Liverpool" has been published by the Liverpool Naturalists' Field Club. The area included is within fifteen miles of Liverpool and two of Southport, and embraces some very interesting districts. The work has been performed by a committee of the society appointed for the purpose, with the assistance of amateurs and previously published records, which have all, when possible, been verified. It appears to have been carried out with great care, and some valuable notes are appended to the records of some of the species.

AMONG the most recently-published foreign flora we may note Dr. J. A. Knapp's "Plants of Galicia and the Bukowina," just published by Braumüller, of Vienna, in one thick volume.

THE last number of the *Quarterly Journal of the Meteorological Society* contains a letter from M. Hoffmeyer, Director of the recently established Meteorological Institute of Denmark, giving some details of the work it is intended to accomplish. The sphere of the Institute embraces all the branches of Meteorological Science, and it is especially intended to establish in favourable situations a series of stations, furnished with accurate instruments, by which it will be possible, every morning, to send telegraphic communications to the chief station at Copenhagen, and from that, according to agreement, to foreign societies. When the stations are fairly in working order, observations will be published monthly. It is also proposed by the Institute to establish about ten complete meteorological stations at the Faroe Isles, Iceland, and in Greenland; half of these are expected to be in trim by next winter. Besides the general interest attaching to these stations, it is hoped they may tend to foster a system of international meteorology, and pave the way for the laying down of a northern telegraphic cable between Europe and America. The observations at these stations will be specially published. The establishment of this Institute is likely to be of the greatest service to general meteorology.

IN "Railways or No Railways; the Battle of the Gauges Renewed," those who take an interest in the subject will find the case on behalf of the narrow gauge fully and ably set forth.

A "DISSERTATION on the Use of the Stethoscope in Obstetrics," by Æneas Munro, M.D., read before the Royal Medical Society of Edinburgh, seems to be a valuable contribution to the science of the subject to which it relates.

WE have received a pamphlet, "Irrigation not necessary in Upper India," by Major A. F. Corbett, Superintendent Budaon Police, in which the author attempts to prove that irrigation, instead of fertilising that country, will inevitably render it an almost barren waste. The statements he adduces, and the opinions of eminent scientific men and others that he quotes, certainly appear to bear out the writer's theory, and on that account his pamphlet deserves the attention of all who take an interest in the welfare of India.

THE "dead season" has brought up its usual crop of reports of the re-appearance of the sea-serpent, mostly easily resolvable into masses of floating sea-weed. The following extract from an evening contemporary well illustrates the hazy ideas prevalent as to the extinct Saurian monsters of which the sea-serpent is supposed to be a descendant:—"If the sea-serpent continues in its present sociable state of mind, we may perhaps have an opportunity of deciding the vexed question regarding the formation of that portion of his figure which, according to English observers, he keeps concealed under the water. The legend of the Lambton Worm, a popular tale in the North of England, describes the worm as a serpent of enormous size, who used to coil himself round a hill overhanging the River Wear, just as thread is wound round a reel, but a very ancient stone effigy of the creature which lately existed at Lambton Castle, represents it with ears, legs, and a pair of wings. If this effigy was made, as it probably was, *from some recollection* or recent tradition of the Lambton Worm, these adjuncts would indicate that the beast was one of the *winged land monsters* which existed at the same time as the *Ichthyosaurus*, but would naturally become an extinct species far sooner than the *fish lizard*, which can conceal itself in the depths of the ocean from the curiosity and violence of man."

IT is not for want of good examples that the British Government is so backward in encouraging deep-sea dredging; other governments seem to think it their interest or duty to do so. The United States, as we know, have fitted out an expedition under MM. Agassiz and Pourtales, to explore the Gulf Stream, the Straits of Magellan, and the Pacific Ocean. A second American expedition will, in the same way, explore the northern regions of this ocean; the German Empire has undertaken to search the depths of the Atlantic; while Sweden has sent to Baffin's Bay two ships fully equipped for deep-sea sounding.

THE BRITISH ASSOCIATION

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE

Fifth Report of the Committee for investigating the Rate of Increase of Underground Temperature downwards, and in various localities of dry land and under water, by Prof. Everett.

IN December last, intelligence was received from Prof. Sismonda that the administration of the railway owning the Alpine tunnel had given permission to Father Secchi to carry on a series of observations in the tunnel concerning terrestrial magnetism, and that this distinguished observer was willing at the same time to conduct observations of temperature in accordance with the plans of your Committee. Two maximum and two minimum thermometers were accordingly placed in Father Secchi's hands; but it appears that the arrangements for commencing the magnetic observations are not yet completed, and that accordingly no observations of temperature have as yet been taken.

Prof. Lubimoff of Moscow, on receiving a copy of last year's report, wrote to the secretary, correcting a mistake in the description of the thermometer used in taking observations in the Moscow well. The thermometer was enclosed in a hermetically sealed case containing air, and was therefore completely protected against any possible effect of pressure. Prof. Lubimoff at the same time asked to be furnished with a thermometer of the new pattern described in the report (the upright-Negretti pattern), and one of these instruments was accordingly sent.

Dr. Wild of the Central Observatory, St. Petersburg, wrote in January, requesting that two thermometers for observations in bores might be ordered in his name. At this time, the Secretary was in correspondence with Sir Wm. Thomson, who entertained doubts as to the successful working of the new thermometer, and expressed a preference for the Phillips pattern (which has been described in preceding reports) and the Casella-Miller pattern (a modified Six) which has been extensively used for deep sea temperatures. Thermometers of these two patterns were accordingly ordered and despatched to Dr. Wild.

A letter was received from Prof. Henry of the Smithsonian Institution, Washington, in April, stating that the Chief Engineer of the Hoosac Tunnel had promised to have observations of temperature taken in the tunnel, if thermometers were sent. Its total length will be $4\frac{3}{4}$ miles, about two-thirds of which has been penetrated, by working from both ends and from a central shaft 1,028 feet deep. The mountain has two ridges, under which the tunnel passes, and their heights above it are respectively 1,720 and 1,420 feet. Four thermometers have been sent, viz.: two large minimum Rutherfords, for observations in the tunnel, and two upright Negrettis, for observations in the shaft.

The Council of the School of Mines at Ballaarat, Australia, have, in compliance with a request addressed to one of their number by our observer, Mr. David Burns, C.E., consented to take charge of these thermometers, and furnish observations from the bores and shafts in that important gold-mining district. Most of the principal mining managers are connected with the school. Four thermometers have accordingly been sent, viz.: two upright Negrettis for observations in bores, and two simple mercurial thermometers, of large size, for observations during the sinking of shafts.

Some exceedingly deep Artesian borings have been undertaken in France in recent years; and the President of the Geological Society, Mr. Prestwich (who has allowed his name to be added to your Committee) has furnished your Secretary with introductions which will probably lead to the obtaining of very numerous and valuable observations from these wells.

The largest of them all is one which is now sinking for the municipality of Paris, at La Chapelle, St. Denis, a northern suburb of Paris, and has already obtained a depth considerably exceeding that of the Puits de Grenelle. It is expected that its final depth will be about 2,300 feet. Application was made by the Secretary to the eminent firm of well-borers, Messrs. Mauget, Lippmann, and Co., who are sinking the well, and these gentlemen at once in the most obliging manner consented to take observations of temperature in it. An upright Negretti thermometer was accordingly furnished; and about the 20th of June your Secretary had the pleasure of receiving from them two complete sets of observations taken on the 14th and 15th of that month with their own hands, at every 100th metre of depth, and also at the bottom of the well, 660 metres deep.

	FIRST SERIES. June 14, 15.			SECOND SERIES. June 17, 18.		
Depth in Metres.	Temp. Fah. °	Time down h.	m.	Temp. Fah. °	Time down h.	m
100	58·0	0	35	58·0	3	30
200	61·1	0	30	61·0	2	0
300	65·0	0	30	65·0	2	0
400	69·0	3	10	69·0	11	20
500	72·6	0	30	72·6	2	0
600	75·8	0	30	75·4	2	0
660	83·25	15	45	83·25	2	0

The observations are given in the subjoined table, in which the third column shows the time that the thermometer was allowed to remain at the depth specified before hauling up and reading. The temperature at which the thermometer was set before letting it down is also given in Messrs. Mauget and Lippmann's report, but is not here inserted.

The agreement between the first and second set of observations is remarkably close; and as the time of leaving the thermometer in the water was about half-an-hour in most of the observations of the first set, and two hours or more in all the observations of the second set, it is obvious that half-an-hour is a sufficient time to give a correct observation. This conclusion is satisfactory both as regards the reliability of the observations themselves, and also as establishing the fact that this pattern of thermometer is not unreasonably slow in its working. The exactness of the agreement also serves to show that the thermometer can be depended on to the tenth of a degree, and that we may henceforth use it with confidence.

Before proceeding to discuss the observations, it will be convenient to give a few particulars respecting the well, which have been kindly furnished by Messrs. Mauget and Lippmann.

It was commenced by the municipal authorities as a masonry well, by the ordinary method of digging, until it had reached a depth of 34·5 metres. The intention was to carry it in this way to the depth of about 135 metres, the estimated depth of the tertiary strata covering the chalk; but the difficulties and dangers which were encountered, from the want of tenacity in the soil (*la nature essentiellement ébouleuse des terrains*), and latterly from the insufficiency of the pumps, rendered it necessary to abandon this intention; and in May 1865 the task of completing the well by boring was assigned to Messrs. Degousée and Laurent, the predecessors in business of the gentlemen to whom we are indebted for these observations. A small trial bore (0·2m. in diameter) was commenced, and continued till January 1866, by which time the machinery for the heavier work was ready. In order to support the masonry, which showed signs of giving way, it was tubed through its whole length with a tube 1·8m. in diameter and 0·02m. thick, cemented externally. From the bottom of this tube, at the depth of 34·5m., a bore 1·7m. in diameter was carried to the depth of 68·7m. from the surface of the ground. A second tube 1·58m. in internal diameter was inserted to the depth of 121·6m., and a third tube of internal diameter 1.39m. was carried down into the chalky marls and the upper portion of the chalk at the depth of 139·15m. from the surface. From this point downwards, the bore has been driven through the chalk, and tubing has been unnecessary, its diameter at the depth of 662m. being still 1·35m.

The thickness of the tertiary strata is 137m., and the elevation of the surface of the ground above sea-level is 48m. or 157ft.

The springs which were met with in the tertiary strata correspond to those found in other parts of the basin in which Paris is situated, and have not sufficient strength to spout above the surface of the ground at this elevation. They were encountered at the depths of 19·2m., 34·5m., 86·0m. and 97·0m., and the water now stands in equilibrium in the central tube at 16·5m. below the surface of the ground.

It was not practicable to take observations of temperature during the regular progress of the boring, but an interruption occurred on the 12th of June, and the tool was not at work from this date till after both sets of observations were finished. In reference to this point, Messrs. Mauget and Lippmann, say, under date April 29, "To obtain the natural temperature, it will be necessary to select a time when the work has been interrupted for several days; for the boring being executed by the fall of a heavy tool upon the bottom of the well, the percussion developes a considerable amount of heat, as we perceive by the mud (*les boues*) which we extract, and which in coming to the surface is found to have still a temperature of from 48° to 90° C. (118° to 194° F.)." In their letter of June 19, containing the report of the observations, they remark :—

"You will observe that though the water at the bottom of the well is still some degrees above its natural temperature owing to the action of the drill *(trepan)*, the latter has not been in operation since the 12th of the month. At a convenient time, we intend to observe the temperature of the mud as it lies at the bottom of the well, immediately after the withdrawal of the drill, when the latter has been working constantly, a temperature which will probably be found to depend upon the hardness of the rock."

The following table exhibits the successive increments of temperature showed in the second series, which purports to be more accurate.

Depth in Metres	Increase in deg. Fahr.	Metres per deg Fah.	Feet per deg. Fah.
100 to 200	3°·00	33·3	109
200 to 300	4·00	25·0	82
300 to 400	4·00	25·0	82
400 to 500	3·60	27·8	91
500 to 600	2·80	35·7	117
600 to 660	7·85	7·6	25

The last two columns of this table show that the rate of increase is about four times as rapid in the last 60m. as in the rest of the well, a circumstance which naturally suggests the explanation given by Messrs. Mauget and Lippmann. There are however some difficulties in the way of accepting this view. Comparing the two sets of observations, one taken on the second and third day after the withdrawal of the tool, and the other on the fifth and sixth day, we have precisely the same temperature at the bottom of the well on both occasions, although the observations were sufficiently precise to detect a difference of a tenth of a degree where such difference existed. It seems difficult to believe that a temperature 2½ degrees above the normal temperature could have remained for two days without sensible diminution. In connection with this question, the apparent cooling to the extent of 0°·4 at the depth of 600m. between the first and second observation demands attention, and is not very easily explained.

If the observed temperature at 660m. is to be taken as the normal temperature, the average increase from 100m. to that depth is at the rate of 1° F. in 22·1m. or in 72·5 ft. If the observed temperature at 600m. in the second series is adopted, the increase from 100m. to that depth is at the rate of 1° F. in 28·7m. or in 94·2 feet.

The observations prepared by Messrs. Mauget and Lippmann in the paragraph above quoted will be eminently calculated to assist in showing the correct interpretation.

Mr G. A. Lebour, F.G.S. of H.M. Geological Survey, has furnished observations taken in a bore hole executed at the bottom of South Hetton Colliery, Durham. The observations were taken by Mr. J. B. Atkinson, a student at the Newcastle College of Physical Science, and appear to have been carefully made. Thanks are also due to the viewer of the colliery, Mr. Matthews, for granting the requisite facilities.

The hole is 2½ inches in diameter, and was bored out of the pumping side of the South Hetton shaft, in order that the bore rods might be the more readily altered. The depth of the shaft is 1,066 feet; that of the bore hole 863 feet from the bottom of the shaft, or 1,929 feet from the surface of the ground. The section of the boring (not including the shaft) consists of 123 alternating beds of shale and sandstone,* with occasional thin seams of coal and some fire clays. The bottom of the boring has reached a very coarse white grit, which is supposed to be the topmost bed of the millstone grit series.

The bore was dry at the time of its execution; but has since become filled with water, probably derived from the shaft above it. Streams, in fact, pour down the shaft, and play about the hole.

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*A complete list of the strata has been furnished, and will be preserved by the Secretary, with a view to future reference if required.

Two thermometers, one of them an unprotected Phillips, and the other a protected Negretti, were supplied by the Secretary to Mr. Lebour, as it was not certainly known at that time whether the bore was dry or wet. Mr Lebour indeed believed it to be dry, but nevertheless selected the Negretti thermometer, as it was thought that the Phillips could not be read off accurately with the poor light which in the position of this bore hole was alone available.

The following table exhibits the results of all the observations which have been taken in the bore, including three which were taken in 1869, while the boring was going on. The boring was stopped, in the case of each of these three observations, only about 20 minutes before the observations were made; and the heat due to friction appears to have produced abnormal elevation of temperature, amounting to about 2° at the depth of 288 feet, to about 6° at the depth of 582 feet, and to considerably more than this at 858 feet. The other observations in the table are Mr. Atkinson's, taken with the Negretti thermometer.

Depth from bottom of shaft, in feet.	Depth from surface of ground, in feet.	Temperatures observed during boring, April 1869.	Temperatures observed April 1872.
100	1166	—	66
200	1266	—	$68\frac{3}{4}$
288	1354	72	—
300	1366	—	70
400	1466	—	72
500	1566	—	$74\frac{1}{2}$
582	1648	82	—
600	1666	—	$76\frac{1}{8}$
644	1710	—	75
670	1736	—	$77\frac{1}{8}$
858	1924	96	

The temperature 75° at the depth of 644 feet, a temperature lower than either of the two between which it stands, was taken on the first day of Mr. Atkinson's observations, and was confirmed by repeated trials at that time. This was the lowest depth that could then be reached, the remainder of the boring being apparently plugged up with "sludge." A spike was subsequently attached to the thermometer case, which enabled it to pierce deeper into the sludge; but the lowest depth which could be reached (670 feet) is still far from the bottom of the bore.

It is intended to take a fresh series of observations at every 50th foot of depth, and especially to re-examine the temperatures at about 650 feet, where the reversal of temperature was observed.

The following are the rates of increase deduced from Mr. Atkinson's observations, omitting the temperature 75° at the depth of 644 feet.

Depth in feet.	Increase in deg, Fahr.	Feet per deg.
100 to 200	$2\frac{3}{4}$	36
200 to 300	$1\frac{1}{4}$	80
300 to 400	2	50
400 to 500	$2\frac{1}{2}$	40
500 to 600	$1\frac{5}{8}$	62
600 to 670	1	70
		51·2
100 to 670	$11\frac{1}{8}$	

The average increase between the depths of 100 and 600 feet is 1° in 51·2 feet. These depths are reckoned from the top of the bore hole, which is 1,066 feet below the surface of the ground. Mr. Lebour assumes that the temperature at the depth of 600 ft. from the surface of the ground is 48°. Accepting this estimate, we have a difference of $29\frac{1}{8}$° in 1,676 feet, (1,066 + 670 − 60 = 1676) which is at the rate of 1° in 57·5 feet.

Mr. David Burns, F.G.S., reports that, from changes in the management of the mines, and other causes, it has not been possible as yet to carry out the dry observations at Allenheads mentioned in last year's report.

Only one other shaft has been met with at all suitable for observation. It is called Brandon Walls shaft, and belongs to the Rookhope Valley Mining Company, to the courtesy of whose agent we are indebted for liberty to take observations. This shaft is some 6 miles east of those reported on last year, and is situated in the very bottom of Rookhope Valley. The mouth is covered over with a wooden shed, the shaft itself is free from all obstruction, and the water in it has not been disturbed for some ears. The shaft is 333 feet deep, and is full of water to within 25 feet of the surface of the ground. Observations (by Mr. Burns and Mr. Curry of Bolkburn) were taken in it on five different days in July of the present year; but though agreeing well with one another from day to day, they are so irregular that they throw little light on the rate of increase of underground temperature. At the depths of 83 and 133 feet from the ground, the temperature was 48°·5. In the next 50 feet there was an increase of about 3°, the temperature at 183 feet being about 51·4, and from this depth to the bottom (an interval of 150 feet) the temperature was nearly constant. The best determination of the temperature at the bottom was 51° 7.

It may be remarked that all observations in shafts thus far have exhibited irregularities of this kind. The water in such large openings seems to have its temperature governed by springs and other extraneous causes, rather than by the temperature of the surrounding soil.

The observations at every fiftieth foot of depth in the Kentish Town well, as given in previous reports, are so complete that it has not been thought necessary to continue them. A very delicate thermometer, reading by estimation to the $\frac{1}{100}$ of a degree, has however been procured, for taking observations from year to year at one constant depth (1,000 feet). It was constructed ten months ago, and being enclosed in a partially exhausted glass tube will probably not undergo much change of zero. It has been four times tested by comparison with standards, and has been found to have no error amounting to nearly so much as 0°·1. In consequence of Mr Symons' illness, no observation has yet been taken with it in the well.

A thermometer which, through the breaking of a rope, had fallen into the mud at the depth of 1,090 feet from the surface of the ground, was extracted by Mr. Symons last November, more than a year after its fall. It had sustained no damage, and its indication when hauled up was 69° 4, nearly agreeing with the temperature previously observed at that depth.

In addition to the large numbers of thermometers above mentioned as having been issued during the past year, one has been furnished for observations which are to be made in the projected boring through the Wealden and underlying strata. With the exception of Mr. Symons' observations at Kentish Town (London, N.), we have as yet no observations of temperature from the Southern parts of England.

SECTION B—CHEMICAL SCIENCE

Mr. Alfred Tribe read a paper *On the Precipitation of Silver by Copper*. In the course of experiments made in conjunction with the President, Dr. Gladstone, it was found that the silver obtained by precipitating the metal from the nitrate by means of copper always contained more or less of the latter metal. When an excess of silver remained in a solution only minute traces of copper were found, but as the silver solution became exhausted the proportion of copper rapidly increased. This co-precipitation of copper was shown to be due to the presence of atmospheric oxygen. In one experiment as much as 15 per cent. of copper was obtained after 48 hours exposure. When carbon dioxide was caused to bubble through the solution during the precipitation the quantity of copper deposited was greatly diminished. The author showed an eudiometric apparatus in which this property of absorbing oxygen was applied to determine the proportion of that gas in the air.

Mr. Gladstone gave a brief account of the physical and chemical characters of the *Volcanic Dust* recently ejected from Vesuvius. In some localities the fall of this dust was very heavy and extended over a considerable area: the sample examined was collected at Ischia, upwards of twenty-five miles from the mountain. It consisted essentially of a mixture of quartz and magnetite. No trace of titanium could be detected. Dr. Thorpe stated that he had recently examined the volcanic sand found in the neighbourhood of Etna, and its agreement in chemical and physical properties with the sand from Vesuvius was very striking. It also contained no titanium.

Dr. Schenck read a paper *On the Amount of Heat required to raise Elementary Bodies from the absolute zero to their state of fusion*. If we assume that a body at — 273° is completely deprived of heat it is possible to calculate the total heat in it at any other temperature provided that the specific heats of the body in its three states of aggregation, its latent heats of fusion and vaporisation, and its melting and boiling points are known. Such calculations are limited from the fact that only in the case of one body—water—are the data sufficiently well known. In the course of the paper the author pointed out a remarkable coincidence between cadmium, tin, and lead, in the amount of heat required to raise gram-equivalents from $-273°$ to the state of fusion.

Mr. W. Lant Carpenter made a communication respecting the presence of *Albumen in Fats, and on a new method of obtaining Stearic and Palmitic Acids.* The paper mainly consisted of an account of Dr. Bock's remarkable process for the decomposition of Fats which is now being generally adopted on the Continent, in the manufacture of improved stearin candles. When fats are decomposed in the ordinary process by alkali, a considerable excess of the alkali above the theoretical quantity is required unless the operation is conducted under great pressure, when the risk of explosion increases the disadvantageousness of the process. When the fats are decomposed by oil of vitriol, or other strong acid (the method usually adopted in England), a considerable proportion of the fat is lost by being charred and burnt, and that which remains is so blackened that it is necessary to distil it, an operation of expense and of danger owing to the risk of fire or explosion. All these advantages are obviated by the use of Prof. Bock's process. Dr. Bock has shown that most neutral fats are made up of minute globules surrounded by albuminous envelopes, which form from 1 to 1·5 per cent. of the weight of the fat, and he considers that the action of the alkali, acid, or of heat or pressure was to break up these albuminous envelopes. The destroyed envelopes had a remarkable power of attracting the colouring matters contained in the fat or produced therein during the action of the acid or alkali. The existence of the albumen may be demonstrated by dissolving the fat in ether or benzol and adding water to the solution, or by boiling the fat with a strong solution of oxalic acid. In each case the albumen envelopes collect at the plane of juncture between the two liquids. In the new process the envelopes are broken up by the action of a small quantity of strong sulphuric acid for a limited time only and at a given temperature. The fat is then poured away from the destroyed envelopes and is ready for decomposition by water in open tanks. This operation requires some time for its completion; its progress may be readily determined by a microscopic examination of the crystallised fatty acid formed by slowly cooling a thin layer upon a glass slide. When the process of decomposition is at an end, the solution of glycerine is drawn off purified and concentrated for sale. The fatty acids thus obtained amount to 34 per cent. of the original fat: they are however far from pure and contain more or less brownish or black matter. By submitting the fatty acids in open tanks to the action of a dilute solution of certain oxidising agents, the dark coloured matters are partially oxidised and their specific gravity is so far increased that when the oxidation has proceeded far enough, they readily subside together with the envelopes to the bottom of the tank, and the supernatant fatty acids are rendered comparatively good in colour. After two or three repetitions of this process the resultant stearin is hot and cold pressed in the ordinary manner. The acid thus obtained is of a better quality, has a higher melting-point, and is yielded in greater quantity than that obtained in the ordinary way.

Mr. J. F. Walker contributed a paper *On Dinitrobrombenzene*, and Dr. Wright gave an account of the continuation of his experiments on *New Derivatives from Morphine and Codeine.*

Mr. John Williams described an improved method of preparing Guaranine, the active principle of *Guarana*, the fruit of the *Paulina sorbilis*, which is used by the Amazonian Indians for an infusion. This principle was isolated by Stenhouse, and pronounced by him to be identical with theine or caffeine, the active substance contained in tea and coffee. In the author's process the guarana is reduced to fine powder mixed with one-third of its weight of hydrate of lime and moistened with water. It is then allowed to stand for a couple of hours and thoroughly dried at a gentle heat. The mixture is exhausted with boiling benzol filtered, the benzol distilled off, when a small quantity of light coloured oily matter remains. This is treated with hot water and heated for some time over the water bath, filtered through a moist filter, and after concentration, the solution is set aside to crystallise. In about twenty-four hours the guaranine separates out perfectly pure. The same process is applicable to tea, but the author is inclined to believe that guaranine differs in several particulars—taste, solubility in water, &c.—from theine.

Mr. Wanklyn described a method of analysing the *Compound Ethers*—acetic ether, for example. It consisted in determining the amount of alcohol liberated in the decomposition of the ether by the known methods of alcoholimetry. The complete proximate analysis of a compound ether is thus rendered possible.

Prof. Crum-Brown made a brief communication on the subject of *Chemical Nomenclature.* Setting aside the trivial or proper names (names which are simply arbitrary words or marks each indicating in virtue of a convention applicable to each individual case, a particular substance), there are two systems or kinds of systems of chemical nomenclature. These may be distinguished as 1st, the composition system, and 2nd, the functional or relational system, or class of systems. In the first the name of a compound indicates the elements or radicals contained in it, and sometimes their proportions. Thus Chlornatrium, Chloriod, Dreifach chloriod, Silicium wasserstoff, &c. In English we have few names so distinctly compositional in form (we have indeed, Zinc methyl and all the other allied names) but many of our names, although apparently functional in form, are really compositional. Thus, chloride of A means with us nothing more than, or different from, a compound containing the elements chlorine and A: and chloride of sodium, chloride of iodine, ter-chloride of iodine, siliciureted hydrogen, not only represent the same substances as the German names just quoted, but tell us neither more nor less about the substances than these German names do. On the other hand, functional names present the chemical relations between substances. We may take as examples such names as the anhydride, the amide, the aldehyde, the nitride of acetic acid. These derivatives of acetic acid contain no acetic acid, but they stand in certain definite relation to that substance, and the anhydrides, amides, aldehydes and nitrides of other acids stand in the same relation to them. What is still, notwithstanding the efforts of modern chemists, the common popular nomenclature of salts, although originally intended as a compositional nomenclature, might, with perfect consistency, be retained as a functional nomenclature. The objection to the term "muriate of soda" was that the substance so named contains no soda. But the amide of benzoic acid contains no benzoic acid. Soda contains oxygen; muriate of soda contains none (unless chlorine be an oxide), but the nitride of benzoic acid contains no oxygen, although the acid itself does. The name muriate of soda originally meant the *compound* of anhydrous muriatic acid, $2HCl-H^2O$, and anhydrous soda Na^2O – $(2HCl-H^2O)+Na^2O$. We may now, if we please, use the name to mean the result of the action $2HCl+Na^2O-H^2O$. If we do so, the name becomes a functional one, and the phrase "muriate of," or, what is neither better nor worse, "hydrochlorate of," expresses the complex operation. Addition of hydrochloric acid and simultaneous separation of water. Similarly, in the case of such names as sulphate of potash, nitrate of oxide of silver, &c., the phrases "sulphate of," "nitrate of" express the complete operations, addition of sulphuric, or nitric acid, and simultaneous separation of water.

While the old view that salts are compounds of anhydrous acids and anhydrous bases is now abandoned by most theoretical chemists, a relic of this view still remains in the most advanced systems of nomenclature, producing an inconsistency really inconvenient to the teacher and student.

The objection taken to the name hydrochlorate of soda was not only that the substance contains no soda, but also that it contains no hydrochloric acid. This objection is perfectly valid against the name as a compositional one, but does it not equally hold against the words sulphate, nitrate, acetate, &c.? If we are to have hydric sulphate and hydric acetate for sulphuric and acetic acids, why not hydric muriate for muriatic acid? That this question is not altogether an absurd one will be obvious if we consider that all chlorides are not muriates. Those substances which are by general consent called salts stand in a definite genetic-relation to the corresponding acids (or the hydric salts of the series), and it is inconvenient to have the same general name —chloride—applied to substances which do stand in this relation to hydrochloric acid, and also to those which do not. We may divide the chlorides into two groups, very different in character in their extreme members, and gradually shading into one another. We may take chloride of sodium as a representative of the one, and the chloride of phosphorus as a representative of the other. Chloride of sodium is a muriate; the chloride of phosphorus might be better described. We may call the acids and acid anhydrides negative, the hydratic bases, anhydrous bases positive—arranged in a series, we find the series a continuous one from the most positive or basic oxides or hydrates to the most negative; it is however convenient to have a zero point, and it is no disadvantage if this zero point be an arbitrary one. When we come to express numerically the amount of positiveness or negativeness of these oxides and hydrates, it will be necessary to have a zero point, and a very convenient one is that which corresponds pretty nearly to the generally understood limit between bases and acids, and depends upon the direction in which the action takes place.

SECTION C.—GEOLOGY

On the Cambrian and Silurian Rocks of Ramsey Island, St. David's, by Henry Hicks, F.G.S.*

In a report to the British Association in 1866, by the late Mr. Salter and the author, Ramsey Island was mentioned as a part of the district which had been examined and a short description of the rocks exposed there was given. At that time three distinct formations in succession had been recognised, and also correlated by their fossil contents and lithological characters with the Lingula flags, the Tremadoc group, and the Arenig group. Since then the author has further examined these beds, and recently along with Messrs. Homfray, Lightbody, Kirshaw, and Hopkinson.

During these researches numerous new forms have been discovered in these rocks, and many additional and interesting facts observed. In a section at the north end of the island the following rocks occur in succession:—

1. *Lingula Flags.*—A series of hard silicious sandstones with grey flaky slate, about 600 feet in thickness, and containing *Lingulella Davisii* in great abundance but no other fossils save worm tracks and burrows, and some plant-like markings.

2. *Tremadoc Group.*—Bluish grey flag, and earthy grey rock of a tough texture, from 800 to 1,000 feet in thickness. Fossils are very abundant throughout the whole series, and nearly all the species as well as many of the genera are new. They comprise Brachiopods of the genera *Lingula*, *Obolella*, and *Orthis*, and Lamellibranchs of the genus *Ctenodonta*. There are also two species of *Orthoceras*, a *Theca*, a *Bellerophon*, an *Encrinite* and a star fish, and nine species of Trilobites belonging to the genera *Dikelocephalus*, *Conocoryphe*, *Niobe*, *Asaphus*, *Cheirurus*, and *Calymene*, and a supposed land plant named *Sophyton explanatum*. Some of these genera are characteristic of the Cambrian rocks, and others of the Silurian; and there are several forms which had not previously been discovered in rocks of so early an age. Until the discovery of these rocks at St. David's the Tremadoc group was supposed to be a local formation only.

3. *Arenig Group.*—A series of ironstained slates and flags, having a thickness of 1,000 feet. The fossils comprise Trilobites belonging to the genera *Asaphus*, *Ogygia*, *Oeglina*, *Trinucleus*, *Ampyx*, *Calymene*, and *Agnostus*; also a *Conularia*, *Theca*, *Orthoceras*, *Bellerophon*, *Lingula*, and *Orthis*, and about 20 species of Graptolites.

In this section the succession from the Cambrian to the Silurian rocks is probably better shown than at any other place in Britain.

SECTION D.—BIOLOGY

DEPARTMENT OF ZOOLOGY AND BOTANY

Second Supplementary Report on the extinct Birds of the Mascarene Islands, by Alfred Newton, F.R.S.

The speaker stated that a portion of the grant unexpended at the last meeting of the Association had been expended by his brother in a renewed examination of the caves in the island of Rodriguez. This has been conducted by Mr. George Jenner, lately Chief Executive Officer of the island. No detailed account could at present be given. Several missing parts of the skeleton of Pezophaps, and of additional remains of the large Psittacine bird, described from a single fragmentary maxilla by Milne Edwards as *Psittacus* (?) *rodericanus*. This may enable its affinities to be more exactly determined, and also allow more light to be thrown on *P. mauritianus* of Owen. A bird described by Leguat, and hitherto believed to be extinct, had been found still to exist, and had been described by himself as *Palæornis exsul*. The remains of a Ralline bird, considered to be allied to Ocydromus Milne Edwards, was disposed to identify with the "Gelinotte" of Leguat, the nature of which had hitherto only been a matter of guess.

Dr. Sclater said it was well to bear in mind that Rodriguez was one of the stations where it was proposed to place a staff of astronomers to observe the transit of Venus, and the opportunity of carrying on ornithological observation at the same time should not be lost sight of.

On the Perforating Instrument of Pholas candida, by Mr. John Robertson.

The author attributed the perforating action of the animal to a rasping effected by the rotatory movements of the shell and also by putting the valves together.

Prof. Allman said that the late Mr. Bryson, of Edinburgh, had observed the habits of the Pholas, and had come to the conclusion that the boring was effected by the foot charged with silicious particles and acting like the leaden wheel of the lapidary.

Mr. Gwyn Jeffreys was of opinion that in the whole of the perforating conchifera and some of the univalves the foot was the instrument of perforation. In *Cardium*, *Mactra*, and especially *Solen*, as well as other bivalve mollusca, the posterior extremity of the shell was shaped to receive the foot which worked like a gardener's dibble. In the case of *Pholas dactylus*, Mr. Caillaud thought that at Nantes the gneiss was perforated by the rasping action of the shell. Man might do this, but it was doubtful whether it could be accomplished in this way by the *Pholas*. In *Teredo navalis* he believed that, as Sellius had shown in 1733 in his work "De Teredine Marina," the foot was the sole instrument of perforation, and in this case the posterior extremity of the shell had a large excavation to receive the foot. Again, *Pholadidea* in a young state excavated by means of its foot, but afterwards the aperture was closed by gelatinous matter, the animal became encysted, and no further excavation took place. The limpet he had seen in Aberdeenshire excavate the rock to the depth of a fourth of an inch, and this could only have been accomplished by the foot. In *Pholas* also no part of the shell can act at the bottom of the excavation. The prickles it was supposed were renewed; but this could not take place throughout the shell, and many excavating shells had no prickles at all. Deshayes had advocated the chemical theory; but this too had been exploded, as Deshayes himself admitted.

Summary of Flowering Plants of Sussex, by W. B. Hemsley.

Taking Babington's Manual (5th Edition) as a standard, the Flora of Sussex includes 1,059 species of flowering plants, reckoning Ferns and Horsetails as well. These last amount to only 33, or about 3 per cent. Roughly speaking $\frac{3}{4}$ are Dicotyledons and $\frac{1}{4}$ Monocotyledons; $88\frac{1}{2}$ per cent of the species are herbaceous, and $11\frac{1}{2}$ woody; $27\frac{1}{2}$ per cent are annuals, and $72\frac{1}{2}$ perennial; 12 natural orders include rather more than half the whole number of species; 76 of the species are maritime, and 56 peculiar to the chalk. *Pyrola media*, *Habenaria albida*, and *Festuca sylvatica* are outliers of Watson's Scottish type not found in adjacent counties. The three species peculiar to Sussex, *Phyteuma spicatum*, *Lonicera Xylosteum*, and *Trifolium stellatum* are probably all introductions, the last being certainly so. In the centre of the county the heath grows as high as three or four feet, and covers considerable tracts of land.

Prof. Lawson in answer to a speaker who had inquired the useful purpose of these investigations into indigenous plants, and who had lamented the want of adequate knowledge how to keep them in their place, pointed out that the researches of Messrs. Lawes and Gilbert were likely to lead to practical methods of developing the useful constituents of pasture and of restraining the growth of the undesirable elements. He was especially struck with the presence of *Centaurea calcitrapa* about Brighton. This he had generally seen as a ballast plant, and thought almost certainly an introduction.

Diversity of Evolution under Uniform External Conditions, by Rev. John T. Gulick.

The terms "Natural Selection" and "Survival of the Fittest" present different phases of a law which can act only where there is variation. Does this variation ever produce from one stock distinct varieties and species, while the external conditions remain the same? When a species is subjected to a new set of conditions, does the change that is brought about in the organism expend itself in producing just one new species completely fitted to the conditions, or may it produce many that are equally fitted? Facts in the geographical distribution and variation of the terrestrial molluscs of the Sandwich Islands seem to throw light on the subject. A forest region on the island of Oahu, 40 miles in length, and 5 or 6 miles in breadth, furnishes about 175 species, represented by 700 or 800 varieties. The average area occupied by each species is about 5 or 6 square miles, though many are restricted to half that area. The valleys that lie on one side of the mountain range that traverses this district preserve, as far as we can observe, the same conditions; but the varieties, and in some cases the species, found in each valley, differ from those found in any other.[*]

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*The discussion referring to this paper occurs at p. 383 (after Mr. Hopkinson's paper.)

If we would account for these facts on the hypothesis of evolution, it seems necessary to suppose: First, that these molluscs possess an inherent tendency to variation, so strong that all that is needed to ensure the divergence of type in the descendants from one stock is to prevent, through a series of generations, their intermingling with each other; and secondly, that either the tendency to variation in this family is very much greater than usual, or their tendency to migrate weaker, and their opportunities fewer than usual. An investigation of the conditions under which that species exist leads me to believe that the smallness of the areas occupied by each is due to freedom from that competition that retards variation in Continental species, rather than to any deficiency in the means of transportation. On the continents, "Natural Selection" arising from severe competition with species that have a wide range, tends to prevent the development of varieties, and to give a wider diffusion of forms, that would otherwise be limited in their range, and variable in their type.

Mr. Wallace agreed with the Rev. J. T. Gulick in his interpretation of facts which appeared to be exceedingly remarkable. He had had the opportunity of working at a limited group of organisms in a small part of the world. The results he had described were a type of what took place over whole continents, and exhibit an example of variation and geographical distributions, perhaps the most remarkable that occurs on the surface of the earth. With the general principle that variation does not depend on difference in external condition, he altogether agreed. He thought in this matter that there was a confusion of two distinct things, even in some cases by Mr. Darwin himself. Variation was confounded with the formation of varieties. That it was not dependent on the change of conditions was evidenced by the fact that the varieties of domestic animals and plants were not due to this cause, but only to advantage being taken of spontaneous variation and identical conditions. Horticulturists obtained new varieties of any plant that was introduced into cultivation by growing it upon a very large scale, and selecting the sports which were sure to occur. In this case variation was accumulated by artificial selection, just as it is accumulated in nature by natural selection. This requires, as a condition of its action, a change of external conditions. We all know that closely allied, though distinct species, were found inhabiting distinct areas—for example, islands; and with large continental areas it was the same. This had led to the very general idea that it was variation of condition over those areas which had produced the varieties, whereas it had merely selected them. In the Sandwich Islands there was no difference of physical conditions adequate to produce this result. This was seen in the number of intervening forms which existed. It seemed due to the absence of any weeding-out effect. The land molluscs had hardly any competitors to struggle with, and no enemies, quadrupeds and reptiles being absent, and birds few. The rivers were small and would only distribute any form through the same valley. All these conditions favoured this remarkable persistence of closely linked forms.

SECTION G—MECHANICAL SCIENCE

On Rolling in a Seaway, by Mr. W. Froude, F.R.S.

This was a description of an apparatus for automatically recording the rolling of a ship in a seaway and the slopes of the waves.

The fundamental principles on which the performance of the apparatus depends are (1) that when waves act on a ship or other floating body which would stand stably upright in still water, she will be for the moment in equilibrium if upright or normal to the mean or effective slope of the wave which she occupies; and if she possess a given righting force when inclined to a given angle in still water, she will be urged by approximately the same righting force towards the normal position in wave water, if she at any moment deviate from it by the same inclination. (2) A plumb line or pendulum, if its point of suspension be at or very near the ship's centre of gravity, will be for the moment in equilibrium if it occupy the normal position, and if it have a very short period of oscillation it will instantly assume that position throughout the changes of the wave slope. These two propositions are but expressions of the interdependence which exists between the change of translatory motion which at any moment affects a mass or particle of matter, and the direction at the same moment proper to any force-direction-index carried by the man, whether it be a plumb-line, which lies in the direction, or a spirit level, which lies at right angles to it; the direction being simply the resultant of gravity, and of the disturbing forces which at the moment affect the mass. Mr. Froude described his apparatus as follows:—A revolving cylinder covered with paper and turned by rough clockwork receives the marks made by several pens. One of these pens records time, jerks being given it at successive equal intervals by an exact clock. The apparatus being placed at the centre of gravity of the ship, a pendulum of very short period and considerable power, oscillating in the plane transversely with the keel, records continuously by a second pen the angles made at each moment by the ship, with the mean or effective wave slope, that is to say, her relative inclinations. Another pen, actuated by a rocking arm kept level by the observer on deck, who points it to the horizon, records the angle made at the same moment by the ship with the horizon, that is to say, her absolute inclinations. From the records thus obtained, the amount of the roll of the ship with regard to the wave slope is at once shown, and the form of the wave can be easily worked out graphically, the wave slope at each moment being simply the difference between the records produced by the pendulum pen and the horizon pen respectively. But the graphic integration of the results supplied by the pendulum pen, if correctly performed, supplies what might be called the theoretical measure of the oscillations, which the ship ought to have performed with regard to the horizon during the period embraced in the record. For the pendulum record itself supplies, throughout, a measure of the accelerating force by which the ship's oscillation is governed; so that the integration of this gives a diagram representing the angular velocity which the ship should theoretically have acquired under the operation of that force. And the integration of the velocity diagram in turn gives the sequence or total of motions which the varying velocity involves. The performance of these integrations involves indeed a correct knowledge of the ship's dynamic constants, but these, so far as they are not already known by calculation, may be readily obtained by a single experiment with the ship in still water, where, if she be artificially brought into oscillation (an operation easily performed), and the instrument be made to record the oscillations as they subside under the influence of resistance, the natural period of her oscillation is at once known, and the coefficient of resistance is deducible in a shape which is approximately applicable to the ship's seaway oscillation, All the conditions required for the integration are thus supplied. Several series of diagrams thus obtained by the oscillation of ships in a seaway have been thus integrated, and the theoretical oscillations accord so completely with the recorded oscillations that the fundamental elements of the theory of rolling have been most satisfactorily verified. Mr. Froude said he had more recently contrived and executed an apparatus which would substitute an automatic record of the ship's absolute inclinations for that supplied by the observer on deck, as above described. For this purpose he employed a heavy stationary wheel, which was so delicately supported as to be incapable of receiving any rotation from the motion of a ship. This wheel, if placed transversely in the ship, would remain still at rest—that is to say, without rotation—and would thus, while the ship performed oscillations of rotation under it, communicate to one of the tracing pens a virtual motion along the record cylinder, so as to form a continuous record of the ship's absolute inclinations. The wheel is 3 ft. in diameter and 200 lb. in weight. Through the boss is carried out a strong steel axis, the prolonged ends of which are coated with hardened steel. The axis thus prolonged rests between two pairs of rocking arms, the ends of each pair forming a kind of V. The ends of the arms are, in fact, hardened steel plates, forming segments of circles struck from the axes or centres on which the arms rock, so that they are virtually portions of the circumferences of very large friction rollers. In order still further to reduce the friction of the working parts, the axes of the rocking arms have been finally reduced to hardened steel pins of small diameter, and so mounted that their motions, when of small range, should be rolling not sliding motions, and great delicacy is thus obtained. The centre of gravity is brought to within 0·0065 in. of the axis of suspension, and the time of a single swing is over thirty-five seconds. Yet so great is the delicacy of the suspension, a weight of $\frac{1}{250000}$ part of that of the wheel itself, if placed at its extreme radius, will produce an oscillation of 1¼ in, in range, and which will continue for many minutes; or if the wheel be moved 90° from its position of rest, the oscillations will continue for nearly twenty minutes, the movement being so slow and solemn as to impress on the mind of an observer who had not seen it put in motion that the action was self-originated, or induced by some mysterious agency. The oscillation of a ship can scarcely communicate any motion at all to the wheel, and any minute rotation which is, in fact, communicated will assume the form of an oscillation, having so long a "period" that its effects will be easily separable from those proper to the oscillation of the ship. Thus the indications will be more exact than those produced by the rocking arm on deck. This improved apparatus has not yet been tried, but is ready, waiting a suitable day for trial on board a ship at Plymouth.

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[*] A fuller statement of the fact has been given in an article on "Variation of Species related to their Geographical Distribution," in NATURE July 18th, 1872.

Mr. Froude stated that though the apparatus he had described was purely his own invention, it had interested him greatly to learn recently that an arrangement substantially identical with that combination which he first described had about two years previously been invented and successfully used by an able French naval architect (M. Bertin, of Cherbourg), with whom, partly in virtue of this circumstance, it has since been his good fortune to become acquainted and to correspond. It was, however, a satisfaction to him that he was at the present time ahead of his friendly competitor in the race, so far as regarded the delicately-hung heavy fly-wheel which was to furnish an automatic constant record of the angles or absolute rolling or deviations from the horizontal assumed at each moment by the ship.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, Aug. 5.—Prof. Cayley presented a memoir on Orthogonal Surfaces.—M. E. Becquerel communicated a spectroscopic analysis of the light emitted by the phosphorescent uranium compounds.—M. Daubrée presented a note on the discovery of a second meteorite, which fell on the 23rd of July last, in the canton of St. Amand (Loir-et-Cher). This appears to have formed part of the fall noticed at the meeting of the Academy on July 29.—A long letter, illustrated with figures, from Father Secchi, on the solar eruption observed on July 7, and on the phenomena which accompanied it, was communicated. In this paper the author referred to the phosphorescent light emitted by certain animals, and upon this subject MM. de Quatrefages, Milne-Edwards, and E. Becquerel made some remarks.—M. Dumas read an important memoir on alcoholic fermentation, and a note on the ferments belonging to the diastase group.—MM. Favre and Valson presented a continuation of their researches upon crystalline dissociation.—M. G. Ville presented a memoir on the quick quantitative determination of phosphoric acid.—A note by M. Houzeau, on the decolorising power of concentrated ozone, was read, and upon this M. P. Thenard made some remarks.—M. Wurtz presented a note by M. E. Grimaux, on some derivations of tetrachloride of naphthaline.—A note was read by M. Sirodot on a bone-deposit situated at the foot of Mont Dol, containing bones and teeth of elephant, horse, ox, rhinoceros, and other mammals, generally broken and often calcined, with a few fragments of flints and at least one stone implement.—M. C. Sainte-Claire Deville presented a note by M. Gorceix, containing a summary of the phenomena presented by the volcano of Santorin at the close of the eruption of 1866, or from December 1869, to October 1871.—M. T. Lestiboudois presented a note on what he calls heterogeneous Dicotyledons, or those which do not produce their new tissues exclusively in the generative zone between the wood and the bark.—M. Duchartre communicated a note by M. J. Duval Jouve on a form of epidermic cell which appears to be peculiar to the Cyperaceæ.—M. de Quatrefages read a memoir on the Mincopies and the Negrito race in general, containing a discussion of the characters of the Andaman islanders, and of their relations to the other black races of man.—M. Blanchard presented a note by M. J. Kunckel on the development of the striated muscular fibres in insects, in which the author maintains that the primitive element of the muscle is a cell, which, by its elongation, forms the fibrilla, the fibre or primitive bundle being a secondary formation.—M. Blanchard also communicated a note by M. A. Tillot on the embryonic form of the Hairworms (*Gordius*), in which the development of those parasites from the egg is described, and they are shown to possess, in the embryonic state, some analogy with the *Echinorhynchi.*—A note by M. J. Gerbe on the formation of the adventitious products of the ovum of the Plagiostomi was presented by M. C. Robin.

August 12.—Prof. Cayley communicated a continuation of his memoir on orthogonal surfaces.—M. Yvon Villarceau presented a further memoir on the applications of his new theorem of general mechanics to the equilibrium of gases.—General Morin presented a report upon a memoir by M. Graeff, on the action which the breakwater of Pinay exerts upon the floods of the Loire at Roanne.—A note on the vibrations of cords and rods in liquids, by M. E. Gripon, was read.—M. Pasteur presented a note, by M. E. Branly, on the measurement of the intensity of currents by means of the electrometer.—A note, by M. Broun, on magnetic variations observed at Trevandrum during the eclipse of December 11, 1871, was presented; as also a note containing observations of meteors at various stations on August 9, 10, and 11, by MM. Le Verrier and Wolf.—A short note on the observations relating to presence of magnesium in the chromosphere of the sun, by M. Tacchini, was transmitted by M. Faye.—MM. Favre and Valson presented the continuation of their thermo-chemical researches upon crystalline dissociation.—M. Berthelot presented a note on the partition of a base between several acids in solutions, in which he treated of the monobasic acids; and M. Pasteur communicated a note, by M. E. Jungfleisch, on the conversion of right tartaric acid into racemic acid by exposure to heat in the presence of water.—M. Dumas called attention to some researches, by M. Latimau, on *Phylloxera vastatrix.*—M. Brongniart presented a detailed report upon a most important memoir, by M. Grand-'Eury, on the Carboniferous flora of the Department of the Loire.

BOOKS RECEIVED

Foreign.—Through Williams and Norgate—Alexander von Humboldt, 3 vols.: Karl Bruhns.—Etudes sur les Facultés des animaux comparés à celles de l'homme. J. C. Houzeau, 2 vols.—Oeuvres des Verdet, Tome vii—Théorie mécanique de la chaleur: E. Verdet, Vol. i. and Vol. ii, Parts 1 and 2.

American.—Report of the Palæontology of Eastern Nebraska: T. B. Meek.

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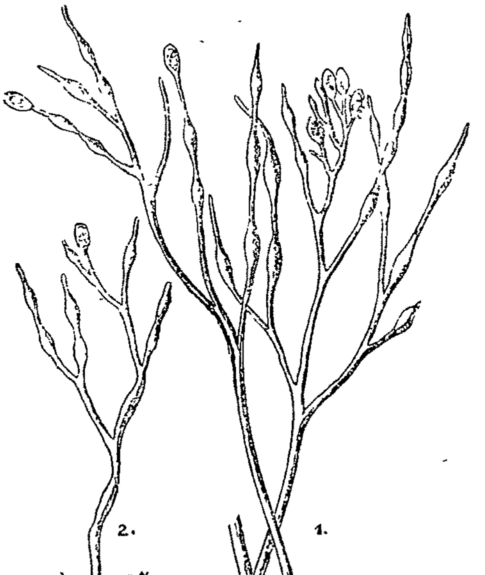
Errata.—Vol. vi, p. 382, Section C, Geology, line 7, for "graptolite" read "graptolites," line 20, for "*T. serrus*" read "*T. serra*," line 25, for "*Dictyograptus*" read "*Dichograptus*;" line 31, for "*Phyllograptus*" read "*Ptilograptus*;" line 35, for "not" read "two."

THURSDAY, SEPTEMBER 19, 1872

THE POTATO DISEASE

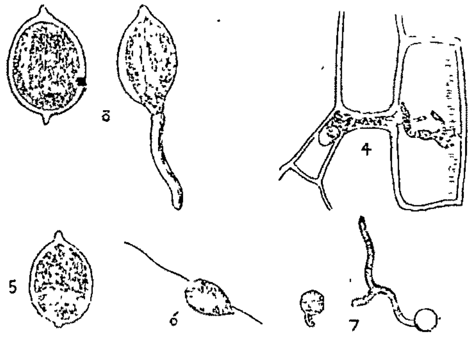
II.

THE habits of the fungus which produces the potato disease, *Botrytis* or *Peronospora infestans*, have been closely investigated by Montagne, De Bary, and Berkeley. The latter gentleman has described its life-history in the *Gardener's Chronicle*, and the editor of that paper kindly permits us the use of the accompanying woodcuts, which illustrate Mr. Berkeley's paper. The fungus bears abundance of spores on the tips of the branches, the mycelium, or spawn, burrowing amongst the cellular tissue of the leaf and causing rapid decomposition, while the vertical threads which branch and bear the spores, find their way through the stomata, or leaf pores. The spores themselves, falling on different parts of the plant, germinate, and, penetrating the tissues, produce a brown tint, not only in the cells with which they are in immediate contact, but also in the adjacent cells. In addition to these spores, or, more correctly speaking, conidia, the fungus produces also zoospores, or moving spores, furnished with the well-known movable threads or processes, and which are differentiated from the contents of some of the ordinary spores. These bodies, like the ordinary spores, germinate and penetrate the tissues, producing the same brown tint, and in the same way as the kind already mentioned.

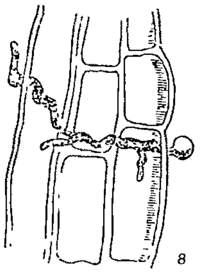


1. *Peronospora infestans.*
2 The same, burrowing among the tissues of the leaves, and making its way through the stomata.

The fungus was unknown to the older mycologists, having, at all events, never attracted attention before the first great outbreak of the potato-blight in 1845. It is not quite peculiar to the potato, being found also occasionally on other plants belonging to the natural order Solanaceæ, especially on the fruit of the tomato. The first indication is the well-known brown spots on the leaves of the plants; but by the time it manifests itself in this manner, its mycelium has already made deep inroads into the cellular tissue of the leaf and stem, and cure is well-nigh hopeless. The cutting-off of the haulm close to the ground will generally prevent the spread of the disease to the tubers, but this remedy is at the risk of destroying the quality of the crop, since it is impossible for the tubers thoroughly to mature after the entire removal of the vegetative organs. The growth of the fungus is greatly promoted by wet weather, and the "sowing" over the crop of lime, which acts as a powerful desiccant, is therefore a natural remedy, as also is the application of soot, the properties of carbon as a disinfectant and absorbent of gases being so well known. Prof. Gardner states that the diseased tuber is strongly alkaline, from the presence of ammonia, the sound potato having an acid reaction. Since, also, we are commonly visited by heavy showers during the latter part of July and early part of August, the period when the disease generally first decidedly manifests itself, it is probable that the advice of the *Gardener's Chronicle* is sound, to plant chiefly early varieties, and lift early, though this will involve some loss to the productiveness of the crop. On all these points, however, we are greatly in want of the authoritative opinion of practical men, the result of a careful series of experiments from which all accidental causes have been eliminated. The Royal Horticultural Society has a fine opportunity of performing



3. Spores, or, more properly speaking, conidia, germinating.
4 The same, sown artificially, and penetrating the tissues after eighteen hours.
5. Spores with contents differentiated.
6. Zoospores.
7 Zoospores germinating.



8 Zoospores sown artificially in the stem, and after twenty-four hours penetrating the tissues and entering the intercellular spaces

a most important service to practical agriculture by the institution of a series of crucial experiments in its experimental grounds at Chiswick.

Dr. Hooker's statement that the starch of the diseased potatoes is not affected by the parasite, but retains its nutritive properties, is worthy of more attention than it has yet received. He recommends rasping the peeled tubers upon a grater into a tub of cold water. In a few minutes the starch will be found to have sunk to the bottom, and the diseased matter, woody fibre, &c., will be suspended in the water, and should be poured away with it. Fresh water should then be added, the starch stirred up, and again allowed to settle. Two or three of such washings will remove all impurities, and render the starch fit for use. If thoroughly dried it will keep for any time, and can be used as arrowroot for puddings and cakes, and for mixing with flour as bread. This plan is open to obvious objections, both from its tediousness, and from the fact that when the disease has made any considerable progress, the smell is so offensive as to render both the peeling and the grating alike impossible to those possessed of ordinary olfactory perception. Dr Hooker, however, states that the plan was successfully pursued in 1845 and 1846 by the villagers of Hitcham under the direction of the late Rev. Prof. Henslow; and it seems incredible that chemical science should be unable to devise some practical and economical method for separating a wholesome substance of such enormous value in the bulk from the noxious ingredients.

Although the immediate cause of the potato disease has been clearly determined, as has been stated above, to be the attacks of a parasitic fungus, yet we are by no means prepared to agree with the conclusion of a distinguished writer,* that it "seems quite absurd, when the whole *rationale* of the potato disease has been so carefully explained, to look for an explanation in mere climatic conditions, exhaustion, weakness of constitution, or any of the empirical causes which are so often brought forward." The *Peronospora* is undoubtedly the proximate cause of the disease; for the ultimate cause we may have to look to a very different set of circumstances. It is probable that many epidemic diseases, both of men, animals, and plants, are caused by parasitic fungi; yet the attacks of the parasite may be favoured by special climatal or other conditions. We know that many animal plagues can be absolutely eradicated by the removal of the conditions which favour the propagation of the pest. The idea hinted at in our article last week that epidemic diseases may be expected in periodically recurring cycles is at least one deserving further investigation, and is supported by some curious facts. No reason has been given why the potato blight should have broken out so violently in 1845, when we had experienced before that many as ungenial summers during which it did not make its appearance, nor why it should have appeared simultaneously in so many remote countries—in St. Helena and Canada as virulently as in Europe—where the climatal conditions are altogether different; 1860 and 1872 are also not the only wet and thundery summers we have known during the last twenty years. It is quite possible that cosmical conditions may at definite intervals favour the disease, but that it may be developed only when certain other special conditions co-operate with these. We refer in another column to the systematic investigation now being carried on in France and Portugal as to the cause of the vine disease.

The period of maximum sun-spots of between eleven and twelve years, as shown by the researches of De La Rue, Stewart, and Loewy, falls in 1848, 1860, and 1872; and it is very singular that history seems to point to nearly the same approximate period for great national epidemics. Thus, according to Hecker's "Epidemics of the Middle Ages," the dates of the five great visitations of the sweating sickness in this country were 1485, 1506, 1517, 1528, and 1551, the epidemic being accompanied on almost every occasion by other pestilences in man and beast in this and other countries, or by general failure of crops. We have, of course, no means of ascertaining the condition of the surface of the sun during those years; but it is at least a singular coincidence that 29 cycles of 11·1 years bring us from 1551 to 1872.

We do not wish to lay more stress on these curious facts than they deserve, but simply to indicate the number of points of view from which the subject may be investigated. To have ascertained accurately the immediate cause of the potato disease is no doubt a great step gained; but the scientific method will not allow us to stop here. We must do our best to penetrate further into the arcana of Nature, and bring all the resources of Science to bear to investigate the conditions which appear to favour the appearance and spread of the invader, and the means which promise the greatest chance of success to repel his attacks.

GLADSTONE'S LIFE OF FARADAY

Michael Faraday. By J. H. Gladstone, Ph.D., F.R.S., &c. (London: Macmillan and Co., 1872.)

THERE can be no doubt that a life of Faraday suitable for the general public was much needed. Dr. Bence Jones's work, though full of interest to scientific men, and to those who knew Faraday personally, was too voluminous and too lacking in cohesion to be very widely read. Dr. Tyndall's brilliant sketch fascinated the reader with the scientific aspect of the life it recorded, but left one longing to know "the inner supplement to that noble outward life" of Faraday. Dr. Gladstone's memoir very largely meets the wants we express.

There are few men better fitted to write a life of Faraday than Dr. Gladstone. Not only was he the personal friend of Faraday, associated with him in the management of the Royal Institution, and in scientific inquiries connected with the Trinity House, but, what is more important, their pursuits and sympathies ran in nearly parallel paths. And so with a loving hand Dr. Gladstone has gathered the materials for his memoir. These are drawn only to a small extent from sources with which we are already familiar, for the author gives us the benefit of his own recollections, and also numerous incidents from the many private sources of information to which he has had access. A delightful freshness and personal interest are thus given to the narrative. Here, for example, is an anecdote that will be new to most of our readers, and which illustrates the quiet humour which Faraday possessed One evening at the Royal Institution, when a lecture on the English language was being delivered, the lecturer mentioned as a common vulgarism the habit of using "don't" in the third person singular, and cited, as an instance, "He don't pay his debts." Faraday, who was sitting in his usual place to the right of the lecturer, immediately exclaimed aloud, "That's very wrong!"

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* Rev. M. J. Berkeley, in *Gardener's Chronicle*, Nov. 4, 1871.

A very striking story was related to Dr. Gladstone by Cyrus Field. It is a sequel to the very circumstance that Dr. Tyndall has chosen as an example of Faraday's wonderful insight into nature, so like "a divining power." When Mr. Field was making the preliminary arrangements for that great enterprise with which his name will always be associated, he sought Faraday's advice on the electrical questions involved in a transatlantic cable. Faraday told him that he doubted the possibility of getting a message across the Atlantic. Mr. Field saw that this fatal objection must be settled at once, and begged Faraday to make the necessary experiments, offering to pay him properly for his services. The philosopher, however, declined all remuneration, but worked away at the question, and presently reported to Mr. Field—"It can be done, but you will not get an instantaneous message." "How long will it take?" was the next inquiry. "Oh! perhaps a second." "Well, that's quick enough for me," was the conclusion of the American; and the enterprise was proceeded with. This is an important incident, for not only does it show the readiness with which Faraday, when appealed to, freely lent himself to aid any scientific work, but it also indicates the confidence with which he applied the results of the laboratory to the grandest practical operations.

There are many other parts of Dr. Gladstone's memoir that we have marked for quotation, but our space will not allow us to do justice to the book by extracts. With pleasure we refer our readers to the volume itself, which is distinguished throughout by the modesty and self-forgetfulness of its author, its earnest tone, and the entire absence of all technicalities.

Dr. Gladstone will, however, permit us to indulge in a little friendly criticism on the arrangement of his materials. The memoir is divided into five parts—first, "the story of Faraday's life" is told us; then comes a "study of his character;" after which we have "the fruits of his experience," followed by "his method of working;" and in the last section we are shown "the value of his discoveries." With all deference to the reasons—no doubt well considered—which induced Dr. Gladstone to arrange his memoir thus, we venture to think that such a division is inartistic, and at first sight apt to repel a thoughtful mind. For is it not a slight upon the intelligence of his readers if an author presumes they cannot draw the study of a man's character nor "the fruits of his experience" from "the story of his life." Moreover, after having read the first part, which every one must agree is admirably done, we come upon a collection of incidents in Faraday's life, and anecdotes illustrating his character, that lose much of their force, by being massed together for a predetermined effect. It is the very spontaneousness and unobtrusiveness of the words and actions of a noble character that constitute their real claim to our admiration. And therefore we think it would have been far better if the author had woven into the life of Faraday the numerous incidents he has collected, rather than have let them lose their charm by a process of classification.

But if the arrangement is, in our humble opinion, not quite perfect, it certainly is the only blemish we can find in the work. It is a biography that will be read with interest by every intelligent person, and can be thoroughly enjoyed by those who are quite ignorant of science. It is a capital book for the youth of the present day, and among many of them we trust it will arouse a noble enthusiasm.

Now let us turn from the book to the man. Our readers do not need us to remind them of the history of Faraday, a history, which viewed in the grand achievements of his life, is so simple as to be almost sublime. Everyone knows that from a bookbinder's apprentice he lifted himself to the highest position in the scientific world. Here we may remark that we doubt whether a truly heroic nature has ever been quite covered up by the crowd of humanity. A great soul is a hero anywhere, and will win the recognition and often the worship of mankind, in spite of every obstacle. Faraday was an example of this. For the first twenty years of his life who knew him? What was his residence but a mere number in a back street? But before his death who did *not* know him? Every scientific society had laid a tribute of admiration at his feet, and he received letters inscribed to "Professor Faraday, member of all Academies of Science, London."

As his fame grew, and it became possible for him to make his name profitable, and realise thereby a considerable fortune, he permitted no selfish consideration to influence his career. Promptly rejecting that income which came from the sacrifice of his investigations, he obeyed the voice that said to him, "*Wisdom* is the principal thing, therefore get wisdom." So for fifty years one lofty idea animated his thoughts and sustained his indefatigable labour. His life became the ideas of what a philosopher's life should be, written only in the imperishable work he has done. Accepting no reward for his labours, declining any title or elevation of rank, seeking no public applause, living and dying in retirement and comparative poverty, Faraday consecrated himself to his work, and freely left to the world the magnificent revelations of a genius devoted to Nature. This surely is the life of a hero: noble devotion combined with utter unselfishness, the characteristics of a Cromwell, a Bruce, or a Garibaldi.

Though Faraday lived in retirement, and spent most of his hours in the laboratory, few men have passed a happier lifetime. No doubt the orderly and sustained simplicity of his daily life was one cause of the perpetual gladness of his heart. He confined his thoughts chiefly to his work and his home, and he regulated his life in the most methodical and natural manner. This may be seen from a little incident mentioned to the writer by Mr. Faraday's early friend, the late Mr. Benjamin Abbott. Some thirty years ago Mr. Abbott called to see Mr. Faraday, whom he had not met for many years. On inquiring at the Royal Institution the hall-porter told him that Mr. Faraday was at work, and could not be seen by anyone, indeed, that he dare not even take a visitor's card down to him. On Mr. Abbott's explaining that he was an old friend, the hall-porter suggested, with an obliging politeness which is conspicuous there still, that, as it was near one o'clock, Anderson (Faraday's faithful assistant) would soon be going to his dinner, when probably he might catch sight of Mr. Faraday coming upstairs. Mr. Abbott waited; punctually at one Anderson emerged from the laboratory, Mr. Faraday followed, and, recognising his old friend at once, begged him to join them at dinner. "For," added Faraday, "I am a Goth you know, and always dine in the middle of the day." At dinner Faraday told Mr. Abbott a characteristic story about Anderson; how one morning during his glass experiments he found his assistant had been stoking the furnace all night long. Faraday had told him to keep the fire up, and omitting to release him in the evening, Anderson, with his soldier's excellent experience, stuck to his post till he received the next orders from his master. The fact that this simple obedience was all the assistance Faraday ever had increases the astonishment with which one regards the extent of his labours. The secret of the massiveness of Faraday's work was no doubt that he felt he had *one* aim before him, and therefore he rigidly kept from himself everything that would fritter away his time; political and commercial matters were passed by; he had his warfare to accomplish, and "no man that warreth entangleth himself with the affairs of this life."

Whilst Faraday found it necessary to shut out society, he enjoyed a social gathering, especially a family of children, whom he would amuse by dexterous experiments in glass-blowing, or with other less scientific manipulation. He liked occasionally to take his nephews to the pantomime, himself enjoying, as he said, "the immense concentration of means which it requires." Or he would attend in his early days, as Dr. Gladstone tells us, "the artistic and musical conversaziones at Hullmandel's, where Stanfield, Turner, and Landseer met Garcia and Malibran; and sometimes he would join this pleasant company at supper and charades, at others in their excursions up the river in an eight-oared cutter."

His delight in giving others pleasure showed itself continually. He was considerate and courteous to the lowest servant at the Royal Institution. The writer can testify to the enduring impression made by his words of kind encouragement and acts of thoughtful friendship towards the assistants in the laboratory. As Dr. Bence Jones well remarks, his humility had a living root in his religion. Of the intense earnestness of the religious aspect of his nature, it does not become us here to speak; he did not talk, but he lived Christianity. He rose above his peculiar creed. In no sense contracted or censorious, he shed a warm radiance on all with whom he came into contact. It was impossible to dwell in his presence without feeling all that was best in one's nature rising to the surface. Most tender and touching were the words he addressed to sorrowing friends. What a depth of affection is revealed by the following letter, written to a lady who had recently lost her husband:—

> "Believe that I sympathise with you most deeply, for I enjoy in my life-partner those things which you speak of as making you feel your loss so heavily.
>
> "It is the kindly domestic affections, the worthiness, the mutual aid in sorrow, the mutual joy in happiness which has existed, which makes the rupture of such a tie as yours so heavy to bear; and yet you would not wish it otherwise, for the remembrance of those things brings solace with the grief. I speak, thinking what my own trouble would be if I lost my partner, and I try to comfort you in the only way in which I think I could be comforted."

We are reminded by this and other letters of Faraday, that the inner life of a man is best seen in the letters to his intimate friends never meant for publication. In Faraday's exquisite letters, exquisite because so utterly artless, are revealed the happy, loving spirit, and, as Mr. Ruskin would say, the "*fineness of nature*" he possessed.

Another striking characteristic of Faraday was the quiet and unostentatious manner in which he did everything. His style of lecturing was so simple and easy as to appear effortless. This apparent absence of effort is, however, the climax of self-culture. A man only does that perfectly well which he does without the evidence of exertion. Faraday had reached this point through the mental discipline to which he habitually subjected himself. He translated into practice the words in which he expresses his own "strong belief that that point of self-education which consists in teaching the mind to resist its desires and inclinations until they are proved to be right, is the most important of all, not only in affairs of natural philosophy, but in every department of life."

With all this stern reality there was also a fine poetic fancy in Faraday. To him the Universe was no machine. His was "a face-to-face, heart-to-heart, inspection of things," and this, as Carlyle says, is "the first characteristic of all good thought in all times." Scientific phraseology never hid from him the grandeur and mystery that at bottom lies in everything. A thunder-storm was to him no mere affair of positive and negative electricity, no mere discharge of electric potential, but something infinitely beyond all this—"a window through which he looked into Infinitude itself." Dr. Gladstone tells us "he would stand gazing at the lightning, a stranger to fear, with his mind full of lofty thoughts, or perhaps of high communings. Sometimes, too, if the storm were at a little distance, he would summon a cab, and in spite of the pelting rain, drive to the scene of awful beauty." A friend thus met him once at Eastbourne, "in the thick of a tremendous storm, rubbing his hands with delight because he had been fortunate enough to see the lightning strike the church tower."

We are told that a new fact "seemed to charge him with an energy that gleamed through his eyes and quivered through his limbs." The writer remembers an illustration of this, when Dr. Tyndall brought Mr. Faraday into the laboratory to look at his new discovery of calorescence. As Faraday saw for the first time a piece of cold, black platinum raised to a dazzling brightness when held in the focus of dark rays, a point undistinguishable from the air around, he looked on attentively, putting on his spectacles to observe more carefully, then ascertained the conditions of the experiment, and repeated it for himself; and now quite satisfied he turned with emotion to Dr. Tyndall and almost hugged him with pleasure. And so on another occasion, when Prof. Plücker was showing in the laboratory some lovely experiments with vacuum tubes, Faraday literally danced with delight round the electric discharge, exclaiming, as he gazed at the moving arches of light, "Oh! to live always in it!"

We have said enough. Our readers will forgive us for recalling a character which many of them know far better than the writer. But we are never tired of contemplating a nature so singularly beautiful as that of Faraday. Whether we think of him as pourtrayed by Dr. Bence Jones, Dr. Tyndall, or Dr. Gladstone, or by Prof. Helmholtz, Dumas, or De la Rive, the picture is still the same;—

> "His life was gentle; and the elements
> So mix'd in him, that Nature might stand up
> And say to all the world, 'This was a Man!'"

W. F. BARRETT

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

American Stone Arrowheads

I AM glad to perceive that Dr. Abbott has called attention to the variations in form among North American stone arrowheads, although he cannot admit the correctness of some of the remarks I have incidentally made on this subject in my "Ancient Stone Implements of Great Britain." At the time when that work went to press, I was not in possession of Dr. Abbott's "Stone Age in New Jersey," if, indeed, it had appeared, and in describing the various North American forms, for want of other authorities I principally referred to Schoolcraft, Squier and Davis, and Prof. Daniel Wilson, some of whom Dr. Abbott appears to consider as somewhat antiquated.

It is not to be supposed that my acquaintance with the collections in the United States should in any way approach that of Dr. Abbott, as I have never visited America. My views as to the prevalence of different types were, however, mainly founded, not on printed descriptions, but on actual specimens preserved in collections in this country, my own among the number; and with all deference to Dr. Abbott, who has kindly promised to send me the specimens on which he bases his remarks, I think it will be found that his views, even if completely applicable to New Jersey, will hardly hold good throughout the northern part of the United States and Canada. The principal points on which he disagrees with me are—

1. As to the arrowheads with a notch at the base on either side constituting a prevailing type in North America.
2. As to the leaf-shaped form being very rare.
3. As to the chipping being for the most part but rough, as compared with that exhibited on the arrowheads found in Britain.

1. As to the first point, Dr. Abbott does not appear to appreciate the difference between "a" and "the;" and though it is possible that other forms of arrowheads are found in America in equal numbers with those such as I have mentioned, yet I think he would agree with me, if he were comparing a series, from North America with one from some other country, in recognising those with a notch at the base on either side as offering one of the prevailing types.

2. As to the leaf-shaped form being very rare, I may observe that among the thirty-eight figures of arrowheads given by Dr. Abbott in his "Stone Age in New Jersey" only one is leaf-shaped, though he mentions the plain leaf-shaped form as the prevailing variety of the leaf-shaped arrowhead and its modifications. In my own collection, comprising upwards of 160 North American arrowheads, there are only six that can be termed leaf-shaped, and but two of these are really typical specimens of the form. These two have come into my possession since my book was written. If not "very rare" I think the form must be regarded as comparatively rare.

3. Lastly, as regards workmanship, Dr. Abbott seems not to have observed that my statement as to the roughness of chipping was qualified with the words "for the most part." Anyone comparing a collection of stone arrowheads from North America with one formed in Britain or Ireland would, I think, at once admit that, as a whole, the latter were more finely and delicately chipped. Or, again, comparing Dr. Abbott's illustrations with mine the same general conclusion will be attained. Even the most elegantly formed American specimens that I have seen cannot compete in delicacy of workmanship with some of the English examples, such, for instance, as those represented in my Figures 317, 318, 319. I can only say that if Dr. Abbott or any other American collector will kindly send me some specimens equally well-chipped, I shall be proud to add them to my collection, and not too proud to acknowledge any errors into which I may have been betrayed through insufficiency of knowledge.

JOHN EVANS

Nash Mills, Hemel Hempsted, Sept. 14

Botanical Terminology

BOTANY is now being taught to large numbers in public and middle-class schools; its admission as a subject of general study can only be justified by its high educational value, not so much as an end, but as a means. Many of the boys who work at it will have no time to continue the study in detail, and the little they know of it will be what they learn at school.

Upon all these grounds it is desirable that there should be no unnecessary or conventional impediments placed in the way of as much education as possible being given in the short time allotted to it. I am desirous of eliciting an opinion as to whether the botanical terminology is or is not an impediment, diminishing the educational value and restricting the scope of the subject. For some years at Rugby I have felt myself hampered with a weariness of names. Is a terminology as polyglot in its derivation as the tower of Babel, and often involving questionable hypotheses of function, or incorrect ideas of morphology, the best instrument for making young pupils observe accurately or reason accurately on observed results? And yet a teacher cannot select the terms he will use, (1) because the books, and especially the English and local floras, imply in their readers a knowledge of a wide terminology; and (2) because, as in the fight against Euclid, so here, the teacher is subject to the examiner, and at present the examination papers of the Science and Art Department, Cambridge Local, &c., are incomprehensible to a boy not bristling with terms.

Allow me specially to call attention to some of what appear to me unnecessary faults in the terms used, and to suggest one or two remedies. The names chosen to denote parts are mainly derived from Latin or Greek, or both, or have no received derivation. Well, even supposing that the words do, by their derivation from the dead language, suggest to those who know it some clue to their meaning, yet how few boys can keep these derivations in their heads, especially in schools in which Latin is not much and Greek not at all taught. Why on earth, when the promoters of science are driving out Greek to let in science, should a knowledge of Greek be a necessary "open sesame" to the correct remembering and spelling of botanical terms?

But the above supposition is seldom correct, the words when translated expressing either a fanciful resemblance, or some pre-adamite stage of botanical knowledge. If the opening through which the pollen-tube passes is already called "little gate," perhaps we must submit to the name, but why call it "micropyle."?

And here perhaps is the place to make a protest against such words as were flooded into the science at the time when the sexuality of plants was first established.

The usefulness of insisting on the generalisation that gamogenesis was common to plants and animals only exists in teaching those who know what gamogenesis in animals means. Does that usefulness exist in the case of boys and girls who learn botany.? In teaching Human Physiology, some subjects are omitted and rightly omitted, from such text-books as Huxley's Elementary Physiology; why then should a knowledge of such subjects be assumed in our botanical terms? The facts of botany can all be taught, as far as my experience of boys and girls goes, without the slightest difficulty; the generalisation will follow of itself, as soon as it is wanted; I only object to its being gratuitously, and almost pruriently, forced on the attention. Who can approve of the use in our elementary floras of the term "hermaphrodite," or the same symbols for pistillate and staminate plants being used which the boys are taught in their astronomy as the recognised symbols for Venus and Mars? To this class belong the tribe of words ending in *—androus* and *—gynous*, which must either be taught as unintelligible gibberish, or explained on gamogenetic analogy. I do not wish to be misunderstood: if you are teaching the physiological subjects in animals, let "honi soit" be your motto, and call a spade a spade; there are worse evils than we dream of arising from obscurantism; but if you are teaching botany, keep to your subject, and do not go out of your way to call a club a spade.

Lastly, many words are utterly confusing: either they seem by their jingling similarity to make the learner and indeed even older botanists sure to confuse them, as *loculicidal* and *septicidal* and the troop of words which end in *—tropous*; or they convey a morphologically impossible idea, as *inferior* and *superior* as applied to the ovary. To see how these faults can be avoided, let us inquire why an unusual amount of names are required at all.

(I.) Popular names being vaguely used require to be restricted in their meaning by accurate definition.

(II.) A new name is required for any part to which no name is popularly assigned, either because the thing to be named escapes popular observation, or because two or more things are included in the connotation of the popular term.

(III.) New adjectives, or adjectival-periphrases, are required to express characteristics, or relations of part to part.

Let me briefly suggest some principles, which, while remedying the faults of the old terminology, seem not to clash with these three necessities of the subject.

(*a*) Names for new things to be given in English, *ex. gr.*, the names *calyx* and *corolla* to be taught as *cup* and *crown*: in this we should be only following the German use of Kelch and Krone.

(*b*) Where a part of a thing already named requires a fresh name, the preference to be given to a name framed like the German double words—Kelch-blatt, Staub-blatt—so as to indicate the relation of part to part, thus *cup-leaf*, *leaf-stalk*, *flower-stalk* to be taught instead of *sepal*, *petiole*, and *peduncle.*

(*c*) Short expressions involving English (not Greek) prepositions to be used for adjectives: thus *splitting by mid-ribs*, *on seed-vessel*, *united by dust-pouches*, to be used for *loculicidal*, *epigynous*, *syngenesious.*

(*d*) Where the definitions of the terms is given in numbers, numbers or fractions be used instead of those terms: thus in æstivation, $\frac{2}{5}$ to be used for quincuncial; in cutting of leaves the fraction of the leaf cut to be stated instead of *omnia quæ exeunt*, *in-fid*, *-sect*, and *-partite.*

But it will be said—how will pupils taught thus get on afterwards? The answer is, either they will do no more of the subject than they do at school, in which case they will have got the idea without the obstructions of the terms; or they will care to go on further with the subject, in which case they will learn the terms very quickly, being now familiar with the facts and ideas. In neither case will time have been lost, and the scope of botanical subjects which may be treated in the time will have been doubled.

I must apologise for the length to which this letter has run.

FRANK E. KITCHENER

Rugby, September 16

Hutton's Trigonometrical Tables, for Arcs expressed as portions of the Radius

AT the end of the preface of the first edition of Hutton's Mathematical Tables (1785) is the following postscript:—"P.S.—Since my History of Trigonometrical Tables in the following Introduction was printed, there has been published in the 'Philosophical Transactions' for the year 1784, a paper of mine concerning a project for the trigonometrical tables to be constructed on a new plan, namely, in which the arc of the quadrant is divided into aliquot parts of the radius, or according to the real lengths of the arcs, which construction is now in some degree of forwardness, as myself and several assistants have been closely engaged in the execution of it ever since." And in the succeeding editions, down to the sixth, 1822, there occurs on p. 2 of the Introduction the following remark:—"But the complete reformation would be to express all arcs by their real lengths, namely, in equal parts of the radius decimally divided, according to which method I have nearly completed a table of sines and tangents." Hutton died in 1823, and I can find no further reference to the table in question. I feel pretty certain that it has never been published, and there is no other paper on the same subject (except that in the Phil. Trans., 1784) of Hutton's referred to in Watt's "Bibliotheca" or the Royal Society's Catalogue.

The table was intended to give the sines, tangents, &c., of $\frac{1}{1000000}$, $\frac{2}{1000000}$, &c. (the unit being the arc equal to radius) to seven decimal places, and would be very useful. If it has not been published, perhaps some reader of NATURE might be able to say what has become of the manuscript that was nearly completed.

I may mention that the calculation of such a table was under the consideration of the Tables Committee of the British Association, but it was thought that some other tables were at present more urgently needed.

J. W. L. GLAISHER

Cambridge, Sept. 16

THE "HASSLER" EXPEDITION

WE are indebted to the courtesy of the Editor of the *New York Tribune* for early communication of the following information from Prof. Agassiz's expedition:—

OFF GUATEMALA, July 29, 1872

To Prof. Benjamin Peirce, Superintendent U.S. Coast Survey.

MY DEAR PEIRCE:—Do not be surprised at my few messages. It is about all I can do to take advantage of every opportunity that offers for study and collecting; but I rarely feel sufficiently collected to do any connected writing. I have another new chapter concerning glacial phenomena, gathered during our land journey from Talcahuano to Santiago, but it is so complicated a story that I do not feel equal now to recording the details in a connected statement, while the whole may be put in a few words.

There is a broad valley between the Andes and the coast range, the Valley of Chillan extending from the Gulf of Ancud, or Port Montt, to Santiago, and farther north. This valley is a continuation, upon somewhat higher level, of the channels which, from the Strait of Magellan to Chiloe, separate the islands from the main land, with the sole interruption of Tres Montes, which gives the clue to the whole, as we have here in miniature a valley between the Andes and the coast range. Now this great valley, extending for more than 25 degrees of latitude, is a continuous glacier bottom, showing plainly that for its whole length the great southern ice-sheet has been moving northwards in it. I could find nowhere any indication that glaciers descending from the Andes had crossed this valley and reached the shores of the Pacific. In a few localities only did I notice Andean, *i.e.*, volcanic erratics upon the loose materials filling the old glacier bottom. Between Currillo and Santiago, however, facing the gorge of Tenon, I saw two distinct lateral moraines, parallel to one another, chiefly composed of volcanic boulders, resting upon the old drift, and indicating by their position the course of a large glacier that once poured down from the Andes of Tenon, and crossed the main valley, without, however, extending beyond the eastern slope of the coast range. These moraines are so well marked that they are known throughout the country as the Cerillos of Tenon; but nobody suspects their glacial origin; even the geologists of Santiago assign a volcanic origin to them.

What is difficult to describe in this history are the successive retrograde steps of the great southern ice-field, that, step by step, left to the north of it larger or smaller tracks of the valley free of ice, so that large glacial lakes could be formed, and, in fact, seem always to have existed along the retreating edge of the great southern glacier. The natural consequence is that there are everywhere stratified terraces, without border barriers (as these were formerly the ice that has vanished), resting at successively higher or lower levels, as you move north or south, upon unstratified drift of older date, the northermost end of these terraces being the oldest, while those farther south belong to the latter steps in the waning of the ice-fields. From these data I infer that my suggestion concerning the trend of the striæ upon the polished and glaciated surfaces of the vicinity of Talcahuano, alluded to in the postcript of my last letter, is probably correct.

I was much grieved on reaching Valparaiso to hear of the mishaps of the dredging apparatus. The subsequent departure of Pourtales has been a great loss to us all, for notwithstanding his silent nature, he is a powerful standby.

Our visit to the Galapagos has been full of geological and zoological interest. It is exceedingly impressive to see an extensive archipelago, of most recent origin, inhabited by creatures so different from any known in other parts of the world. Here we have a positive limit to the length of time that may be granted for the transformation of these animals, if they are in any way derived from others dwelling in different parts of the world. The Galapagos are so recent that some of these islands are barely covered with the most scanty vegetation, itself peculiar to these islands; some parts of their surface are entirely bare, and a great many of the craters and lava streams are so fresh that the atmospheric agents have not yet made an impression upon them. Their agent does not, therefore, go back to earlier geological periods; they belong to our time, geologically speaking. Whence then do their inhabitants come from—animals as well as plants? If descended from some other type, belonging to some neighbouring land, then it does not require such unspeakably long periods for the transformation of species as the modern advocates of transmutation claim; and the mystery of change, with such marked and characteristic differences between existing species, is only increased and brought to a level with that of creation. If they are autochthones, from what germs did they start into existence? I think that careful observers, in view of these facts, will have to acknowledge that our science is not yet ripe for a fair discussion of the origin of organised beings.

Our stay in Panama has allowed us to make very extensive collections in the Bay and across the Isthmus. I was surprised to find so little difference in the character of the flora and of the terrestrial fauna between the two oceans. Marked peculiarities are only to be found among the marine animals, and even among them the American character of the Atlantic and Pacific marine fauna is unmistakable; we are not surrounded by animals recalling by their peculiarities the many groups of islands of the Pacific. I expect that our visit in Acapulco will confirm these impressions. L. AGASSIZ

CAPTAIN HALL'S ARCTIC EXPEDITION

THE *Washington Chronicle* of August 26 contains the following interesting account of the progress and position of this important expedition:—"The Navy Department has received later despatches from Captain Hall, by the way of Tydskland and Copenhagen, completing his official record up to the moment of final departure from North Greenland. These despatches, which are quite full, bear date off Tossak, Tussuissuk, N. lat. 73° 21′, W. long. 56° 5′, August 24, 1871, and are, therefore, only four days later than Hall's Upper Navik despatch, August 20, 1871, which reached the department within three months by the way of Copenhagen. The explanation of this long delay *in transitu* is that there is no regular communication between Denmark and these far-off colonies but once a year. Hall's Upper Navik despatches were timed to reach the Danish brig just then sailing, and this present letter sent back by native pilots, as he notes in concluding, may have had near a year's detention in Disco. It seems to have reached the American Minister at Copenhagen about July 30. Although thus divested of any special value as news, the present despatch is of much intrinsic interest. All on board the *Polaris*, officers, scientific corps, and men, were well and in excellent spirits. The seagoing qualities of the vessel had been tested and found admirable; the engines and machinery were in perfect working order, coal and rosin in good supply, and the ship's crew abundantly provisioned. For the long Arctic night before them they had books, games, instrumental music, &c.—in a word, everything that the thoughtful care of the department could supply, or letters of credit at Newfoundland and in Greenland furnish, had been laid in to complete their outfit, and of all this Captain Hall makes characteristic and thankful acknowledgments. Governor Elberg, of the Navik district, had accompanied the *Polaris* as far as Tossak, the extremest northerly limits of Danish jurisdiction as well as of civilised life, and was to the last moment assiduous in his exertions to further the interests of the expedition. Mainly through his co-operation Hall was fortunate enough at Tossak to make up his complement of Esquimaux dogs—sixty strong, healthy animals—a matter of almost vital importance. He likewise laid in a large supply of dog food, and considerably augmented his stock of reindeer-furs, sealskins, &c., for the adventurous voyage. At Upper Navik the expedition had shipped Hans Christian, a famous native hunter and dog-driver, with his wife and three children. Jensen, the Dane, who was under promise to join the expedition at Tossak, backed out at the last moment. Governor Elberg, of whose many kindnesses Hall speaks with full heart, awaited at Tossak the return of the native pilots, bearing this despatch to him, and it closes with the prow of the *Polaris* northward in the early morning of August 24, with a complete roster of all on board, thirty-three souls, and a fervent, hopeful prayer for success. It will be remembered that Captain Hall's previous despatches speak of his good fortune in meeting at Holsteinburg the returning Swedish expedition, and that the commander, Baron van Otter, kindly furnished him copies of log, deep-sea soundings, &c., assuring him that the season was more than usually favourable, and extremely wide iceberg-channels, &c. Of the same purport was the information received of Governor Rodolph, thirty years resident in North Greenland, who declared the year to be more favourable for any northern voyage than many years agone or to come. Acting on this information, and under discretionary power vested in him by the Navy Department, Captain Hall had abandoned the Jones's Sound route, and had decided before he left Upper Navik that after stopping at Tossak he would cross Melville Bay to Cape Dudley Digges, and from that point steam direct to Smith's Sound, thence make all possible attempts to find a passage on the west side of the Sound from Cape Isabella up to Kennedy Channels, wintering there probably in about the same latitude or a little higher than Kane's winter quarters, and thence on and up to the North Pole. The letter published in the *New York Times*, April 25, purporting to narrate a disaster to the *Polaris* and her return last February to Disco, was a *canard*. Not one word of it has ever been credited at the Navy Department. It is not believed that any disaster has overtaken the Expedition, or that any ground for apprehension exists."

THE BLIND FISHES OF THE MAMMOTH CAVE AND THEIR ALLIES[*]

THE *Amblyopsis spelæus* undoubtedly has quite an extensive distribution, probably existing in all the subterranean rivers that flow through the great limestone region underlying the Carboniferous rocks in the central portion of the United States. Prof. Cope obtained specimens from the Wyandotte Cave and from wells in its vicinity, and in the Museum of Comparative Zoology at Cambridge there is a specimen labelled "from a well near Lost River, Orange Co., Ind.," which, with those from the Wyandotte Cave, is conclusive evidence of its being found on the northern side of the Ohio[†] as well as on the southern, in the rivers of the Mammoth Cave. I have been able to examine a number of specimens from the Mammoth Cave, and have carefully compared with them the one from the well in Orange Co., Ind., and find that the specific characters are remarkably constant.

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[*] Reprinted from the *American Naturalist*, a sequel to "The Blind Crustacea of the Mammoth Cave." See NATURE, vol. v pp 445, 484

[†] I have also been informed by Mr Holmes of Lansing, Mich, that *blind fishes* have been drawn out of wells in Michigan.

In 1859[*] Dr. Girard described a blind fish, received by the Smithsonian Institution from J. E. Younglove, Esq., who obtained it "from a well near Bowling Green, Ky." The general appearance of this fish, which was only one and a half inch in length, was that of *Amblyopsis spelæus*, but it differed from that species in several characters, especially by the *absence* of ventral fins. Dr. Girard therefore referred the fish to a distinct genus under the name of *Typhlichthys*[†] *subterraneus*. Dr. Günther[‡] considers this fish a variety of *Amblyopsis spelæus*, and records the specimen in the British Museum "from the Mammoth Cave," as half-grown.[§]

By the kindness of Prof. Agassiz, I have been enabled to examine nine specimens of *blind fish without ventrals*, in the Museum of Comparative Zoology. Seven of these were collected in the Mammoth Cave by Mr. Alpheus Hyatt, in September 1859. One was from Moulton, Lawrence County, Alabama, presented by Mr. Thomas Peters; and another from Lebanon, Wilson Co., Tennessee, presented by Mr. J. M. Safford. It is not stated whether these latter came from wells or caves, but probably from wells. They are all of about one size, one and one-half to two inches in length, and are constant in their characters. Moreover, four of the seven specimens from the Mammoth Cave were females with eggs. These eggs were as large in proportion as those from *Amblyopsis*. The ovary was single, and situated on the right side of the stomach, as in *Amblyopsis*. The difference in the number of eggs was very remarkable, each of the four specimens examined having but about thirty eggs in the ovary, while in three females of *Amblyopsis* (all, however, of nearly three times the size of *Typhlichthys*) there were about one hundred eggs in each. As in both species there were no signs of the embryos in the eggs, it is not probable that any of the eggs had been developed and the young excluded, nor is it at all likely that the great variation in the number of eggs would simply indicate different ages. For these reasons, taken in connection with the absence of ventral fins, I have no hesitation in accepting Dr. Girard's name as valid for this genus, of which we thus far know of but one species, with a subterranean range from the waters of the Mammoth Cave, south, to the northern portion of Alabama. In this connection it would be most interesting to know the relations of the "blind fishes" said to have been found in Michigan. For thus far we have *Typhlichthys* limited to the central and southern portion of the subterranean region, *Amblyopsis* to the central, and the species in the northern portion undetermined.

In 1853, on his return from a tour through the southern and western states, Prof. Agassiz gave a summary of some of his ichthyological discoveries in a letter to Prof. J. D. Dana.[‖] In this letter are the following remarks:—

"I would mention foremost a new genus which I shall call *Chologaster*, very similar in general appearance to the blind fish of the Mammoth Cave, though provided with eyes; it has, like *Amblyopsis*, the anal aperture far advanced under the throat, but is entirely deprived of ventral fins; a very strange and unexpected combination of characters. I know but one species, *Ch. cornutus* Ag. It is a small fish scarcely three inches long, living in the ditches of the rice fields in South Carolina. I derive its specific name from the singular form of the snout, which has two horn-like projections above."

This is the only information ever published regarding this interesting fish, and the only specimens known are those on which Prof. Agassiz based the above remarks.

The only specimen known of this second species was drawn from a well in Lebanon, Tenn., and presented to the Museum by Mr. J. M. Safford, Jan. 1854. It is a more slender fish than *C. cornutus*, but the intestine follows the same course, and the four pyloric appendages are present as in that species.

In the genus *Chologaster*[†] we have all the family characters as well expressed as in the blind species, though it differs from *Amblyopsis* and *Typhlichthys* by the presence of eyes, the absence of papillary ridges on the head and body, and by the longer intestine and double the number of pyloric appendages, as well as by the position of the ovary; and agrees with *Typhlichthys* by the absence of ventral fins. *Amblyopsis* and *Typhlichthys* are nearly colourless, while *Chologaster Agassizii* is of a brownish colour, similar to many of the minnows, and *C. cornutus* is brownish yellow, with dark, longitudinal bands.

Among the most interesting points in the history of this genus is the fact of its occurring in two widely different localities, *C. Agassizii* having been found in a well in the same vicinity (probably in the same well) with a specimen of *Typhlichthys*, and undoubtedly belonging to the same subterranean fauna west of the Appalachian ridge, while *C. cornutus* belongs to the southern coast fauna of the eastern side of that mountain chain, and is thus far the only species of the family known beyond the limits of the great subterranean region of the United States.

Having now given an outline of the structure, habits, and distribution of the four species belonging to the family and recapitulated the known facts, we are better able to consider the bearings of the peculiar adaptation of the blind fishes, in the Mammoth and other caves, to the circumstances under which they exist.

Prof. Cope, in stating, in his account of the blind fish of the Wyandotte Cave, "that the projecting under jaw and upward direction of the mouth renders it easy for the fish to feed at the surface of the water, where it must obtain much of its food," suggests that:—

"This structure also probably explains the fact of its being the sole representative of the fishes in subterranean waters. No doubt many other forms were carried into the caverns since the waters first found their way there, but most of them were like those of our present rivers, deep water or bottom feeders. Such fishes would starve in a cave river, where much of the food is carried to them on the surface of the stream. . . . . . The shore minnows are their nearest allies, and many of them have the upturned mouth and flat head. . . . . . Fishes of this, or a similar family, enclosed in subterranean waters years ago, would be more likely to live than those of the other, and the darkness would be very apt to be the cause of the atrophy of the organs of sight seen in the *Amblyopsis*."

This suggestion was undoubtedly hastily made by Prof. Cope when writing the letter which was printed in the *Indianapolis Journal*, and were it not that the article has been reprinted in the "Annals and Magazine of Natural History," I should not criticise the statement made in an off-hand letter for publication in a newspaper; for with Prof. Cope's knowledge of fishes it could simply be a hasty thought which he put on paper, when he suggests that it is because the *Cyprinodontes* have a mouth directed upwards and are surface feeders that they were better adapted to a subterranean life than other fishes, and hence maintained an existence, while other species, which he supposes were introduced into the subterranean streams at the same time, died out.

If the fishes of the subterranean streams came from adjoining rivers, why were not many of the Percoids, Cyprinoids, and other forms, that are as essentially surface feeders as the *Cyprinodontes* (many of the latter are purely "mud feeders"), as capable of maintaining an existence in the subterranean waters as any species of the latter? Neither is it necessary for us to assume that the structure of the fish should be adapted to feeding on the surface, for not only have we in the blind cat fish described by Prof. Cope himself from the subterranean stream in Pennsylvania, an example of a fish belonging to an entirely different family of bottom feeders thriving under subterranean conditions, but the blind fishes of the Cuban caves are of the great group of cod fishes which are, with hardly an exception, bottom feeders. The fact that the food of the blind fishes of the Mammoth Cave consists in great part of the cray fish found in the waters of the cave, as shown by the contents of several stomachs I have examined, and also that one blind fish at least made a good meal of another fish, as already mentioned, shows that they are not content with waiting for what is brought to them on the surface of the water, and that they are probably as much bottom as surface feeders.

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[*] Proceedings Acad. Nat. Sci. Philad., p. 63.

[†] Blind fish.

[‡] Catalogue of Fishes in the British Museum, vol. vii. p. 2, 1868

[§] The largest specimen I have seen of *Typhlichthys* is one and seventeen-twentieths inches in length, and the smallest *Amblyopsis* one and eighteen-twentieths inches.

[‖] Published in American Journal of Science and Arts, vol. xvi. (2d series) p. 134, 1853.

[*] Literally "bile-stomach;" probably named from the yellow colour of the fish.

Again, in regard to sense of sight, why is it necessary to assume that because fishes are living in streams where there is little or no light, that it is the cause of the non-development of the eye and the development of other parts and organs? If this be the cause, how is it that the *Chologaster* from the well in Tennessee, or the "mud fish" of the Mammoth Cave, are found with eyes? Why should not the same cause make them blind if it made the *Amblyopsis* and *Typhlichthys* blind? Is not the fact, pointed out by Prof. Wyman, that the optic lobes are as well developed in *Amblyopsis* as in allied fishes with perfect eyes, and, I may add, as well developed as those of *Chologaster cornutus*, an argument in favour of the theory that the fishes were always blind, and that they have not become so from the circumstances under which they exist? If the latter were the case, and fishes have become blind from the want of use of the eyes, why are not the optic lobes also atrophied, as is known to be the case when other animals lose their sight? I know that many will answer at once that *Amblyopsis* and *Typhlichthys* have gone on further in the development and retardation of the characters best adapting them to their subterranean life, and that *Chologaster* is a very interesting transitionary form between the open water *Cyprinodontes* and the subterranean blind fishes. But is not this assumption answered by the fact that *Chologaster* has every character necessary to place it in the same family with *Amblyopsis* and *Typhlichthys*, while it is as distinctly and widely removed from the *Cyprinodontes* as are the two blind genera mentioned?

If it is by acceleration and retardation of characters that the *Heteropygii* have been developed from the *Cyprinodontes*, we have indeed a most startling and sudden change of the nervous system. In all fishes the fifth pair of nerves send branches to the various parts of the head, but in the blind fishes these branches are developed in a most wonderful manner, while their subdivisions take new courses and are brought *through* the skin, and their free ends become protected by fleshy papillæ, so as to answer, by their delicate sense of touch, for the absence of sight. At the same time the principle of retardation must have been at work and checked the development of the optic nerve and the eye (which probably exists externally in the young fish), while acceleration has caused other portions of the head to grow and cover over the retarded eye.

Now, if this was the mode by which blindness was brought about, and tactile sense substituted, why is it that we still have *Chologaster Agassizii* in the same waters, living under the same conditions, but with no signs of any such change in its senses of sight and touch? It may be said that the *Chologaster* did not change because it probably had a chance to swim in open waters, and therefore the eyes were of use, and did not become atrophied. We can only answer, that if the *Chologaster* had a chance for open water, so had the *Typhlichthys*, and yet that is blind.

If the *Heteropygii* have been developed from *Cyprinodontes*, how can we account for the whole intestinal canal becoming so singularly modified; and what is there in the difference of food or of life that would bring about the change in the intestine, stomach, and pyloric appendages, existing between *Chologaster* and *Typhlichthys* in the same waters? To assume that under the same conditions one fish will change in all these parts and another remain intact, by the blind action of uncontrolled natural law, is, to me, an assumption at variation with facts as I understand them.

Looking at the case from the standpoint which the facts force me to take, it seems to me far more in accordance with the laws of nature, as I interpret them, to go back to the time when the region now occupied by the subterranean streams was a salt and brackish water estuary, inhabited by marine forms, including the brackish water forms of the *Cyprinodontes* and their allies (but not descendants) the *Heteropygii*. The families and genera having the characters they now exhibit, but most likely more numerously represented than now, many probably became exterminated as the salt waters of the basin gradually became brackish and more limited, as the bottom of this basin was gradually elevated; and finally, as the waters became confined to still narrower limits, and changed from salt to brackish, and from brackish to fresh, only such species would continue as could survive the change, and they were of the minnow type represented by the *Heteropygii*, and perhaps some other genera of brackish water forms that we have not yet discovered.

In support of this hypothesis we have one species of the family, *Chologaster cornutus*, now living in the ditches of the rice fields of South Carolina, under very similar conditions to those under which others of the family may have lived in long preceding geological times; and to prove that the development of the family was not brought about by the subterranean conditions under which some of the species now live, we have the one with eyes living with the one without, and the South Carolina species to show that a subterranean life is not essential to the development of the singular characters which the family possess.

That a salt or brackish water fish would be most likely to be the kind that would contine to exist in the subterranean streams, is probable from the fact that in all limestone formations caves are quite common, and would in most instances be occupied first with salt water then with brackish, and finally with fresh water so thoroughly impregnated with lime as to render it probable that brackish water species might easily adapt themselves to the change, while a pure fresh water species might not relish the solution of lime any more than the solution of salt; and we know how few fishes there are that can live for even an hour on being changed from fresh to salt, or salt to fresh water. We have also the case of the Cuban blind fishes belonging to genera with their nearest representative in the family a marine form, and with the whole family of cods and their allies, to which group they belong, essentially marine. Further than this, the cat fish from the subterranean stream in Pennsylvania belongs to a family having many marine and brackish water representatives. As another very interesting fact in favour of the theory that the *Heteropygii* were formerly of brackish water, we have the important discovery by Prof. Cope of the Lernæan parasite on a specimen of *Amblyopsis* from the Wyandotte cave; this genus of parasite crustaceans being very common on marine and migratory fishes, and much less abundant on fresh water species.

Thus I think we have as good reasons for the belief in the immutability and early origin of the species of the family of *Heteropygii*, as we have for their mutability and late development, and, to one of my, perhaps, too deeply rooted ideas, a far more satisfactory theory; for, with our present knowledge, it is but theory on either side.

F. W. PUTNAM

PASTEUR'S NEW PROCESS FOR THE MANUFACTURE OF BEER*

DURING the last few years several improvements have been introduced into the manufacture of beer; in Germany especially, land of sauer-kraut and hops, much attention has been devoted to this spicy and delicate question. In general, however, the apparatus for this manufacture leaves much to desire, and to this day brewers are far from being in possession of a process anything like perfect. Scientific men have given scarcely any attention to the question, which, perhaps, they consider unworthy of their study; while, like the rest of mortals, overcome by the heat and burden of the day, they perhaps have not disdained at times to raise to their lips the golden beverage, which even the most dainty prefer to the ancient and divine nectar.

Happily, we have commenced to review our past errors; the great masters of science have consented to descend from the ethereal heights of their empire, and to bring to bear upon industrial art their precious co-operation. For my part, I prefer a result to a formula, an application to a cloudy theory. Should some, losing themselves in impenetrable mists, turn away their attention with alarm from the useful pursuits of these men of science who approach with satisfaction the vulgar necessities of this world, let them do so, so long as M. Liebig is willing to be the world's cook, and M. Pasteur its brewer! It is an established fact in the eyes of earnest men, that "science," as Flourens said, "ought not merely to remain a source of self-satisfaction to the soul who has acquired it, and been elevated by it above the crowd, and that it becomes false as soon as it ceases to be profitable to that crowd." Dunoyer said with justice that "he who is a man of science, and nothing more, knows only how to make science useless to himself."

M. Pasteur is one of those men essentially scientific and keensighted, who understand the saying Geoffroy St. Hilaire one day made to re-echo through the Institute, "The social question is the first with which to occupy ourselves at the present day." But it was in that same room that Bert exclaimed, "Let us continue to study Nature in her most secret processes, to discover, to measure, to calculate the forces that she brings into action, having no thought of any useful applications we can put our studies to." Without doubt, with his friend Gay-Lussac, the great mathematician would reproach Pellegrini-Savigny with having degraded Science in occupying himself with questions as to alimentation. But on the other side we have the names of Berthollet, Monge, Chaptal, Conté, d'Alembert, Franklin, Geoffroy St. Hilaire, Arago, and especially Pasteur, for whom posterity will undoubtedly have a profound respect. M. Pasteur has added to his other fruitful inventions a new process for improving the manufacture of beer, for which he asked a patent in June 1871 in the following terms :—

"I desire to take out a patent for five years for a new mode of manufacturing beer, which consists essentially in fermentation sheltered from all contact with air.

"The wort, after being boiled, is turned into vessels of wood or iron, cooled in a current of carbonic acid gas; and then put to ferment.

"This process is founded upon new scientific data which I have expounded elsewhere, and from which it follows that contact with air is most injurious to the manufacture of beer. It is applicable to mild and strong, brown or pale ales.

"I wish the beers made by my process to bear in France the name of *Bières de la Revanche Nationale*—I shall say why elsewhere—and abroad that of *Bières françaises*.

"The disuse of cooling *bacs*.

"No loss by evaporation, &c.

"The abolition of store cellars, for future use.

"An increase of the quantity produced, and, at the same time, an increase in the strength of the beer.

"The development of a very agreeable 'bouquet.'

"No further use for ice-houses, for fermentation at a low temperature.

"Such are some of the principal advantages to be derived from the application of my process, and some of the qualities that will mark the beers *de la Revanche*."

On Nov. 4, 1871, an additional certificate, accompanied by the sketch here reproduced, was deposited at the Prefecture of the Department of the Seine.

"The annexed sketch," says M. Pasteur in this addition, "represents the construction of the apparatus for the application of the new process of beer manufacture. A description of the design accompanies it.

"The size of this apparatus varies with the importance of the manufacture. It can be made of any size, from one hectolitre to 100,000 hectolitres. When the apparatus is of a somewhat considerable size, the employment of carbonic gas is indispensable to prevent the formation of deleterious ferments—lactic ferment, butyric ferment, injurious alcoholic ferment, &c. The employment of air, previously purified by calcination, by passing it through cotton, or by any other mode, is also a remedy for this drawback. A very small quantity of air is not injurious, perhaps even ultimately improves the quality of the beer. The beer can absorb all the flavour which malt and hops can give it; it can acquire a thoroughly free taste, sparkling limpidity, great strength, and in general all its characteristic qualities, only if we can totally, or in a very large measure, suppress the combustion which takes place in the ordinary process.

"We can also fill up the void which is made in the apparatus during the cooling of the wort, by bringing the apparatus into connection with a vessel full of warm wort, to kill the seeds of disease in the wort and in the beer which is the ultimate result.

"A man-hole is necessary at those parts of the apparatus marked F, F, when they are of great size.

"The cylinders should be surrounded with a muffler of flannel."

On November 25, 1871, M. Pasteur added a new improvement to his invention.

"The facility with which my apparatus may be applied will be increased by the employment of carbonic acid gas, after a more convenient fashion.

"The carbonic gas produced during fermentation, after having passed through a washing-flask, to be freed from any froth it might hold, is delivered into a zinc or tin reservoir, placed a little below the fermenting apparatus. At the lower part of this reservoir are arranged a number of sockets or taps. When it is desired to cool the wort in presence of the carbonic acid, as it is useless to mix up this gas with the liquid, it is sufficient to put one of the taps of the reservoir in communication with one of the tubes of the apparatus F, F, F'. This arrangement enables us to do without a gasometer, and obviates all the difficulties that could result from the movement of a gas which ought to overcome the resistance of liquid. Moreover, as the reservoir always keeps itself full, its capacity need not be very large.

"With a rapid cooling we can dispense with the use of carbonic acid; we can even allow ordinary air to enter in proportion as the bulk of the wort diminished while cooling."

The germs of disease in the wort are killed in the boiling wort, and those which the volume of free air just mentioned may carry, will not have time to develop in the wort, if the cooling process has been prompt—a condition always easy to bring about by having a suitable flow of water, and by having the bulk of wort always comparatively small.

"In reference to these assertions, it may not be useless to recall the proof I formerly gave of the remarkable fact, that liquids the most variable, even common household broth, preserve their limpidity for years by being kept from contact with air, provided that they be deprived of their germs. In the ordinary processes for manufacturing beer, all these germs are carefully accumulated in the wort, and even in the beer; and besides, the aromatic part of the hops is utterly destroyed by the oxygen of large masses of air; hence a deterioration of the product and a difficulty in keeping it.

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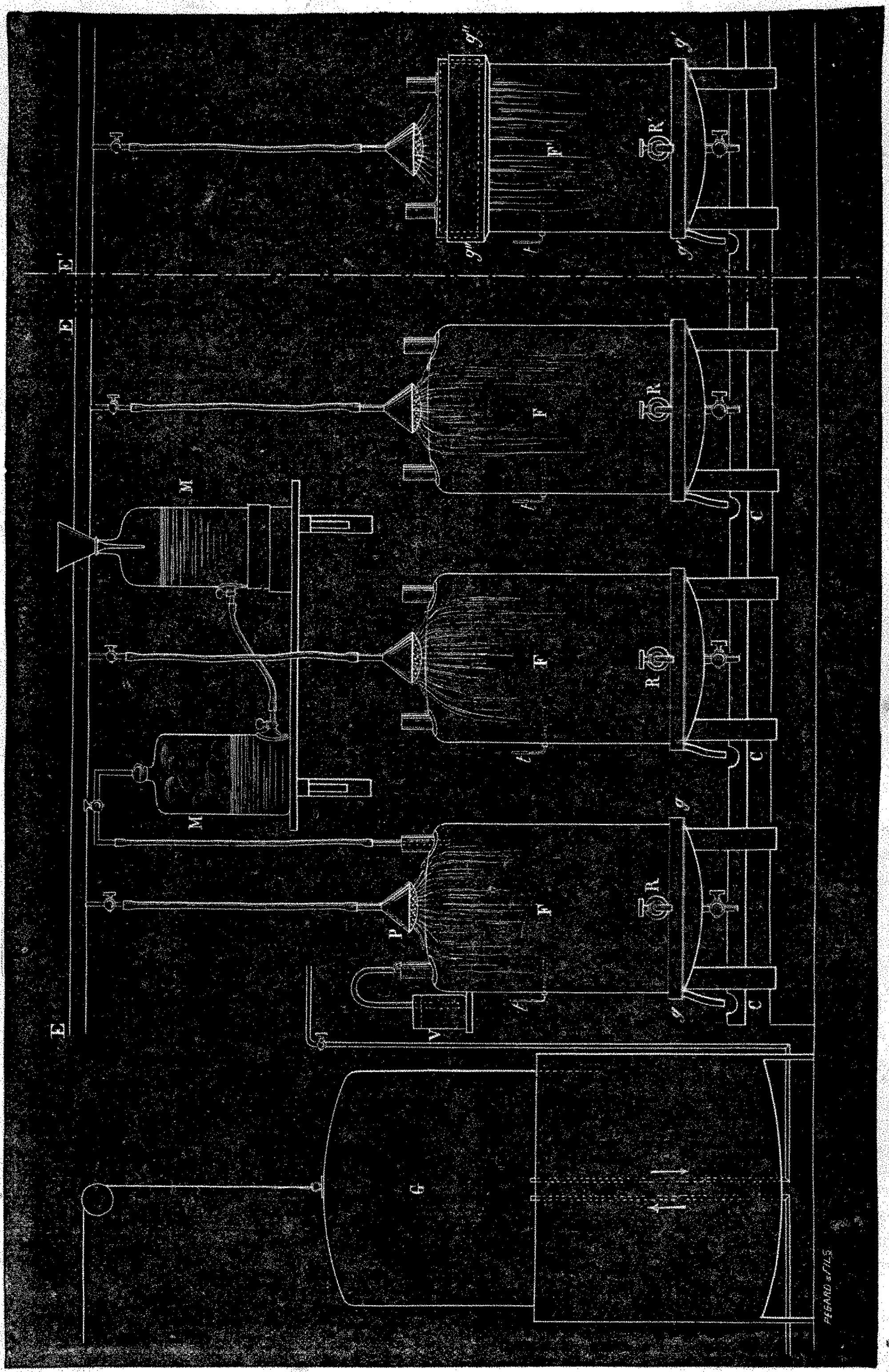
* Translated from an article by M. A. Jouglet, in the *Moniteur Scientifique* for September 1872.

APPARATUS FOR PASTEUR'S PROCESS FOR MANUFACTURING BEER

Lent by the Editor of the "Moniteur Scientifique."



E, E, E'. Pipe for receiving cold water which is distributed by the nozzles P into the fermenting apparatus F, F, F, F'. M, M. Apparatus for manufactured carbonic acid. C, C, C. Discharge-pipe for the water which is thrown out by the troughs *g*, *g*, *g'*, where it collects very warm at the outset of the operation. F'. Another arrangement of the fermenting apparatus in which the moveable cover closes the cylindrical apparatus by means of the exterior trough *g''*, *g''*, with which the border of the cover dips. G. An ordinary gasometer, with a receiving and egress pipe. The fermenting gas of various apparatus can easily be turned into this gasometer, which afterwards supplies this gas when a new process is set agoing. *t*, *t*, *t*. Thermometers to indicate the temperature for putting in the yeast.

"The cooling of the wort can be hastened by various arrangements. One of the most simple consists in having serpentine or cylindrical tubes placed vertically in the interior of the vessels F, F′, where cold water circulates. The boiling wort can be turned into the fermenting vats themselves, to which the cooling tubes alluded to have been added, besides covering over these vats with a tin cover by hydraulic pressure, or else the boiling wort will cool in a large apparatus F or F′, fitted with cooling tubes for the circulation of water; then, at the moment when fermentation begins, the wort is turned into the fermenting vats closed by their hydraulic covers. It is even possible to make the common refrigerators serve by enclosing them in a vessel full of carbonic acid gas, or of air deprived of germs, and even ordinary air, if the vessel is of small capacity.

"To recapitulate:—the main and altogether novel principle of my process consists in the employment of vats of tin or wood, into which the wort is run as hot as possible, and is cooled by a current of water outside, or outside and inside at once, without any evaporation, over which there is absolute control; so much is this the case that, according to the terms of my patent, nothing is more simple than to transport the wort without danger to the greatest distances. With regard to the action of the air, we can limit it at pleasure, in so far as it is noxious, for we can always annihilate the mischievous influence of the germs it contains. The brewer has, moreover, the control over the action of free oxygen gas, so far as it consumes the aromatic or other very delicate principles. Besides, my process allows the temperature to be kept steadily at any height for the purpose of fermentation. In short, its advantages are valuable for the fermentation of German beer, or for mild fermentation, for we can proceed to the employment of ice or any other powerful means of cooling during the process of fermentation. There, is, however, no distinction between strong and mild fermentation, except in so far as the greater or less specific differences of two yeasts, strong and mild, are concerned. The two yeasts can be kept equal; the fermentation will be accomplished in the cold vats.

"Ere long I shall indicate how we may obtain at pleasure, at all seasons, and in all places, the two yeasts in a state of purity, without having recourse to those of the brewer.

"The ferment which is deposited at the bottom of the vessels, F′, is of a brown colour, because it is mixed with the deposit characteristic of the wort during the cooling process. It will be easy to collect it almost white, and without mixture, either by scraping the surface of the cake which it forms at the bottom of the apparatus, or by introducing at the outset into the apparatus, at the moment when we place the cover on the boiling worts, circular plates, attached to a bar which passes through the cover. This upright bar should terminate in the quadrant of a circle, round the extremities of which it can be turned, and moved up or down. While the wort is cooling, the plate of each apparatus will have its plane in a vertical position, and thus it will remain during the first days of violent fermentation. Then, when the ferment begins to settle down, the plate should be gently lowered until it is horizontal. After the product has been drawn off, a cake of ferment will be found upon the plate."

M. Pasteur made still another addition to this process, which was added to his patent in January 1872. It is as follows:—

"When the yeast in one pan is spoiled from any cause, it is necessary to have recourse to yeast taken from another pan. It then becomes a matter of importance to be able to prepare for one's self in any kind of vessel whatever a yeast deprived of all deleterious germs. I have solved this problem by discovering that the *Mycoderma vini* can be made the nucleus of a mild yeast. It can be made to develop itself in the wort of beer sheltered from contact with the air. I have also discovered that the fermenting principle of the grape is a mild yeast. It is a source to which breweries established according to my process can resort."

This addition is, in my opinion, of great importance. M. Pasteur's researches will not probably end here. Every day brings with it a new idea. At all events, from this time the manufacture of beer has received such valuable improvements as will tend to its increased production and use. The process has not yet received the sanction of long experience, but it appears to have fulfilled all that was expected of it. Some have asserted that M. Pasteur's system involves an enormous expense. I do not believe it; experience will show these objections to be unfounded. After all I know very well that M. Pasteur may yet be the victim of envy. It will be remembered what annoyance an enemy gave him in connection with his process for improving wines.

M. CHEVREUL

A VERY interesting episode took place at the *séance* of the French Academy of Sciences of September 2, on the occasion of what may be regarded as the academic jubilee of the Dean, the famous chemist, M. Chevreul. The fiftieth year of his membership does not strictly occur till 1876; but it is well known that he would have been elected in 1816, had he not urged the Academy to give the vacant place to M. Proust, his compatriot, and a celebrated chemist, who was old and infirm, and could not afford to wait. M. Faye, as president of the Academy, intimated that the members had resolved, as a token of their estimate of his works, and their regard for his personal character, to present the venerable Dean that day with a medal, without waiting for the arrival of the formal jubilee. The medal represents the features of the illustrious chemist, who bears the weight of his 86 years much more lightly than many of his fellows who are considerably younger than himself. M. Dumas, the celebrated chemist, and permanent secretary of the Academy, in an eloquent and gracefully-worded speech, recounted the many valuable services rendered by M. Chevreul, who modestly styles himself "le doyen des étudiants français," and at the same time bore warm testimony to the personal character of the man. After M. Elie de Beaumont, who had been a pupil of M. Chevreul, had added a few words of veneration and respect for his old master, the latter attempted to respond, but had simply to express his inability to do so. It was in 1806 that M. Chevreul published his first most important work. He was collaborator of Vanquelin: and he has just completed a volume, entitled "Mémoires de l'Académie," a most interesting work, which throws light upon many of the most scientific questions of the day. M. Chevreul is one of the most distinguished chemists of the age; and, besides being Dean of the Academy of Sciences, is Director of the Museum of Natural History at the Jardin des Plantes. He has chosen for his motto that beautiful maxim of Malebranche, which indeed affords a true key to his life, his works, and his discoveries, "*Chercher toujours l'infaillibilité, sans avoir la prétention de l'atteindre jamais.*"

NOTES

IT is stated, on the authority of a private telegram from Bombay of Tuesday last, that letters from Dr. Livingstone, dated July 2, 1872, have been received at Zanzibar. He was still at Unyamyembe, was well, and waiting the arrival of Stanley's second expedition.

THE fourth three-yearly meeting of the French Institute will take place on the 2nd of October, and the yearly public meeting on the 25th of October.

At the sitting of the French Academy of Sciences, on Sept. 9, a number of communications on the subject of the ravages of the *Phylloxera vastatrix* in the vineyards of France were read by M. Dumas, and referred to the "*Phylloxera* committee" of the Academy. It appears that the disease is making fearfully rapid advances in Provence, threatening the speedy entire destruction of the crop. In the department of Vaucluse it is also rapidly increasing; while in that of l'Hérault it is rather diminishing. All the correspondents agreed that when once a plant is attacked cure is hopeless, and that it is almost impossible to prevent the parasite spreading to neighbouring plants by any other means than complete submersion under water, though the application to the roots of a soil composed of sand, manure, and some insecticide, will delay it for some years. There is no doubt that the wingless insect migrates above ground from the diseased to the healthy plant, and is carried in great quantities by the wind. M. d'Armand, of Marseilles, demanded that a prize of 500,000 fr., or, if necessary, 1,000,000 fr., be offered by the State to any one who shall discover a means of arresting the disease. The pest has made great advances also in Portugal, especially in the neighbourhood of Porto, Villa Réal, Douro, and Santarem; and a Royal Commission has been appointed to investigate fully the causes of a disease which threatens the destruction of one of the most important branches of national wealth, and the best means of curing it.

We have received from New York copies of the *Tribune*, containing full reports of the A.A.A.S.—the transatlantic expansion of the British Association—contributed by one of the editors of that journal. There can scarcely be imagined a more striking indication of the root that Science is taking in America than this, for the report has necessitated some 2,400 miles of travel, and the white heat of a political contest is raging; and yet it appears: in other words, knowing the cleverness of our transatlantic cousins—it pays!

The present excessive price of coals is stimulating the wits of everyone concerned to endeavour to discover some means of reducing it. Certainly one of the best means would be to increase the supply from beneath the soil of our own country; how this may probably be accomplished is suggested in a letter to the *Liverpool Daily Post* of Sept. 16, by Mr. T. M. Reade, C.E. His suggestion is that in all likelihood coal would be found by boring the district immediately around Liverpool. The whole of the rock on which the town stands, and in fact the whole country, from a fault passing near and west of Neston, under the estuary of the Dee, to the Croxteth fault, just beyond West Derby, belongs to the Trias, which in this locality consists only of two members, the Bunter and the Keuper, the third, or Muschelkalk of Germany, being absent. Eastward the Triassic formation extends as far as Manchester, from which it follows an irregular line as far south as Shrewsbury; but as this portion of the district is deeply covered with the red marls of the Keuper, and as it would take deep sinking to reach the underlying coal (assuming it to be present), Mr. Reade confines his proposition to the district enclosed by lines drawn through Liverpool, Warrington, Chester, and West Kirby, at the mouth of the Dee. The whole of this district has been let down as in a trough, by which the Trias has, to a large extent, been preserved from denudation, and the underlying coal formation is consequently, in places, at a considerable depth below the surface; whether at a workable depth is the question which must be considered. Leaving out of account the red marls, which only occupy a small patch by Upton, in Cheshire, and again by the Weaver opposite Frodsham, the highest rock in the series is the Keuper sandstone, which forms the surface at Oxton, Wallasey, Heswell, and Stourton, in Cheshire, and under a portion of Liverpool, west of what is called the Everton fault—a strip of Keuper sandstone running almost due north from Toxteth Park. The remainder of the Triassic formation consists entirely of the Bunter sandstone, which is itself divided into Upper soft red sandstone, the pebble bed, and Lower soft red sandstone. Under these again, before we reach the highest members of the carboniferous rocks, it is an open question whether we should or should not have to penetrate Permian strata. The Permians have been proved in a few places, principally occurring on the northern boundary of the Trias in a narrow strip, and the total depth being but small. At Croxteth, where coal was formerly worked, the New Red sandstone or Trias is said to be directly superposed upon the coal measures; but a well-boring at Winwick, after penetrating 150 feet of red sandstone, the upper part of which is placed with the pebble beds in the geological survey sheet, was sunk 210 feet through strata consisting of hard red rock, stiff red marl, red and white sandstone, with a zone of limestone bands at the base, the boring terminating at 360 feet from the surface in hard rock. These beds, Mr. Reade is inclined to think, belong to the Permians rather than to the Upper coal measures; but it is doubtful if the Permians would be found under the whole area, as they have evidently been subject to denudation before the New Red sandstone was laid down. If coal is to be bored for at Liverpool, it should be at one of the several places where the Lower red sandstone is thrown up to the surface, the probability being, Mr. Reade thinks, that the upper members of the coal-measures would be reached at the depth of 400 or 500 feet, while the workable coal, or commencement of the middle coal-measures, would probably be found at a further depth of 1,200 feet. If the shaft were sunk near a fault, it would soon be seen whether it were worth while to sink deeper. Mr. Reade suggests a patch of Lower sandstone, about half a square mile in extent, at Eastham, and a less one at West Kirby, in addition to that about Croxteth on this side of the river. The depth conjectured by Mr. Reade is very much under some of the depths which have been canvassed in estimates of the increased difficulty and costliness of meeting the industrial demand for fuel.

A great international exhibition of fruit will be held at South Kensington, in connection with the Horticultural Department of the London International Exhibition, 1872, on Wednesday, Nov. 6, in which all home and foreign growers of fruit are invited to take part, and for which occasion a liberal schedule of prizes has been issued by the Council.

The discovery of a new planet, No. 124, by M. Prosper Henry, on the night of September 11-12, is announced from Paris. Its position and motion are as follows:—September 11, $15^m 47^h 35^s$, Paris M.T.; R.A. $23^h 59^m 35^s$; Decl. $-0° 55' 57''$; horary motion in R.A. $-1^s{\cdot}9$, in Decl. $-20''$. Magnitude about 11·7.

During the closing hours of the last Congress of the United States an appropriation of 150,000 dols. was made for the purpose of introducing salmon, shad, and other valuable food fishes into the rivers and lakes of the United States, and its expenditure was placed in the hands of Professor Baird, the United States Commissioner of Fish and Fisheries. The late period at which this appropriation was made rendered it difficult to accomplish much in reference to shad, as the season for their hatching was very nearly over; but, notwithstanding this, a very satisfactory beginning to the enterprise is announced by the commissioner.

Dr. Augustus Krantz, of Berlin, the well-known dealer in specimens of geology and mineralogy, died on the 6th of April last, in the sixty-second year of his age. This gentleman was well known throughout Europe and America for the immense collections kept for sale by him, and many colleges and other cabinets in the United States contain series purchased from him, furnished at very reasonable prices.

THE Newcastle-on-Tyne College of Physical Science has issued its prospectus for the Session 1872-73. The first Session closed with 70 day and over 100 evening students, and considerable accessions are hoped for. The teaching of Biology is still conspicuous by its absence; but there is so strong an element of Natural History on the governing board of the College that we may hope the just claims of this branch of science may not be disregarded much longer. Why is the obsolete term "Natural Philosophy" retained among the subjects taught? It is here apparently meant to embrace Mechanics, Hydrostatics, and Optics. An examination will be held on October 7 and 8 for four exhibitions of 15*l*. each, tenable in the College for two years, in Arithmetic, Algebra, and Euclid, and either Geology, Heat, or Chemistry.

IN 1871 the important papers of Dr. Petermann upon the Gulf Stream, with their accompanying maps and charts, were translated into English and published by the United States Hydrographic Office, under the direction of Captain R. H. Wyman. Since then two supplements have been issued by the office, including additional information obtained by Dr. Petermann, the second one accompanied by a map of the northern regions of Europe and Asia east of Greenland. This, which is on quite a large scale, gives us the results of the discoveries made up to the end of 1871, including the work done by Lamont, Mack, Johannesen, Payer and Weyprecht, Rosenthal, &c. The text of this supplement contains reports of the cruises of Smith and Ulve, and of Captain Torkildsen, papers on the sea north of Spitzbergen, and on Gillis's Land and King Charles's Land, &c. Petermann is of the opinion that, as far as the discoveries of land go, the results of Smith and Ulve are more important than those of any cruise between Greenland and Siberia for many years past, as they show that the north-east line of Spitzbergen extends across 10½ degrees of longitude instead of the 7½ previously assigned, this extension including the southern coast as well as the northern. The easternmost point reached by this expedition was a little beyond the 28th degree of east longitude.

AT no previous period (says *Harper's Weekly*) has there been so much activity displayed on the part of the United States Government in the way of thorough explorations of its territories, the liberality of Congress at the last session in authorising such having been very great. The operations of the Coast Survey have been largely extended, including the commencement of a triangulation between the coasts of the Atlantic and Pacific. Arrangements have been made for extended surveys by the Navy Department of the North Pacific, and an appropriation also made for the expense of making the observation of the coming transit of Venus. Under the War Department are progressing the new survey of the northern boundary of the United States, between the Rocky Mountains and the Lake of the Woods, the geological survey of Mr. Clarence King along the fortieth parallel, and the surveys in Utah and Nevada by Lieutenant George M. Wheeler; while Dr. Hayden's work in the Interior Department is advancing satisfactorily in its two main divisions, as also that of Major Powell along the Colorado.

THERE are at this time four chemical laboratories in Japan, where the science is taught, three of them being presided over by Germans and the fourth by an American. The chief one is at Osaka, where there are nearly 100 students. The rest are at Kaga, Shidzoka, and Fukuwi. A fifth will soon be opened at Jeddo. The students are said to be fairly intelligent, but their minds are at present encumbered with astrology and other kinds of spurious philosophy.

THE BRITISH ASSOCIATION

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE

On the Application of Photography to Copy Diffraction Gratings, by the Hon. J. W. Strutt.

GREAT interest has always attached itself to the beautiful phenomena discovered by Frauenhofer, which present themselves when a beam of light falls on a surface ruled with a great number of parallel and equidistant lines. Their unexpected character, the brilliant show of colour, and the ready explanation of the main point on the principles of the wave theory, recommend them to all; while the working physicist recognises in them the key to the exact measurement of wave-lengths, which has been so splendidly used by Angström and others.

The production, however, of gratings of sufficient fineness and regularity is a matter of no ordinary difficulty. Indeed, the exactness required and obtained is almost incredible. The wave-lengths of the soda lines differ by about the thousandth part. If in two gratings, or two parts of the same grating, the average interval between the divisions differed by this fraction, the less refrangible soda line of one would be superposed on the more refrangible corresponding to the other. In point of fact the gratings ruled by Nobert, to whom the scientific world has been greatly indebted, are capable of distinguishing a difference of wave-length probably of a tenth part of that above mentioned. But in order that the D lines may be resolved at all, there must be no average error (running over a large part of the grating) of $\frac{1}{1000}$ part of the interval between consecutive lines. When it is remembered what the interval is (from $\frac{1}{1000}$ to $\frac{1}{6000}$ of an inch, or even less), the degree of success which has been reached seems very remarkable.

A work requiring so much accuracy is necessarily costly—the reason, probably, why gratings fit to be used with the telescope for the purpose of showing the fixed lines are comparatively rare. The hope of being able to perfect a process for the reproduction of gratings at a comparatively cheap rate has induced me to return at the first opportunity to the experiments described in a preliminary note read before the Royal Society in June last. Although the subject is as yet by no means exhausted, I have thought it worth while to bring before the Section an account of the progress that has been made, with specimens of the results.

The method of procedure is very simple. A dry plate prepared by any photographic process on a *flat* surface of glass or other transparent material not affected by the fluid media employed, is brought into contact with the ruled surfaces of the grating in a printing frame, and exposed to light. In my first experiments I used exclusively as a source of light the image of the sun in a lens of short focus placed in the shutter of a darkened room; but so small a source is not necessary. The light from the clouds or sky, reflected by a mirror through a hole several inches in aperture, will be sufficiently concentrated if the frame be a few feet distant. I have not as yet specially investigated the point, but I believe that if the light were too much diffused the experiment would fail. Much would, no doubt, depend on the perfection of the contact—an element very likely to vary.

The variable intensity of diffused daylight, which it is almost impossible to estimate with precision, has induced me to use exclusively in my later experiments with ordinary photographic plates the light of a moderator lamp. This, with globe removed, is placed at a distance of one or two feet from the printing frame, the distance being carefully measured. Working in this way there is little difficulty in giving consecutive plates any relative exposures that may be required. A collateral advantage is the possibility of operating at any time of the day or night.

With regard to the preparation of the plates, I have latterly been using the tannin process introduced by Major Russell. A preliminary coating with dilute albumen is generally advisable, as any loosening of the film from the glass must be avoided, on account of the distortion that it might introduce. In some states of the collodion, an edging of black varnish put on after the exposure is sufficient to hold the film down. The glasses, after being coated with collodion (I have used Mawson's), are immersed as usual in the silver bath, and then allowed to soak in distilled water, best contained in a dipping-bath. They are then washed under a tap for about half a minute, and put into the tannin solution (about 15 grains to the ounce) held, in my practice, in a small dish. I usually prepare my plates in the evening, standing them up to dry on blotting-paper. In the morning they are in a fit state for use. Artificial heat might no doubt be used if a more rapid drying were desired.

At a distance of one foot from the lamp the exposure required is four or five minutes. The development is the most critical part of the process. The pyrogallic solution should contain plenty of acid (acetic or citric), and its action must not be pushed too far, the mistake which a photographer accustomed to negative work is most likely to make. At this stage the spectra given by a candle flame are not very brilliant, on account of the iodide of silver still covering the parts which are to be transparent. Any trace of fog is especially to be avoided. I have experienced advantages in many cases from a solution of iodine in iodide of potassium applied to the film previously to fixing; but its action must be carefully watched, or too much silver will be converted. The iodide of silver is then cleared away with hyposulphite of soda or cyanide, followed by a careful washing under the tap.

With regard to the gelatine copies, I have not much to add to the account read before the Royal Society. The process is very simple, and some of the results very perfect; but I have not hitherto succeeded in sufficiently mastering the details. Plates apparently treated in precisely the same manner turned out very differently. That difficulties should arise is not very extraordinary, considering the novelty of the method; but it is curious that some of the very first batch prepared are among the best yet produced. The value of the results is so great that I have no intention of abandoning my attempts, and perseverance must at last secure success.

I will now say a few words about the performance and prospects of the new copies. Their defining power on the fixed lines in the solar spectrum is all that could be desired, being, so far as I can see, in no way inferior to the originals. In the third spectrum the 3,000 to the inch gratings show the line between the D's, if the other optical arrangements are suitable. The fourth line of the group, *b*, is distinguished with the utmost ease. I am not sufficiently familiar with spectroscopic work to make an exact comparison, but I presume that two prisms of 60° at least would be required to effect as much. I am here speaking of photographs on worked glass. With ordinary patent plate, although very good results may be obtained, if tested by the naked eye only, it is a great chance whether the magnifying power of a telescope will not reveal the imperfect character of the surface.

With direct sunlight the light is abundantly sufficient, but it is here in all probability that the weak point of gratings lies. It should be distinctly understood that where light is deficient gratings will not compete with prisms. There are cases, however, where the scale might be turned by the opacity of all highly dispersive substances to the rays under examination. Even if glass be retained as the substratum, it may be used in a very thin layer, while prisms are essentially thick. The immense advantage of a diffraction spectrum for the investigation of dark heat need not here be insisted on. Taking all things into consideration, it is probable, I think, that photographed gratings will supersede prisms for some purposes, though certainly not for all.

The specimens in the hands of Mr. Ladd are copies of two gratings by Nobert, each of a square inch in surface, the one containing 3,000 and the other 6,000 lines. The latter cost about twenty pounds.

SECTION C.—GEOLOGY

Monday, Aug. 19.—*On the Occurrence of Erect Bases or Trunks of Psaronius in the Devonian Rocks of New York, U.S.A.*, by Prof. James Hall.

During the year 1870 some excavations were made in beds of fine sandstone, referred at that time to the upper part of Hamilton's group, but which probably belong to beds higher in the series. In these beds several thousand trunks of tree-ferns were found in an upright position, with their bases resting in and upon a bed of clay, in which they appear to have originally grown. In the clay, and in the sandstone above, to the height of two or three feet, great quantities of vegetable substance occurred. Principal Dawson refers these trunks to the genus *Psaronius*, and he has determined two or more species from the locality.

These facts were held to indicate a point of comparatively dry land upon the eastern margin of the Devonian Sea. Receding from this ancient shore we find the sands and slates to become finer, and the latter to change into calcareous muds. For some distance the shells occurring in these beds are all Lamellibranchs, and it is only when we have travelled a considerable distance to the westward that Brachiopods appear. The author then entered into some detail as to the mode of accumulation of these beds, which are admirably exposed along a line of outcrop 300 miles in length.

Sur les Animaux Fossiles du Mont Leberon, Vaucluse, by Prof. A. Gaudry.

The author remarked that the fossils found by him and others in this place bore a striking similarity to those he had before collected in Attica. In comparing the 4,940 bones from the latter place with the 1,200 from the former, he had been much struck with the great variations exhibited by animals that seem to descend from the same parent. The presence of numerous and large herbivores proves the existence of a considerable extent of meadow-land during the Miocene period.

Brief Notice of the Present State of our Knowledge in connection with the Brachiopoda, by Thomas Davidson, F.R.S.

The object of this paper was "not to trouble you with details, but to mention, in general terms, what has been the advance effected in this portion of palæontology since 1853, the period at which I first published my general introduction." Mr. Davidson first alluded to the general question of classification, dissenting from the views of Prof. Morse, who wished to remove the Brachiopoda from the Mollusca, and to place them with the Annelides. The great importance of the Brachiopoda to the palæontologist was then dwelt upon, and the author remarked that "many instances are on record where the sight of a few specimens of Brachiopoda have enabled a palæontologist to determine accurately the age of a rock in some distant land to which he had no access." Mr. Davidson stated that the number of so-called species of recent forms amounted to about 100, but that probably the number would have to be reduced to about 60. "The number of so-termed species of *Lingula* had been greatly exaggerated, and a certain number of the others are known only by a single specimen."

Tuesday, Aug. 20.—Mr. H. Woodward read his *Sixth Report on Fossil Crustacea.* The report was illustrated by a large number of diagrams, and announced the discovery of new crustacean forms in Silurian, Coal Measure, and Permian rocks.

Remarks on the Genera Trimerella, Dinobolus, and Monomerella, by Thos. Davidson and Prof. W. King. The authors proposed to group these three genera into a new family to be called Trimerellidæ. It was shown to be structurally allied to the Lingulidæ, and it was inferred that the two families were genetically related. This is a point of great interest, inasmuch as the Lingulidæ are the earlier forms, occurring in Cambrian rocks, whilst the Trimerellidæ first appear in the Silurian strata. The Cambrian Lingulas have a horny shell, and so too have generally the animals associated them. In later formations the Brachiopods and other animals have much more calcareous shells; and from these facts it was inferred that lime was less abundant in the Cambrian sea than during later periods. As the result of long labour in this field of research, the authors were led to adopt the doctrine of evolution of species "effected mainly through the operation of Divine laws, and not by purposeless or accidental modifications." The paper concluded with detailed descriptions of the structure of the Trimerellids.

The Rev. John Gunn's paper was then read *On the Prospect of Finding Productive Coal Measures in Norfolk and Suffolk, with Suggestions as to the Places best adapted for an Experimental Boring.* The author controverted the views expressed by Sir R. Murchison as to there being no coal beneath these counties, and on the following grounds:—The "Anglo-Belgian Basin," in which the forest bed was deposited, is bounded by the chalk on the west and south, and by older rocks on the east. It was contended that this area has been characterised by vegetable growths at several successive periods; and that as regards the coal-growths, these were accumulated in a basin bounded by the carboniferous limestone, just as the forest-bed was accumulated in a basin bounded by the chalk. Hunstanton was suggested as the best place to bore; because there the cretaceous rocks have been denuded, and there too some of the oolites are absent. Probably the bore would not exceed 1,000 feet in depth.

An interesting paper was then read by the Secretary, forwarded from Bonn by G. vom Rath, *On a remarkable Block of Lava ejected by Vesuvius during the great Eruption of April* 1872, *proving the Formation of Silicates by Sublimation.* This was a block of old lava which it is assumed had been floating in the melted lava of the late eruption, and was subsequently ejected by the volcano. It shows that in its interior there were formed crystals of pyroxene, mica, sodalite, specular iron, and magnetite;

whilst at the exterior the pyroxene was melted and the leucite destroyed.

The author particularly pointed out that sodalite, which was found sublimed in the interior of the block, is the silicate most rich in sodium. This he contended was not an accidental circumstance, but resulted from the percolation of sea-water charged with chloride of sodium. The author remarked that the study of such matters is conducting us to the conclusion that the quantities of water, hydrochloric, and sulphuric acid, &c., exhaled by craters and streams of lava, are not only an accompanying phenomenon in the production of volcanic rocks and mineral aggregates, but that they are essentially co-operating at their origin. If once we succeed in proving and explaining the origin of minerals through vapours, or under the co-operation of vapours, then the key to many problems relating to the plutonic rocks and their minerals will be found.

A few Remarks on Submarine Explorations, with Reference to M. Delesse's work entitled "Lithologie du fond des mers," by J. Gwyn Jeffreys, F.R.S.

The lithology of the sea-bottom is not only a vast subject in its various relations to natural history and physical science, but is especially interesting in a geological point of view, because every part of our globe has been at one period or another covered by the sea. Mr. Jeffreys contended that it is almost impossible to ascertain with any degree of certainty what stratified formations are marine, unless we find in them such remains of marine animals as were capable of being preserved. Exceptions doubtless occur, *e.g.* where the stratum had been subject to the action of carbonic acid, produced by the subsequent passage of rain or fresh water; in which case all cretaceous organisms might have been dissolved before they became silicified or petrified. He then gave a short account of submarine explorations, from the time when O. F. Muller first used a dredge for scientific purposes (about 1772), to the present day; and he summarised the results of the expeditions conducted by his colleagues and himself on board H.M.S. *Porcupine*, under the auspices of the Royal Society in 1869 and 1870. But next to nothing is known of the enormous tracts of sea-bed which underlie the depths of the ocean in both hemispheres. He attributed the diffusion and geographical distribution of the marine invertebrate fauna to the action of currents, and not to voluntary migration.

M. Delesse's work was recently published at Paris, and consists of two octavo volumes, besides an atlas of charts and maps. The precise date of publication does not appear; the dedication is dated December 1, 1871. It forms part of a series called "Publications scientifiques industrielles," and purports to have been published with the sanction of the Ministers of Marine and Minister of Public Works.

While giving M. Delesse full credit for the laborious and conscientious manner in which he has evidently performed his great task, Mr. Jeffreys regretted that he had omitted to notice the reports on deep-sea explorations published by the Royal Society in 1869 and 1870, or the Address of Mr. Prestwich (the late President of the Geological Society), which was published in May 1871, and particularly treated of those reports. M. Delesse is a foreign member of the Geological Society. By consulting what had been published on the subject, M. Delesse would have been able not only to give fuller information, but to correct errors which unavoidably occur in an extensive compilation. For instance, his map of France during the tertiary epoch does not show the communication which has been proved by naturalists and geologists to have then existed between the Bay of Biscay and the Gulf of Lyons. According to M. Delesse, there has been no communication since the Liassic period between the Atlantic and the Mediterranean north of the Pyrenees. His division of the French marine fauna into three provinces (Celtic, Lusitanian, and Mediterranean) does not agree with modern observations. Zoophagous mollusca do not, as stated by him, live on those which are phytophagous; pebbles ("*galets*") are not everywhere unfavourable to mollusca, even on coasts exposed to a stormy sea; and foraminifera never crawl at the bottom of the sea. But it is to be hoped that these omissions and errors will be rectified in another edition of a work so desirable and important to scientific inquirers.

Mr. W. Boyd Dawkins, F.R.S., then read his paper *On the Physical Geography of the Mediterranean during the Pleistocene Age.* The author showed from the researches of M. Gaudry that during the late Miocene period it is probable that there was some communication between Attica and Africa. During the Pliocene period a similar communication must have existed at some part or parts of the Mediterranean area. The object of this paper was to show that a like union existed during the Pleistocene age. The palæontological evidence was first gone over. It was shown that African mammalia are found at Gibraltar, in river gravels near Madrid, in Sicily, Malta, the Morea, and in Candia; particular reference was made to the occurrence of a small species of Hippopotamus (*H. Pentlandi*) in these localities, and it was contended that there must have been communication between them and with Africa.

An examination of the soundings makes this probable. It is found that the Mediterranean consists of two deep basins, separated from each other by comparatively shallow water, one barrier extending from Africa, past the Straits of Gibraltar to Cadiz, and the other reaching from Tunis, past Sicily and Malta, to join Italy. An elevation of 2,000 feet would effect this. It was pointed out that the existence of such a mass of high land in the south of Europe must have had an important effect upon the climate of the period.

Mr. Charles Moore's paper *On the Presence of Naked Echinodermata (Holothuria) in Oolitic and Liassic Beds*, was then read. Soft-bodied animals, such as these, are rarely found in a fossil state; but the author had fortunately discovered some minute wheel-like plates, somewhat resembling those of a recent Greenland species, and which he referred to at least four different species of Holothuria. Some of these plates indicate structures not hitherto seen in recent species.

Mr. J. E. Lee read a *Notice of Veins or Fissures in the Keuper filled with Rhætic Bone-bed, at Goldcliff in Monmouthshire.* There are exposed on a scarf of Keuper marl, bare at low water, a number of rounded masses of irregular form, but of great length, consisting wholly of bone-bed. The author presumed that these projections are the casts of fissures in the marl which were afterwards filled up with bone-bed. Mr. C. Moore afterwards made some remarks on the extraordinary richness of the bone-bed.

Wednesday, August 21.—Mr. J. H. Judd communicated through Mr. Hughes, a note *On the Discovery of Cretaceous Rocks in the Islands of Mull and Inch Kenneth.* In the former they are seen at several places, and the author supposed that they would be found underlying a great part of the volcanic area of this district. The rocks are all of upper cretaceous age and lie unconformably upon the Jurassic series and older rocks. Like all other rocks of these islands the cretaceous beds are penetrated by intrusive dykes and sheets of trap. Mr. Judd observed, that this discovery gave great confirmation to Prof. Geikie's views as to the Tertiary age of the volcanic rocks of the Hebrides.

Mr. T. A. Readwin's paper, *On the Coal and Iron Mines of the Arigna District of the Connaught Coal Measures, Ireland,* was then read in abstract.

The shales overlying the Upper Limestones of district were surmised by the author to belong to the Yoredale Series. Over these there are grits and shales with three seams of coal which the author referred to the Gannister Series, remarking that a bed of true "gannister" occurred there.

The coal field was divided into three districts, each of which was described by the author. He noticed at some length the clay-ironstone bands and nodules which occur over a much larger area than do the coals. The ironstone is richer and purer than most of our English clay ironstone. The author believed that the time had come for a vigorous and scientific exploration of the district, which he felt convinced would soon become, as Sir Robert Kane had long ago predicted, "an important centre of industry for the interior of the country."

SECTION D.—BIOLOGY

On Deep Sea Dredging round the Island of Anticosti in the Gulf of St. Lawrence, by T. F. Whiteaves, F.G.S.

Depths of from 100 to 250 fathoms were successfully explored during July and August 1871. The temperature of the deep sea mud was found by using a common thermometer to be almost invariably 37° or 38° F. About 100 species of Invertebrata new to the Gulf of St. Lawrence were collected. These included a remarkable foraminifer, *Marginulina*, with spinous processes from the first chamber, *Grantia ciliata*, a new *Pennatula*, &c. Two rare Echinoderms were collected—the well-known *Schizaster fragilis*, and the curious *Calveria hystrix* of Wyville Thomson. Nearly all the marine invertebrates of the northern part of the Gulf of St. Lawrence are purely Arctic species. Three-fourths of the mollusca of Greenland range as far south as Gasté Bay. The species which belong exclusively to the deep sea in Canada have a decidedly Scandinavian aspect.

Preliminary Notice of Dredgings in Lake Ontario, by H. Alleyne Nicholson, M.D., D.Sc.

In this communication the author gave a short preliminary account of a series of dredgings carried out in June and July in Lake Ontario. With a praiseworthy appreciation of the value of such researches, the Provincial Government of Ontario had placed at the author's disposal a sum of money to be expended in this investigation, and the results had been very satisfactory. The dredgings were carried on partly in a yacht and partly in a small paddle-wheel steamer, and were prosecuted wholly by hand, the apparatus employed being similar to that used in marine dredging, except that a bag of embroidery canvas was attached outside the ordinary net, an addition rendered necessary by the exceedingly fine nature of the mud at great depths.

Upon the whole, the results obtained in Lake Ontario agreed very fairly with those obtained in Lake Superior in 1871, there being a general conformity in the phenomena observed in both areas. The fauna of Lake Superior, however, so far as deep water is concerned, is decidedly richer than that of Lake Ontario, whilst some of the more remarkable species discovered in the former appear to be altogether absent from the latter. As might have been anticipated, the fauna of Lake Ontario is not extensive, though some forms occur in great profusion. The shallow-water fauna is very rich in individuals, and the number of species is quite considerable for fresh water. Beyond eight or ten fathoms, the fauna becomes very scanty; and when depths of from twenty to fifty fathoms are reached, the list becomes reduced to some Annelides and Amphipod Crustacea. The nature of the bottom, also, at great depths is very unfavourable to life, consisting almost everywhere of a fine impalpable mud or clay, the temperature of which is very low.

Out of thirty-one forms in all discovered by the author in Lake Ontario, the most interesting were the Annelides and Crustaceans. The Annelides were very abundant, and consist of species of *Nephelis*, *Clepsine*, *Saenuris*, and *Chirodrillus*, some of the Leeches presenting phenomena of special interest. Of the *Crustacea*, the most important is a little Amphipod, which occurred plentifully in from thirty to forty-five fathoms, and which the author identified with the *Pontoporeia affinis* of the Swedish lakes. This species, and the Stomapod, *Mysis relicta* of Lóven, are found in Lakes Wetter and Wener in Sweden, and it is well known that they have been supposed upon good grounds to support the view that these lakes had been at one time connected with the sea. It is, therefore, a very interesting fact that these crustaceans should both have been found in Lakes Michigan and Superior. The *Pontoporeia* the author had now detected in Lake Ontario; but it was a curious fact that the *Mysis*, which is of common occurrence in Lake Superior, should not have been found to occur at all in the dredgings carried on in Lake Ontario.

On the Flora of Moab, by A. W. Hayne, M.A.

The 250 plants found in Moab from the beginning of February to the middle of March, belong to 58 natural orders, of which by far the best represented are Leguminosæ with 35 species, Compositæ and Cruciferæ each with 26, and Gramminaceæ 23. The remainder belonged to Liliaceæ, Scrophulariaceæ, Labiatæ, Boraginaceæ, Umbelliferæ, etc. From the greater abundance of springs the Eastern shore of the Dead Sea is comparatively fertile. The most conspicuous difference which results is the abundance of the date palm, of which on the West only a single clump survives near Jericho.

On the Structure and Development of Mitraria, by Prof. Allman, F.R.S.

Several specimens of the remarkable larval form, to which John Muller gave the name of *Mitraria*, were obtained by Prof. Allman in the Gulf of Spezzia, and were made the subject of careful study of structure and development. Mecznikoff had recently examined another species of the same form, and the author was enabled to confirm the main result arrived at by him, that *Mitraria* was the larval form of an annelide. In some fundamental points, however, regarding the process of development, his observations did not agree with those of the Russian zoologist; while in structure there are some important features which have not been described by either Muller or Mecznikoff, differences which may, in some cases at least, depend on actual differences between the species examined.

The nervous system is well developed, and consists in the principal central portion of a large quadilateral ganglion, formed by the union of two lateral ones, and situated on the summit of the transparent dome-like body of which the larva mainly consists. From this two very distinct chords are sent downwards, so as to form a pair of commissures with two small ganglia, which are situated at the opposite side of the alimentary canal. Besides these, two other small ganglia exist in the walls of the dome, at the oral side of the great apical ganglion, and two similar ones at the ab-oral side. These send off numerous filaments, which dive at once into the walls of the dome, while each sends off a long filament to the region where the alimentary canal begins to bend downwards towards its ab-oral termination. The great apical ganglion supports two sessile ocelli, with pigment and lens, and two small spherical vesicles, each containing a clear spherical corpuscle. These last the author regards as auditory capsules.

A system of vessels was also described. This consists mainly of a sinus which surrounds the great apical ganglion, and sends off three branches, which run in a radial direction in the walls of the dome, two lateral and one ab-oral, and appear to open into a sinus which surrounds its base.

In the progress of development the ab-oral end of the alimentary canal becomes elongated in the direction of the axis of the dome, carrying with it the walls of the base of the dome, which are to form the proper body walls of the future worm, and in this way a long cylindrical appendage becomes developed, and hangs from the central point of the base. At first there is no trace of segmentation, and this is subsequently induced on the cylindrical body of the worm by the formation of consecutive annular constrictions.

The process of development as observed by the author in the species of *Mitraria* examined by him thus differs in several points from that observed by Mecznikoff. Among these the most important is that the ventral side of the worm is formed simultaneously with the dorsal instead of subsequently to it and independently of it, as in the case described by Mecznikoff. The development of the worm was not traced to the ultimate disappearance of the dome-like body of the larva.

On the Whales of the Antwerp Crag, by Prof. Van Beneden.

A brief account was given of the great accumulation of bones of fossil cetacea, or rather of whales, which are found in the Antwerp Crag, and of which the greater part belong to species new to science. These primitive whales were all small in size, and in that respect have no existing representative except the Neobalæna of New Zealand. It is only in the Upper Crag that we find representatives of larger species which actually exist such as those of the genera of Balæna, Megaptera, and Balænoptera.

Prof. Flower said that the excavations at Antwerp had revealed a perfect cetacean burial ground. Under the superintendence of a *savant* who had a most intimate acquaintance with the osteology of recent whales, the skeletons of the extinct species had been almost reconstructed in the Brussels Museum. It was a remarkable thing that these ancient whales were all small. It was the reverse of what happened in most other cases where the ancient representatives of any type were generally far larger than those at present existing.

Mr. Sclater inquired what was the relation between the cetacea of the Antwerp and Suffolk Crag.

Prof. Van Beneden replied that they were identical. The English material was not in itself sufficient for independent determination; but with the knowledge he had acquired from the more perfect remains, he was able to identify those from the Suffolk Crag.

Prof. Allman said that there was a parallel to the case of the whales in the dwarf fossil elephant of Malta. This was of the more interest, as the affinities of the elephant and of the whale are by no means remote.

On some points in the Development of Vorticellidæ, by Prof. Allman.

The author described, in a beautiful branched and clustered vorticellidan, a process different from any which had been recorded by those observers who had described the so-called encysting process, and the behaviour of the "nucleus" in the Vorticellidæ.

In almost every cluster some of the zooids composing it had become greatly altered in form. They had increased in size, and instead of the bell-shaped form of the others had assumed a globular shape, and had lost both oral orifice and ciliary apparatus, while their supporting peduncle had ceased to be contractile.

In the younger ones the contractile space of the unchanged zooid was still very evident, but was fixed, showing no tendency to alteration of size, and the so-called nucleus was very distinct and larger than in the ordinary zooids. The whole was enveloped in a transparent gelatinous-looking investment.

In a slightly more advanced stage another envelope, in the form of a brown horny capsule, begins to be secreted between the proper wall of the zooid and the external gelatinous investment. It is at first thin and smooth, but it gradually acquires considerable thickness, and becomes raised on its outer surface into ridges enclosing hexagonal spaces.

In this stage the capsule has become too opaque to admit of a satisfactory view into its interior; but if the capsule be carefully opened its contents may be liberated so as to render apparent their real nature. It will be then seen that these consist of a minutely granular semi-fluid plasma surrounding the "nucleus," which has much increased in size and occupies a large portion of the cavity of the capsule. The condition of the contractile space could not be determined; it has probably altogether disappeared.

In a further stage the "nucleus" has undergone an important change; for, instead of the long cylindrical form it had hitherto presented, it has become irregularly branched, has acquired a softer consistence, and has moreover broken itself up into two or more pieces. This change in the "nucleus" is invariably accompanied by the appearance of nucleated cell-like bodies, which are scattered through the corpuscular plasma which had filled the rest of the capsule. They are of considerable size, of a spherical form, and with their nucleus occupying the greater part of their cavity, and having its nucleolus represented by a cluster of granules.

In other capsules, apparently the more advanced, no trace of the so-called nucleus of the vorticella body could be detected, and it seems to be entirely replaced by the spherical nucleated cells, which had now still further increased in number. It is impossible not to regard these cells as the result of the disintegration of the nucleus, and the conclusion is a legitimate one that they are finally liberated by the natural dehiscence of the capsule, and become developed into new vorticellidans.

On the Structure of Noctiluca, by Prof. Allman.

The author gave an account of some researches he had made on *Noctiluca molicus*. They were mostly confirmatory of the results arrived at by other observers, more especially by Krohn, Quatrefages, Busch, Huxley, and Webb, while they further served to supplement the observations of these zoologists.

At one end of the meridional depression is the vibratile flagellum with the mouth at its base, and here the depression becomes quite superficial, while the opposite end is much deeper and is here abruptly closed. Just outside of this deep end of the depression there commences, by a funnel-shaped enlargement, a very slightly elevated ridge of a firmer consistence than the rest of the body; it terminates abruptly after running down, in a meridional direction, over about one-third of the circumference of the body. The author had reason to believe that this ridge is traversed in its length by a canal which opens close to the ab-oral extremity of the meridional depression by a funnel-shaped orifice, thus giving support to the opinion of Huxley, who believes that *Noctiluca* is provided with an anal orifice. The mouth leads into a short cylindrical gullet, and the author confirmed the existence of the vibratile cilium contained within the gullet, as originally described by Krohn; and of the ridge, with its projecting tooth, described by Huxley as existing in the gullet walls. The floor of the gullet is formed by the central mass of protoplasm, here naked and in direct contact with the surrounding medium. The vibratile cilium springs from this floor, and near the root of the cilium is a depression in the floor which can be followed for a little distance into the protoplasm.

Besides the well-known branching processes which radiate from the central mass of protoplasm to the walls of the body, there is also sent off from the central mass a broad, irregularly quadrangular, plate-like process, which extends to the outer walls, where it becomes attached along the line of the superficial meridional ridge. The lower free edge of this broad process is thickened in the manner of a hem.

In contact with the central protoplasm is the nucleus, a clear spherical body about $\frac{3}{2000}$ of an inch in diameter.

The body walls are composed of two layers—an external thin, transparent, and structureless membrane, and an internal thin granular layer of protoplasm, which lines the structureless membrane throughout its whole extent, and which receives the extremities of the radiating processes from the central mass. Under the action of iodine solution and other reagents, the protoplasmic layer may be seen to detach itself from the outer structureless membrane, and, along with the radiating bands, contract towards the centre. It admits of an obvious comparison with the primordial utricle of the vegetable cell.

The flagellum, which is given off close to the margin of the mouth, is a flattened band-like organ, gradually narrowing towards its free extremity, and with its axis transversely striated like a voluntary muscular fibre throughout its whole length. It seems to have the power of elevating its edges, so as to render one of its surfaces concave, and thus becomes converted into a semi-tube, which may assist in the conveyance of nutriment towards the mouth.

The nucleus is a spherical vesicle, with clear colourless contents, among which minute transparent oval corpuscles may usually be detected. When acted on by acetic acid, the difference between the contents of the vesicle and its wall becomes very apparent, and the contents may now be seen accumulated towards the centre as a minutely granular mass, with some of the oval corpuscles entangled in it.

The radiating offsets, which extend from the central protoplasm to the peripheral layer, contain well-defined clear corpuscles which slowly change their relative places, as if under the influence of very feeble currents. These offsets, indeed, closely resemble the radiating protoplasm filaments which extend from the protoplasm, surrounding the nucleus, to the walls of the primordial utricle in the vegetable cell. The peripheral layer contains scattered through it numerous minute cell-like bodies. These are spherical and of various sizes; in the larger ones a distinct central nucleus may be detected.

It is scarcely correct to regard the central mass of protoplasm as a true stomach. The author had failed to find any evidence of a permanent gastric or somatic cavity, and he regarded the protoplasm mass to which the gullet leads as representing the "parenchyma" of the Infusoria, and, like this, allowing of the solid food being forced down into it from the gullet and there encysted in extemporaneously formed vacuolæ. The food also frequently forces its way from the central mass into the radiating processes, and diatoms and other microscopic organisms may be seen in these processes enclosed in cyst-like dilatations of them, extemporaneously formed for their reception at various distances from the central protoplasm.

It was considered probable that the canal which seems to exist in the superficial ridge affords exit for certain effete matters, which may be conveyed to it through the process, by which it is kept in connection with the central protoplasm.

Our knowledge of the phenomena of reproduction and development in *Noctiluca* is still very imperfect, and the author saw little which seemed capable of throwing additional light on this subject. He agreed, however, with Huxley in regarding it as probable that the nucleated cell-like bodies which are present in the peripheral layer of protoplasm have a reproductive function, and are destined after liberation to become developed into new individuals.

From the account now given it will be apparent that *Noctiluca* consists essentially of an enormously vacuolated protoplasm, involving a nucleus and enclosed in a structureless sac, the vacuolation taking place to such an extent as to separate the contents into a peripheral layer of protoplasm which remains adherent to the outer sac, and into a central mass which is kept in communication with the peripheral layer by processes of protoplasm which pass from one to the other. The author believed that the nucleus of *Noctiluca* had a significance different from that of the so-called nucleus of the ordinary Infusoria, and that it admitted of a closer comparison with the true cell-nucleus. He was of opinion that the nearest ally of *Noctiluca* would be found in the somewhat anomalous infusorial genus *Peridinia*.

In conclusion the author detailed some observations he had made on the luminosity of *Noctiluca*; and he gave reasons for maintaining that the seat of the phosphorescence is entirely confined to the peripheral layer of protoplasm which lines the external structureless membrane.

On the Structure of Edwardsia, by Prof. Allman.

The structure of this beautiful little actinozoon differs in many important points from that of both the zoantharian and alcyonarian polypes. It was shown that just within the mouth the walls of the stomach sac project into the cavity of the sac in such a way as to form eight complicated frill-like lobes; that the eight vertical, radiating lamellæ which project into the body cavity from the outer walls, and are composed of parallel longitudinal fibres enclosed between two membranous layers, do not reach the stomach sac in any part of their course, and that eight strong muscular bundles pass symmetrically through the whole length of the body cavity, being attached at one end to the disc which carries the tentacles, and at the other to the floor of the body cavity, while they are free in their intervening course.

Attached along the length of about the posterior half of each muscular bundle is the long sinuous generative band, with its chord-like craspedum loaded with thread cells. Just before terminating at the lower opening of the stomach sac each of the eight generative bands enters a most remarkable pectinated organ, which appears to be quite unrepresented in any other group of the Cœlenterata. It was difficult to suggest the true significance of these organs; their relation to the generative bands might lead to the belief that they are testes, or they may be analogous to the so-called cement glands which exist near the outlet of the oviducts in some of the lower animals. In this case they might supply some additional investment to the ova at the time of extrusion.

The author regarded *Edwardsia* as presenting a very distinct type of actinozoan structure, which occupies an intermediate position between that of the zoantharian and that of the alcyonarian polypes. He also compared it with the extinct rugose corals of the palæozoic rocks to which it corresponds in the numerical law of its body segments, and of which it might in some respects be regarded as a living non-coraligenous representation.

On the Structure of Cyphonautes, by Prof. Allman.

This remarkable little organism, whose structure and ultimate destination have been variously described by different observers, was obtained by the author in considerable abundance in Moray Firth. The animal is enveloped in a mantle, and the whole enclosed in a delicate, transparent, structureless test formed by two valve-like triangular plates which are in contact along two edges, and separated from one another by a narrow interval along the third. Its form is thus that of a very much compressed cone or pyramid. The author distinguishes by the term base the broader edge where the two plates of the test are separated from one another; while the other two edges are distinguished as the anal and ab-anal edges. The apex is the angle opposite to the base, and here a narrow passage exists through which the fleshy walls of the mantle are brought into immediate contact with the surrounding water.

In the base are two large oval openings, one, the larger, situated towards the anal edge, and the other towards the ab-anal. The former leads directly into the cavity of the mantle. Its edges are prolonged by a membranous lobe ciliated on its margin, and uninterruptedly continued round the anal side of the opening, but deficient on the opposite side. The interior of the lobe is occupied by a cavity.

A large part of the mantle cavity is occupied by the pharynx, a spacious thin-walled sac which opens into the mantle cavity by a long curved somewhat S-shaped slit with thickened and ciliated margins, which, at one side, are continued beyond the large opening situated near the anal side of the base in the form of two short ciliated tentacles. Towards the apex the pharynx becomes suddenly narrow, and is here lined by vibratile cilia, and marked by circular striæ which possibly indicate the presence of sphincter fibres. It now turns towards the anal side, and then bends downwards towards the base, and enters a thick-walled sub-cylindrical stomach. This runs towards the base parallel to and a little within the anal edge of the test, and is ultimately continued into a short straight intestine, which terminates by an anal orifice in the mantle cavity near the outer opening of the latter. From the upper part of the walls of the pharynx a narrow bundle of fibres passes to the apex of the mantle cavity.

Upon each side of the pharynx and lying against the stomach and intestine is a large oval mass. Its situation would suggest the probability of its being a hepatic organ, but it is altogether so enigmatical that it would be rash with our present knowledge of it to insist on assigning to it any special significance.

In contact with each of these enigmatical organs is a small tubercle, from which a bundle of short fibres pass off in a radiating direction. The resemblance of these bodies to a pair of nervous ganglia is obvious, but the author was more inclined to regard them with Schneider as indicating points of attachment of the contained animal to the two valves of the test.

The smaller of the two openings in the base, that, namely, which is situated near the ab-anal edge of the animal is, like the other, surrounded by a hollow membranous lobe with ciliated margin. This is uninterruptedly continued round the ab-anal side of the opening, but is deficient on the opposite side. The opening leads into a special chamber entirely shut off from the cavity of the mantle and from the pharynx. The walls of the chamber are lined with cilia, and it has within it, or in immediate connection with its walls, two peculiar structures. One of these is a somewhat pyriform organ which, with one end close to the orifice of the chamber, extends from this point into its cavity; it is composed of a mass of spherical bodies. The other extends over the roof of the chamber in form of a cap; it consists of two portions, one of which lies directly on the walls of the roof, and has a transversely laminated structure, which, however, disappears towards the ab-anal side of the chamber; the other is an oval mass of globular cell-like bodies and lies on the free convex surface of the laminated portion.

Here again this part of the *Cyphonautes* is in the highest degree enigmatical, and yet it is difficult not to believe that in the structures just described we have an ovary and testis with associated accessory structures.

The author observed no further fact which might tend to throw light on the ultimate destination of *Cyphonautes*, and more especially nothing which might tend to confirm the remarkable views lately published by Schneider, who believes that he has traced its devolopment into the polyzoal *Membranipora pilosa*. The structure is considerably more complicated than Schneider seems to be aware of, while the opinion of this observer that the whole of the proper *Cyphonautes* structure becomes absolutely obliterated and the body of the animal converted into an amorphous mass of cells from which the *Membranipora* becomes evolved not by a process of budding but by a differentiation of structure is so startling that notwithstanding the partial assent lately given to it by Nitsche we are compelled to wish for further confirmation of the evidently careful observations of the German zoologist.

If the ab-anal chamber described above with its associated structures really belongs to the generative system—and it is hard to say what else it can be—the view that *Cyphonautes* is a polyzoal larva is scarcely tenable.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Aug. 19.—M. Faye in the chair. MM. Jamin and Richard read the second part of their paper on the laws of cooling, and the cooling power of gases. The authors have determined the amount of heat abstracted by a gas from a warm solid placed in its midst.—A. and P. Thenard presented a memoir on the action of ozone on indigotic sulphate and on arsenius acid. The authors find that ozone decolourises three times as much indigo as the law of equivalents would lead one to suppose, and that this reaction takes place in two well-marked periods. Two-thirds of the indigo are decolourised, in the first of these periods, almost instantaneously, and one-third in the second period after the lapse of several hours. The authors ascribe this second action to hydric peroxide (*eau oxygenée*) formed by the ozone. The authors are led to doubt whether ozone is really a triple atom molecule, or whether it is simply oxygen in which is condensed a powerful selective force. They intend to thoroughly investigate this question.—M. Daubrée reported his examination of the meteorites which fell at Lancé and at Anthon (Loir-et-Cher) on the 23rd July, 1872. The Lancé stone weighed 47 kilogrammes; the one which fell at Anthon, 12 kilometres from Lancé, was much smaller. Their structure was granular, and some of the grains acted strongly on polarised light; they were evidently portions of the same mass. Specific gravity, 3·8. Elements found: iron, cobalt, nickel, copper, sodium, sulphur, chlorine, silicon, and oxygen.—Max Marie followed on the determination of the perimeter of the region of convergence of the series of Taylor, &c.—M. Mallard read a paper on the action of silicic anhydride and analogous oxides on sodic carbonate at a high temperature.—On the combined use of morphia and chloroform during surgical operations, and on a new mode of administering the latter. M. Demarquay, the author, convinced of the great danger incurred by the combined use of these agents, has abandoned it and devoted himself to the improvement of the apparatus employed for the administration of chloroform. The apparatus in question consists of a flannel mask stretched on a wire frame; the chloroform is poured drop by drop on its surface.—Observations on a note by Prof. Respighi on the solar protuberances, by S. Tacchini. The author asserts that no dependence can be placed on the details of any drawings of the prominences except when made with a telescope of large aperture.—M. Trève, in a paper on the magnet, mentions some experiments from which he deduces that the "transformation" of a bar of soft iron into a magnet requires a mechanical work and a molecular action of a kind as yet unknown. —"On the compressibility of Air and Hydrogen at high temperature" by M. Amgat. The author asserts that up to 320° these gases follow the law of Mariotte. M. Berthelot followed, on the distribution of a base between several acids in solutions. "On the aptitude of certain gases to acquire persistent-active properties under the influence of electricity" by M. Chalrier. The author finds that hydrogen when acted on by electricity possesses the power of uniting directly with the nitrogen of the air and of reducing newly precipitated oxide of silver, even after it has travelled some distance from the point where the electricity was allowed to act on it. M. G. Lechartier, in a paper on the reproduction of pyroxene and peridot, stated that he had succeeded in preparing these minerals by heating mixtures of their constituents.—M. P. Bert followed with "Experimental researches on the effects of changes of barometric pressure on the phenomena of life." In a very interesting paper of great practical importance as regards miners and divers working under great pressure, the author cited the case of an English company who in a single year lost ten divers out of twenty-four three of these died suddenly on coming to the surface, *i.e.*, at the moment of sudden release from a high pressure and seven after several months of suffering from paralysis also died. The author concludes (from a series of experiments of cats and dogs) that up to five atmospheres two or three minutes should be allowed for the pressure to decrease, above that much more time must be allowed, and at nineteen atmospheres five minutes per atmosphere at least is required. If the pressure is allowed to decrease more rapidly than this death is certain.—"Comparative researches on the absorption of Gases by the blood: estimation of Hæmoglobin," by M. N. Gréhant. The author describes a method of estimating Hæmoglobin by observing the quantity of carbonic oxide the blood will absorb. Application of Meteoric Metamorphism to the study of the black crust of grey meteorites, by M. S. Meunier.—M. A. Cheux describes a white Aurora Borealis observed at La Baumette near Angers on August 8, 1872, and says that great disturbance was observed on the sun on the morning of the 9th; he gives a view of the sun showing twenty-four spots.—Extracts from two letters from Messrs. Guiscardi and H. de Saussure relative to the late eruption of Vesuvius.—Appearance of a meteor in the department of Vienne, July 23, 1872 (extract of a letter from M. Daubrée). This was the meteor of which portions fell in the Canton of St. Amand, Loir-et-Cher, Vienne is forty kilometres distant from the places where the two portions of the meteorite fell.—M. Tellier read a note on the supersaturation of water. Water may be cooled 3° or 4° below zero in a glass vessel and still remain liquid in which state it may be violently agitated but a very sudden blow often causes its solidification. M. J. Gerard exhibited photographs of the interior of an aquarium.

August 26.—M. Faye, president.—Determination of the mutual actions of Jupiter and Saturn to serve as a base for the respective theories of the two planets, by M. Le Verrier.—In a note on the action of carbon and iron on carbonic anhydride at a high temperature, by M. Dumas, the author refutes a statement lately made by M. Durunfaut that these bodies do no not react unless hydrogen is present.—Mr. C. Peters announced the discovery of two new planets, 122 and 123. The planets are of the 11·5 and 12th magnitudes respectively.—New researches on the propyl compounds, by MM. Is. Pierre and E. Puchot.—In new experiments on spontaneous generation, by M. Donné, the author supports the well-known views of M. Pasteur.—Elementary theory of simple integrals and of their periods, by M. Max Marie.—On the physical constitution of the sun, by M. E. Vicaire. The author returns to the old theory of a comparatively cold nucleus which he regards as most probably liquid. He considers that the tremendous explosions of which the sun is the seat could not occur from the midst of a mass of dissassociated gases.—Notes were received from M. Brachet relating to the improvement of microscopes; from M. Lanale, relating to aerial navigation; from M. Clarke, relating to cholera; from M. Roussett, relative to certain questions concerning medicine.—On the spherical representation of surfaces, by M. A. Bibancour.—Letter from M. Gasparis, on a new mechanical theorem.—On ozone and hydric peroxide (*eau oxygénée*). M. F. Le Blanc sent a note relating to the paper by the Messrs. Thenard in No. 8 *Comptes Rendus*, 1872. The author states that in 1854 he discovered that ozone acted on water with the production of hydric peroxide.—Industrial employment of ozone for the destruction of the empyreumatic taste of whisky, and in the manufacture of vinegar, by M. Widemann. The author established a factory at Boston, U.S., where whisky was thus treated at the rate of 12,000 gallons per week. He also converted maize whisky into vinegar by diluting it with seven volumes of water, and then treating it in the same way.—On the divisions of a base between several acids in solution, dibasic acids, by M. Berthelot.—Action of cupric sulphate on normal urine, by M. Ramon de Luna.—M. P. Bert communicated a seventh note on the influence of change of barometric pressure on the phenomena of life.—On noctilucine, by Mr. T. L. Phipson. Noctilucine is the substance which is secreted by the various animals which are phosphorescent in the dark. The author believes that the same substance is secreted by certain plants (*Agaricus*, *Euphorbia*, &c.) and that it is also produced by the fermentation and decomposition of various vegetable and animal matters. The spectrum of this substance lies entirely between the lines E and F of the solar spectrum. —On the iodide of nitrogen, by Husson, *fils*.—M. Le Verrier presented observations of the August meteorites, from Greenwich, Lisbon, and at Volpeglino.—M. Chapelas announced, respecting the meteorites of the 8th, 9th, 10th, and 11th of August, that the mean hourly number was 33·5, a decrease of 6·4 on last year. The number for 1872 was only about one third of that for 1848. —A new communication from M. Pigeon, on the typhus of horned beasts, was submitted to the examination of M. Bouley.

PAMPHLETS RECEIVED.

English —The Lead and Zinc Mines of the Mendips: H. B. Woodward. —What Determines Molecular Motion, the Problem of Nature: J. Croll.— A Letter to the Marquis of Salisbury on the Public Health Bill: W. Child. —The Building and Ornamental Stones of Great Britain and Foreign Countries: E. Hull.—British Association for the Advancement of Science, Report of Committee on Science Lectures and Organisation, Past and Present —Quarterly Magazine of the Brighton Grammar School, Part II — Science and Art, a Sermon to the Memory of F. D. Maurice: Ll. D. Bevan. —Economy of Fuel in the Blast Furnaces for Smelting Iron: I. L. Bell — Quarterly Weather Report of the Meteorological Office, January to March, 1871.—The Vomiting of Pregnancy: E. Munro.—On the use of the Stethoscope of Obstetrics: E. Munro —A Puzzle in Rain and Air.—Proceedings of Geologists' Association, July.—Quarterly Journal of Education, July — College of Physical Science, Newcastle-on-Tyne, Prospectus for Session 1872-73 —A Discussion of the Meteorology of the part of the Atlantic lying north of 30° N. lat. for the eleven days ending February 8, 1870.—Charts and Diagrams to accompany ditto.

American and Colonial —Canadian Naturalist, July.—Indiana Journal of Medicine: T. M. Stevens, Vol III, No. 2 —Abstract of Reports of the Surveys of the Geographical Operations of India for 1870 71.—Abstracts of Specifications of Patents (Victoria) applied for from 1854 to 1866, No 1.—Metals: W. H. Archer —Report of the Coalfields, Western Part of Victoria.—Reports of Surveyors and Registrars for Quarter ending March 11, 1872, Victoria — Notes on the Post-Pliocene Geology of Canada: J. W. Dawson —The Popular Science Monthly, Nos 1–4.—The Australian Mechanic, No. 7, July, 1872. —Eighth Report of the Board of Visitors to the Observatory, Victoria.— Bulletin of the Museum of Comparative Zoology at Harvard.—Notes on the Ornithological Reconnaissance of Kansas, Wyoming, and Utah: J. A. Allen.

Foreign.—Zeitschrift fur Biologie, Vol. viii, No. 2.—Bulletin de la Société d' Anthropologie de Paris —Classification de 250 matières tannantes: M. Bernardin.—Memorie della Societa degli spettroscopisti italiani.— Matériaux pour la faune Belge, 2^{me} part.—Myriopodes: F. Plateau—Ofversigt. af kongl. Vetenskaps Akademiens Forhandling, Nos. 3, 4, 7.—Atti della reale accademia dei Lincei, tom. 25, Ann. 25, 1871-72.—Verhandlungen des naturhistorischen Vereins, Riga, Vol 1, 2, 1872.

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THURSDAY, SEPTEMBER 26, 1872

BALFOUR'S PALÆONTOLOGICAL BOTANY

Introduction to the Study of Palæontological Botany. By J. H. Balfour, A.M., &c., Professor of Botany, Edinburgh. (Edinburgh: Adam and Chas. Black, 1872.)

THE mastery of an alphabet by a child depends on his recognising and remembering the differences in form of the various arbitrary signs which we conventionally use to represent different sounds. Perhaps in the face of the alphabetic researches of Mr. John Evans we should withdraw the qualification—arbitrary. He may see a connection between the sign and the sound, and be able to give a reason for the various forms employed, and explain the influence of this horn or that loop superadded to the simple line in modifying the sound. We are, however, at a loss to discover what possible connection of affinity or even of analogy can exist between the signs O and Q and the sounds they represent. Whatever the recondite researches of the antiquarian may discover, the letters of the alphabet are practically recognised and universally received as arbitrary signs for particular sounds. The mastery of the alphabet is only the recognition and remembrance of the different signs and the sounds they represent.

This mnemonic education is not unfrequently the only education which some attain to, or perhaps are capable of. In science not a few are looked up to as masters whose extensive knowledge is nothing more than the faculty of discerning differences joined to a good memory. The rationale of the difference is another matter; perhaps they are mentally incapable of appreciating whether the distinguishing characteristics at once perceived by them are dependent on the absence of affinites or of analogies. The differences exist—that is their goal.

Many entomologists prosecute their labours on this low platform, and in botany the mere herbarium systematist occupies the same position. The one classes beetles, and the other plants as the lexicographer arranges words, by a method that exhibits their differences and permits easy reference.

But in no division of science is this class so common as in geology. The mere perception and memory of differences will give one a high class position as a palæontologist: and much useful work will such a one do—work necessary to the progress of science. True, it is not the highest class of work, and too frequently men who are experts at it aim at something higher. But deficient in the power of appreciating analogies, or recognising affinities in the points of resemblance, or their absence in the points of difference; deficient also in the exact knowledge of zoology or botany, such men have grandiloquently proclaimed and ignorantly and of course obstinately defended the most absurd opinions. Confined to the useful work of separating and recording different forms their services to science are most valuable, but the interpretation of obscure structures, and the higher problems of science, must be left to others able to deal with them.

No greater source of the evils we deprecate exists than the common but erroneous practice of separating palæontology as a science by itself. Palæontology has no grounds for recognition as an independent science. The organisms of an existing geographical province might be excluded from zoology and botany on the same grounds that are employed to exclude the plants and animals of a geological period. Zoologists are far ahead of botanists in getting rid of this error. Extinct forms have their place in every philosophic estimate of the animal kingdom. In botany, on the other hand, the study of fossil forms has been recently characterised as a non-scientific pursuit engaging the attention of the "geologist"; extinct plants are excluded from systematic works, and if dealt with at all in class books, all reference to them is eliminated from the general text, and they are confined to a page or two, or relegated to an obscure appendix.

Great praise is due to Prof. Balfour for introducing a different order of things. His manuals have always to a greater extent than any others published in the English language dealt with fossil plants; and he has now given us a special introduction to botanical palæontology. From the position obtained by the study of structural and systematic botany he deals with the problems presented by fossil plants

The earlier pages of the work are occupied with preliminary considerations as to the difficulties which present themselves in attempting to determine fossil plants, the different conditions of preservation in which their remains occur in the stratified rocks, the accepted classification of the sedimentary deposits, and the like.

The plan of the work itself is to treat of the plants as they are associated in the different recognised formations. Looking at palæontology as a separate section of the science of botany this is no doubt the obvious method of treatment, just as in geographical botany we deal with the different provinces of the surface of the earth and the floras which characterise them. If, however, the stratigraphical aspects had been subordinated to the systematic, a more valuable and instructive exhibition of the past vegetation of the globe would have been before us. For example, had the Coniferæ been traced from the first known occurrence of Abietineous wood in the Devonians, through the anomalous Araucarian wood and the Taxineous fruits of the Carboniferous rocks, the last types represented by the Walchias of the Permians and the Voltzias of the Trias, up to the appearance of the still existing groups in the Oolitic and Cretaceous rocks, an exhibition of extinct forms would have been given which would have conveyed to the botanist a clear and comprehensive view of the Order. In this way important light would be thrown on the present geographical distribution of orders and even genera. Look, for instance, at *Araucaria* and *Sequoia*,—two genera limited in numbers as well as in geographical distribution. We find the first represented by several species in our Secondary rocks, entirely absent from the Tertiaries, and at length banished to the southern hemisphere; while *Sequoia* appears in the Cretaceous strata, persists through the lower Tertiaries, and is now limited to a small geographical province in Western North America.

Nevertheless much may be said for the method adopted by Prof. Balfour. So long ago as the year 1828, Adolphe Brongniart detected a correspondence between the great divisions of the vegetable kingdom and the great epochs of the earth's history. He correlated the predominance of Cryptogams with the Primary epoch, of Gymnosperms with the Secondary, and of Angiosperms with the Tertiary. The discoveries of the half century that have elapsed since Brongniart published his views have confirmed the broad truth of his generalisations. Recently they have been expounded and illustrated by one who worthily follows his illustrious countryman in this particular field of study,—by the Comte de Saporta in the preface to his Tertiary Flora of the South of France.

Accepting this classification, which is so far both systematic and stratigraphical, Prof. Balfour prepares the student for dealing with the more obscure fossil remains by introducing each epoch with a *résumé* of the leading characters of the great group of plants which are found in it, drawn from their living representatives. The fossils characteristic of the various formations then follow in detail. The most recent observations are given. Look, for example, at the illustrations and descriptions of the fructification of the Cryptogamic plants of the Coal measures—the ferns, club-mosses and mares-tails—brought together here for the first time.

The numerous woodcuts and the admirable plates greatly enhance the value of the volume.

WM. CARRUTHERS

THE BRITISH MUSEUM PHOTOGRAPHS

Photographs from the Collections of the British Museum. Taken by S. Thompson. 1st Series. (London: W. A. Mansell and Co.)

AMONG all the varied purposes to which the art of photography has been applied, there is perhaps none for which it has proved itself more valuable than for the reproduction of ancient works of art. It matters not whether it be the sublime conception of some ancient Greek sculptor, the thorny-looking inscription on a Babylonian brick, or the stone hatchet of some prehistoric troglodyte, in each case the reproduction by the camera, if executed by a competent operator, will give a more vivid and faithful idea of the original than any drawing by however skilful an artist.

In the case of inscriptions, of minute patterns, of delicacy of form, or of the distinctive character of an object, the merely mechanical process, though not entirely without its drawbacks, possesses a great advantage over the skilful artist, inasmuch as it is entirely free from prejudice. The artist, however conscientious, is always prone to draw incorrectly such details as he does not understand, and to attempt some improvement in force and effect in those which he fully appreciates. It is only in the case of coins and of other small objects which it is necessary to hold in more than one light in order fully to discern the details, that a good drawing is preferable to a photograph; and then the question arises, what is a good drawing?

For rendering available to students the contents of a museum, photography is invaluable. By it the objects which, in many instances, it is impossible to study at leisure in their repository, are, as it were, rendered portable, and made available for extended examination at home, and for reference at a moment's notice. It is with great satisfaction, therefore, that we see this series of nearly a thousand quarto photographs of objects in our national collections issued to the public by Messrs. Mansell and Co. It is divided into seven parts—ethnographical and pre-historic, Egyptian, Assyrian, Grecian, Etruscan and Roman, Mediæval, and Seals, and one great advantage to the student is that he is by means of a comprehensive catalogue enabled to make his own selection of such photographs as come within his own particular province.

That the choice of the objects to be photographed has been judicious may be inferred from the fact that it has been made by the aid of Dr. Birch, Mr. Charles Newton, Mr. A. W. Franks, Mr. Murray, Mr. George Smith, and Mr. Walter de Gray Birch, all well known for their labours in the departments which they represent. Four of these gentlemen have also prepared the catalogue.

The photographs themselves are remarkably well and clearly executed, the figures in all cases being sufficiently large to make the details visible. We have but one fault to find, which it is to be hoped may be easily remedied—the absence of any scale on the photographs, and of any dimensions in the catalogue. In the case of some of the pre-historic and ethnographical objects it would also be an advantage if further particulars were given as to the localities from which they were obtained.

The catalogue is accompanied by an interesting introduction from the pen of Mr. Charles Harrison, giving a good general view of the progress of human civilisation, which the objects photographed illustrate, and also giving the rationale of the whole series. We cordially concur in his hope that each local museum will have its objects photographed, and that the plates like these may be made accessible to the public at a fixed moderate cost. In the meantime we commend these illustrations of our rich national collection to the readers of NATURE.

OUR BOOK SHELF

Autumns on the Spey. By A. E. Knox, M.A., F.L.S. Author of "Ornithological Rambles in Sussex, &c. (London: Van Voorst.)

WE have seldom come across a book in which the *dulce* and the *utile*, science and amusement, are so happily combined as in the modest little volume before us. Mr. Knox's main object, apparently, in spending his autumn on the Spey, was to fish for salmon in that trying river; and some of his wonderful achievements in this exciting occupation are narrated in an almost fascinating, and certainly unpretentious manner, in a few of the chapters of his booklet. But it would be a great mistake to consider this merely a book of sport, and Mr. Knox nothing more than a genial "piscator;" he has already proved what is confirmed by this his most recent work, that his knowledge of British zoology, and especially ornithology, is extensive and thorough. To any one who desires to see the report of a trained and patient observer on the zoology, and even geology, of the basin of the Spey and and of contiguous districts, we would with confidence recommend Mr. Knox's work. It contains much that is valuable and interesting on these subjects, and a good deal that is new to many. Quite charming and very curious is his account of the *modus operandi* of a family of crossbills (*Loxia curvirostris*) which he watched while standing under a tree, a few inches above his head, busily engaged at their marvellous employment of splitting the fir cones and extracting the seeds. His observations on the habits of the water-ouzel, its procedure under water, and the food it seeks there, quite redeem that lively little bird from the imputation of being a destroyer of salmon-spawn, and prove him to be the salmon fisher's best friend. But, indeed, the charms of Mr. Knox's book are many, and will be deemed an acquisition by all who take an interest in British zoology; to those who are both fishers and naturalists it will afford a rich treat. The tail-piece to the book is a beautiful woodcut of a salmon, having underneath the punning legend, "*In spe-vivo.*"

Physical Geography. By Sydney B. J. Skertchley, F.G.S., H.M. Geological Survey. (London: Thomas Murby.)

THIS is one of "Murby's Series of Science Manuals" intended for use in schools. It seems on the whole creditably done, the information conveyed is valuable, and in the main trustworthy, the author occasionally drawing on his own experience for illustration. Amid the many manuals on the same subject competing for favour this deserves to take a place, though the few illustrations introduced are wretched, and there is an occasional attempt at fine writing.

Révue Photographique des Hôpitaux de Paris. Publié par Bourneville et A. de Montméja. 4$^{\text{me}}$ Année. Avril, 1872. (Paris: Delahaye.)

THIS enterprising little publication deserves success. The number before us, which, however, is only interesting to our medical readers, contains three photographs (about 4 in. by 3 in.), one of a calcified enchondroma, and two of a remarkable case of encephalocele. One of these exhibits the whole infant, the other the upper part of the trunk. The details are very clearly visible, and there is an account of the case by P. Budin. The *Révue* contains also a good report of recent anatomical physiological and surgical work.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Potato Disease

I FEEL very much interested in the attempt you have made to connect the potato disease with cosmical phenomena, and I quite agree with you that although the *Peronospora* may be the proximate cause, yet for the ultimate cause we may have to look to a very different set of circumstances.

The researches of Baxendell, Meldrum, Smyth, and others, go to show that the convection currents of the earth are influenced by the state of the solar surface. Now surely anything that influences the motions of our atmosphere may readily be supposed to influence the distribution and activity of those disease germs that are now believed to be present in the atmosphere. Are not various kinds of blight associated with the prevalence of certain winds?

In referring to the five great visitations of the sweating sickness you say quite truly that we have no means of ascertaining the condition of the surface of the sun during those years. Nevertheless, *indirectly*, we may, I think, come to some sort of conclusion more or less probable regarding the sun's surface in those years.

This may, perhaps, be done by means of records of the Aurora Borealis. I have not access at present to the great catalogue of M. de Mairan, and I will, therefore, confine myself to the list of auroral appearances given by Mr. Jeremiah in your columns for November 17, 1870. Very great and extensive auroral outbursts are known to occur during years of maximum sun-spots, and auroræ are phenomena which appeal too much to the imagination to remain unnoticed in an unscientific age.

If, therefore, we can tell the years of very great auroral outbursts, we can at least approximate to those of maximum sun spots.

Now (quoting from NATURE) "in 1574 Camden and Stow inform us that an Aurora Borealis was seen for *two* successive nights, viz., the 14th and 15th of November, with appearances similar to those observed in 1716, and which are not commonly noticed. The same phenomenon was twice seen in Brabant in 1575, viz. on 13th February and 28th September, and the circumstances attending it were described by Cornelius Gemma, who compares them to spears, fortified cities, and armies fighting in the air." This has every appearance of a widely extended and great series of outbursts, and we may, perhaps, suppose that the maximum was not far from 1575.

Again we learn that "on September 2, 1621, the same phenomenon was seen all over France, and it was particularly described by Gassendus in his 'Physics,' who gave it the name of the 'Aurora Borealis.' Another was seen all over Germany in November 1623, and was described by Kepler." Giving equal weight to these two appearances, we may place the maximum in the year 1622.

Again we learn that "in 1707 Mr. Neve observed one of small continuance in Ireland, and that in the years 1707 and 1708 this sort of light had been seen no less than *five* times." We may in this case place the maximum in the year 1708.

We have thus selected as years of maximum auroral disturbances the years 1575, 1622, and 1708, and if they correspond approximately with years of maximum sun spots, we should expect the distances between them to be divisible by 11·1, which Wolf as well as De La Rue, Stewart, and Loewy, agree in representing as the solar period. Now the difference between 1575 and 1622 is 47 years—a period not very different from four solar periods, or 44·4 years.

Again the difference between 1622 and 1708 is 86 years—a period not very different from eight solar periods, or 88·8 years. Furthermore the difference between 1575 and 1708 is 133 years—a period not very different from twelve solar periods, or 133·2 years.

Finally the difference between 1708 and 1816·8, the period of one of Wolf's well-ascertained spot maxima, is 108·8, which is not very different from ten sclar periods, or 111·0 years.

Assuming, therefore, that 1575 is not far from a period of maximum sun spots, and going backwards by steps of 11·1 years, we are led to the following dates:—1552·8, 1530·6, 1519·5, 1508·4, 1486·2, as years of maximum spots, whereas the dates of sweating sickness were 1551, 1528, 1517, 1506, 1485, and the differences between the two sets are as follows: 1·8, 2·6, 2·5, 2·4, 1·2, the mean being 2·1 years, and the difference always in the same direction.

It is, of course, hazardous to place much confidence in these results; nevertheless, it is worthy of remark that the greatest difference between observation and calculation from hypothesis, recorded in the communication, is 2·8 years, whereas it might sometimes have been 5·6 years on the supposition that there is no truth whatever in the hypothesis.

I shall only remark in conclusion that when we have arrived at the position of being able to explain by a probable hypothesis the cause of spot variations, we may perhaps be able to test our conclusions by means of these early notices of the Aurora Borealis.

B. STEWART

HAVING been from home, it is only now that I have read your very interesting article of Sept. 12, on the Potato Disease.

It is certainly most desirable that "an investigation into the origin, cause, and remedies" thereof by the ablest of our scientific men should be promoted; but it appears to me that this is a case for private contributions rather than an appeal to Government. I would, therefore, suggest that a fund be raised by subscription to supply the means of offering three prizes for the above object: the first I should hope would not be less than 500*l.*, the second and third 300*l.* and 200*l.*

These sums would offer an inducement to the ablest men to devote to the object a portion of that time and talent which, with many of them, forms the chief (sometimes the only) source from whence their income is derived.

The judges might be appointed by such of the subscribers as could meet at a given place after due notice.

The sum required, including expenses of advertising, &c., would not be large; there ought to be no difficulty in raising it when we consider what a large interest is at stake.

I should be happy to subscribe 5*l.*

Richmond, Surrey, Sept. 23 M. MOGGRIDGE

On the Substance Exhibited at the British Association, Brighton, by Mr. P. L. Sclater, and stated to be the Ossified Notochord of a Fish

NEARLY every one frequenting the Zoological Section at the British Association at Brighton must have seen and been puzzled by a substance exactly resembling in external appearance a slender willow twig when perfect, two feet or more in length, and pointed at both ends.

This substance was exhibited by Mr. Sclater, and pieces of it were freely distributed by him for examination.

I was, unfortunately, not present when he read his paper on the subject; but I gathered that he had said that the substance had been described to him by the person who sent it as occurring in the back of a fish, and that Mr. Sclater called it an ossified notochord. A drawing of the fish was exhibited.

I further heard that Mr. Gray, of the British Museum, regarded the substance as the axis of one of the Pennatulidæ, and that this opinion was held by several other naturalists also.

I first became acquainted with the substance at the Kew Herbarium, where a piece of it was shown me by Mr. Berkeley, it having been given to him by Prof. Thiselton Dyer; and I was told that Dr. Hooker had examined it with the microscope, and rejected it as certainly not vegetable.

It was almost impossible to conceive of the substance being the notochord of a fish. No fish's notochord is composed of longitudinal fibres, nor has a structure at all resembling that of the substance in question; and moreover a notochord in such a fish as a lamprey, in which it is persistent, is much thicker in proportion to its length than are these calcified rods. Further, the tendency is for a notochord to ossify peripherally, and form rings of bone, not a hardened central core.

On reaching Oxford from Brighton, I got Mr. Robertson to give me a specimen of *Funicularia quadrangularis*, one of the Pennatulidæ, which was preserved in spirits. I found it had a long slender flexible core, exactly similar in appearance to Mr. Sclater's substance, but quadrangular in section instead of circular. The core was about two feet and a half long, and pointed at both ends. Microscopical examination of longitudinal sections of the core, when treated with acetic acid, gave off carbonic acid in quantities, and showed a structure almost exactly resembling that observed under similar circumstances in Mr. Sclater's substance.

I then looked into the literature of the subject, which fully confirmed me in the opinion that the substance in question is the core of one of the Pennatulidæ. A few statements, culled from the two works I consulted, may be interesting to the readers of NATURE. The works were "Anatomisch-systematische Beschreibung der Alcyonarien," von A. Kolliker, Erste Abtheilung: Die Pennatuliden. Erste Halfte (Frankfort: C. Winter, 1870). "Icones Histologicæ, oder Atlas der Vergleichenden Gewebelehre," herausgegeben von A Kolliker. Zweite Abtheilung, Erster Heft. Die Binde-substanz der Cœlenteraten, p. 158 (Leipzig: W. Engelman, 1866).

The Alcyonariæ, a sub-order of polyps, are divided into three groups:—(1) Alcyonidæ; (2) Gorgonidæ; (3) Pennatulidæ.

The Pennatulidæ consist of hard and soft parts. The hard parts appear in most varieties in the form of an inner calcified axis, which in size and position is like that of the Gorgonidæ. It is to be considered as calcified connective tissue, is entirely and completely enclosed within the substance of the polyp colony, and is pointed at both ends.

The Pennatulidæ are thus divided:—

I. Pennatulidæ with polypbearers bilaterally symmetrical.
 A. Polypbearers feather-shaped in Pennatuleæ.
 B. Polypbearers leaf-shaped in Renillaceæ.
II. Pennatulidæ, with polyps arranged radially.

The Pennatuleæ break up into (1) Penniformes; Pennatuleæ with a well-marked feather-shape; (2) Virgularieæ; Pennatuleæ with a long, narrow polypbearer, and small leaves or polyps resting immediately on the axis. To this latter group belongs the genus *Funicularia*, and probably also the genus to which Mr. Sclater's specimen belongs.

With regard to the fine structure of the hard axis of Pennatulidæ, I have gathered the following from Kölliker's "Icones," p. 158.

The axes of Pennatulidæ consist of calcified horny substance, arranged in concentric lamellæ about a central core. The lamellæ are pierced by peculiar soft radial fibres, which, however, are well defined in certain species only. The organic basal substance shows an extremely well marked fibrillar structure. The axes are less firmly calcified than those of the Gorgonidæ, and are thus for the most part able to be cut with a knife and bent.

According to the analyses of Fremy (Ann de Chimie, 1855, t. xliii. p. 98), the axis of *Pteroeides spinosum* contains from 31 to 40 per cent. mineral matter, and that of *Pennatula rubra* from 45 to 48 per cent.

A drawing is given by Kolliker of a transverse section of the axis of a Virgularia (*Lygus mirabilis*) prepared by grinding, which shows a white central core, surrounded by a broad brownish cortex, which is marked with concentric and radial lines.

If the axis of a *Lygus* be treated with acetic acid, a development of carbonic acid takes place. It becomes soft, and allows the following structure to be made out:—

The bulk of the axis consists of a fibrous tissue which resembles ordinary fibrillar connective tissue in the most deceptive manner, and consists of very fine fibrillæ, which run parallel to one another in a wavy fashion, and which can be isolated from one another. On the surface of the axis is a yellowish cuticle.

Drawings are given of longitudinal sections of the axis of *Lygus mirabilis*. It shows the peculiar broad transparent radial fibres crossing the finer longitudinal ones. In another figure of a similar preparation from *Funicularia quadrangularis*, these radial fibres are less marked, but the cavities containing them appear as oval apertures in the section.

Reference is made to Quekett, Lectures on Histology, II., and Histological Catalogue, I., where the structure of the axes of *Pteroeides*, *Lygus*, and *Funicularia* is described, but the radial fibres mistaken for canals.

I think any one who has examined Mr. Sclater's substance, and very many have had such an opportunity owing to his kindness in distributing pieces, will find that both in external characteristics and internal microscopical structure, it conforms very closely to the description given here from Kolliker of the axis of the Pennatulid. I have sent the Editor of NATURE some pieces of the axis of *Funicularia quadrangularis* in case any one cares to compare the two substances, and has not the material at hand. In the mean time I cannot but conclude that Mr. Sclater has been misinformed, and that we are very unlikely ever to see that very marvellous fish in the flesh.

H. N. MOSELEY

Ocean Currents

HAVING just returned from a sojourn of nearly two months amongst the White Mountains, I am now for the first made aware of the publication, both of my last note on Ocean Currents, and also of Mr. Croll's reply. I have not been disposed to enter into an extended discussion of this subject, knowing that it cannot be properly treated without the use of mathematics, in short essays suited to NATURE, and doubting whether the discussion could be made either acceptable to its Editor or edifying to its readers. In my last note, therefore, I endeavoured to be as brief as possible, and considered only the more simple form of the conditions of the problem, as expressed by differential equations, showing the relations between the forces, resistances, and the differentials of the motions, and showed that the deflecting force eastward exerted upon a pound of water or any body in moving toward the pole with a velocity of one mile per day, and which must be sensibly the measure of the resistance of friction, is of the same order near the parallel of 45° as the action of gravity on the same body upon a gradient of 6 ft. from the equator to the pole; and from tidal considerations it was inferred that the resistances to the slow motions of ocean currents may be very much less than the action of gravity upon any body apon a gradient of 6 ft. in the distance of a quadrant.

If, instead of considering the differential equations of any problem, and endeavouring to satisfy them directly, we adopt the less simple method, and consider the integrals of these equations, and endeavour to satisfy them directly, the method, though less simple, is entirely legitimate, and we should obtain the same results. This is substantially the method adopted by Mr. Croll; and from considering the problem in this way, he comes to the conclusion that the deflecting force eastward, which is the measure of the resistance, is at least 1,500 times greater than the action of gravity on a gradient of 6 ft. from the equator to the parallel of 60°; and as the velocity of the pound of water eastward, and that toward the pole, are probably about of the same order, and consequently the resistances, he justly infers that the resistance to the motion toward the pole must be overcome by some other force than that of gravity due to the assumed gradient. But it may be shown by the other method of considering the problem, by assuming a motion towards the pole of one mile per day, that the forces which overcome the resistances in both directions are about of the same order. This very great difference in the results obtained from the two methods of considering the problem, indicates that there is some great fallacy somewhere which needs looking after.

Mr. Croll is misled by adopting the erroneous principle that the amount of work performed by gravity upon a falling or descending mass is in all cases expressed by the mass multiplied into the height through which it descends, or that the foot-pound is a unit of work. The amount of work required to give velocity to any body, or overcome any kind of frictional resistance, is expressed by the intensity of the effort, regarded as constant, multiplied into the time of action. The intensity of the effort of any force, as of gravity, is explained by the mass multiplied into the velocity which such force can produce in a unit of time. If we, therefore, put g, m, t, and v for the force, the mass, the time, and the velocity respectively, we shall have, putting W for the amount of work performed by gravity,

$$(1) \quad W = mgt = mv,$$

that is, the amount of work performed is expressed by the momentum. Now this amount of work is stored away in the moving body, and remains until it is used in overcoming resistance of some kind, as friction or the inertia of other bodies, and W is exactly the expression of the working power which has been communicated to it. But if the moving body has been subject to resistances, as of friction, during the time t, then we shall have

$$(2) \quad W = mv + ft$$

putting f for the coefficient of friction, and supposing it to be constant. In this case mv expresses the amount of work left which has not been expended in overcoming the friction during the time t, and of course in this case we cannot get all the work back again which has been expended, at least mechanically.

If we now suppose a body to fall in a vacuum through the space s, if the amount of work performed by gravity upon it, and the working power communicated to it, is expressed by the mass multiplied into the height through which it has fallen, we shall also have

$$(3) \quad W = ms = \tfrac{1}{2}mgt^2 = \tfrac{1}{2}mvt.$$

Hence, comparing the preceding expressions of W, we have $mv = \frac{1}{2}mvt$, which is impossible; and therefore, if the former expression of W is correct, the latter is not.

Again, illustrating by a special case, if we suppose a body, of which the momentum is mv, to move upon a level plane without friction, and the plane to curve up in the direction of motion, and also suppose another body with half the mass and double the velocity, of which the momentum is $\frac{1}{2}m \times 2v = mv$, m and v being the mass and velocity of the first body, it must be admitted that the amount of labour expended in giving both bodies the momentum mv is exactly the same, in the latter case the intensity of the effort being half as great, but the time of action twice as long; but the momentum will carry the latter up the slope to a height four times greater than that of the former, and after descending again to the plane, both will have the same momentum, and the same amount of labour would be required to bring each to rest, and consequently both have the same power of doing work. But the mass of the latter multiplied into the height through which it has descended is double that of the former, and hence these products do not express the power of doing work which gravity has communicated to them.

In the case in which the descending body is resisted by friction, we have seen (2) that neither the mass multiplied into the height of descent, nor the momentum mv which the body has on arriving at the level plane, expresses the whole action of gravity, and the resistance fv may be so small as not to affect sensibly the amount of work expressed by mv, or it may be so great that mv may be neglected, in comparison with ft. The value of ft may also be so great that the value which mv would have (1) in the case of a free body falling through the space s, would be almost infinitely small in comparison with the whole expression of W (2). In the various cases, therefore, which may be supposed, in which friction may be either very small or very great, so that in the former case the effect of the resistance might be scarcely sensible, and in the latter it might take the action of gravity a long time to drag the body down through the space which it has to descend, we cannot suppose that in all these cases the whole action of gravity is expressed by the same number of foot-pounds, supposed to be units of work.

If, therefore, Mr. Croll's pound of water were moved from the equator to the parallel of 60° by the action of gravity without any resistance, the momentum which it would have on arriving there would express the work done by gravity upon it, and not the six foot-pounds, and the work would be done in a very short time in comparison with the time in the real case of nature; but when it is dragged down there through all the resistance which it suffers, at the rate of a mile per day, as we have supposed, the amount of labour which gravity performs is very many times greater than that expressed by the momentum which the pound of water would have on arriving there without resistance; and with regard to the six foot-pounds, we have seen that the work is no more comparable to them than a surface is to a solid.

Again, if we suppose the gradient upon which gravity acts to be only one foot in the distance instead of six, and the resistance to the pound of water to be as the velocity, then the water would move with only one-sixth of the velocity in the other case, and the water would be six times as long in reaching the parallel of 60°, but the energy of the action of gravity would be only one-sixth as much, and hence the work would be the same, being carried on six times as long in the latter case with one-sixth of the energy. But then the same work would be represented by one foot-pound, if that is a true unit of the work instead of six. In this case also the deflecting force eastward would be only one-sixth as much, but the time being six times as long, the same amount of work would be done, and this would be sensibly the same as that which would be required to give the pound of water a velocity of about 760 miles, as Mr. Croll has it, but really double that amount.

I am well aware that in the action of machines in which force is balanced against force, and consequently the times of action are the same, the amount of work may be expressed by the forces multiplied into the spaces through which they act; but in all cases in which the times differ, the amount of work cannot be expressed by any unit into which the element of time does not enter. The amount of work required to produce a velocity of 760 miles per hour is a function of the time, and proportional to the time where the force is constant, and cannot be measured by foot-pounds.

With regard to the argument based upon M. Dubuat's experiment, the matter briefly and fairly stated stands thus: according to the experiment, water will not flow unless acted upon by a force equivalent to that of gravity upon a given gradient, which makes the force required to move it about fifteen times greater than the horizontal component of the moon's disturbing force which produces the tides. But this force of the moon does move the water of the ocean, and therefore M. Dubuat's experiment is not applicable to water of great depth as of the ocean, and the argument fails. It is true, as Mr. Croll states, that the two cases of motion are somewhat different; in the case of the tides the water from top to bottom flows in the same direction, while in the other the upper and lower strata flow in contrary directions, and the resistances to the lower motions are no doubt greater. Six of the nine feet therefore of Mr. Croll's gradient should probably be given to the lower currents, and only three to the upper ones. But Mr. Croll admits that in the case of the tides a gradient of one inch is sufficient to move the water. A gradient of three feet only, therefore, ought to be sufficient to move the upper half, which would correspond somewhat to the case of tides in an ocean of half the depth. The observations of Col. Graham show that the water of Lake Michigan, about 700 feet deep, readily yields to the moon's disturbing force which causes a tide at Chicago with a range of nearly two inches.

Cambridge, Mass., Sept. 7 WM. FERREL

Spectral Nomenclature

IT seems almost absurd that a subject of such interest, and, as I think, importance, as that of Spectral Nomenclature should be discussed from opposite sides of the globe alone; so it may be hoped that it will not have been allowed to end with Prof. Young's remarks upon the one or two points in which he differs from me, but will have been taken up by others at a less distance. There is a great deal more to be said about it; but probably I should not have troubled you again just yet but for the obligation I feel to disclaim credit which he gives me for what is not mine.

Doubtless it is not necessary, but the obligation is the same; and a mistake into which Prof. Young has fallen is open to others.

The map which he wants—to be based on inverse wave-length or rapidity of vibration or pitch—is, I believe, in course of construction by Dr. Huggins, to whom, and not to me, is due, I think, the first idea and the proposal. Prof. Young has probably associated my name with it through a lecture delivered by my brother at Glasgow, in 1869, in which it was advocated.

I also wish to acknowledge that I was not aware of the fact which has now been so decidedly stated, that the coronal green line is certainly *practically* identical in position with 1474 (K), as tested by direct comparison. Indeed, I was ignorant that such a comparison was possible, having supposed that the line in question was only visible during eclipse. I ought, of course, to have referred to Prof. Young's "Preliminary Catalogue," and probably should have done so had I been in a house instead of in a tent, a few score of miles from the nearest station. But in truth it did not occur to me that there could be any certainty about the position of a line which, as a coronal line, had never been fixed by measurement. I may now venture to ask, What guarantee was there that No. 31 of the "Preliminary Catalogue" was "the coronal line," anterior to the Dodabetta measurement? I do not question it now, but I should like to know if the presumptive identity is supported by any characteristic difference between that line and those which are presumably due to the chromosphere. There is still a link wanting.

However, admitting the identity, and therefore the accuracy of the assigned position, we may still believe what Prof. Young says he would be glad to see proved, that "the apparent coincidence (with the iron line) is merely a very close juxtaposition." More than this: even were a very much higher dispersive power to show no resolution of the identity, should we not still be in nearly the same position as to any inference to be drawn therefrom? Evidence of physical relation between metals which present one or more lines common to both spectra may, indeed, eventually be shown (by the improbability of so frequent an accidental concurrence) to amount to proof. But this must be a prior step. To conceive it taken, and then to apply the like reasoning by analogy to the case of the single coronal line tallying with an iron line, seems to me speculation of the second order. Undoubtedly it would be matter for congratulation to be relieved from the liability to temptation of this kind by definite disproof. In the meantime, I cannot but regret that Prof. Young has half neutralised the good of a plain disavowal of belief in the ferrous interpretation of the coronal green line, by hazarding the query whether it may not "turn out" to be quasi-ferrous.

I am sorry—to return to the subject of nomenclature—that your respondent does not agree to my objection to "D_3." Is it not plain that such a designation is haphazard? The association of idea is with D_1 due to sodium, instead of with the origin or source of the line. It tells nothing beyond the position, roughly, in the spectrum, by reference to a position which we happen to be familiar with, but with the occupant of which it has no connection otherwise. The name, in short, has no foundation in principle; and that, I apprehend, is a lack of the first requisite in a scientific name.

The objection to Greek alphabet letters is of a different character, but not less easily answered. It is very true that, through the exertions of Prof. Young and others, "the whole Greek alphabet would not suffice to name one in three of the lines" already known; but it would nevertheless suffice (as in the somewhat analogous case of the stars) "to express as many as the memory would require to hold." There is ample precedent. The principal lines of the elements, like the principal stars of constellations, are known to some extent by Greek letters; and as for the difficulty in respect of order, there was a time when the "lucid" stars, though very many in number, and having no very clear claims to precedence, were only known individually by personal names. Yet no sooner did a Bayer rank and name them according to apparent brilliancy, by Greek letters (to say nothing of the Roman), than the advantage of a fixed nomenclature was recognised and his work accepted; although observation must have shown that the assigned order was not always strictly correct. So would it happen now if, the lines having been lettered, further knowledge should show that the established precedence was not quite all that could be wished. The evil of slight incorrectness of this kind would be felt to be trifling compared with that which would result from an unsettling of a nomenclature established solely for convenience and involving no theory.

This would not prevent nor conflict with, neither would it render unnecessary, a far more extended tabulation depending on refrangibility. On the contrary, the want of such a classification and means of indication would be felt as soon as precise tabulation should come to be undertaken. In Kirchhoff's solar chart we have, graphically, something like what is wanted in a much more general, numerical, and tabular form—an example of a catalogue of lines. Charts are very useful, but not most handy; and they are not susceptible of such ready improvement and extension. The accumulation of results of spectroscopic research must sooner or later take the form of a catalogue of lines, from all sources, arranged in order of refrangibility; designating individuals (for special reference) where possible, according to their parent element or compound, their physical source, their cosmical habitat, or other characteristic and distinguishing indication, implied under the system of nomenclature which may be adopted; upon which would follow such details as to character (including intensity, width, definition, complexity, variability, &c. &c.) as present knowledge or future research may represent as suitable material for incorporation.

So long as spectroscopic analysis is content to remain in its earliest stages—and it must be allowed to be still in its infancy, though a giant from its birth—the student and experimentalist may to a very considerable extent learn by heart or by practice such spectra as he needs; but this can never suffice for all purposes. Accumulation is continually going on, and products must be stored. Let that be once acknowledged and the task attempted, and it must follow that, no matter how rigorous and precise may be the system of tabulation, there will be not only room, not only gain, but a positive necessity for an intelligible use of that kind of descriptive indication which is only to be found in scientific classification and nomenclature.

A general catalogue such as I contemplate would command, if compiled with even moderate knowledge and care, a very general acceptance. Unquestionably it would be extended, modified, improved upon, by subsequent work; but, so far as nomenclature is concerned, it would probably undergo but slight alteration—the less the better. It would form a basis on which any number of special catalogues might rest, without interfering with its permanence as a catalogue of reference.

I do not pretend to say that the task is a simple one; quite the reverse. But, then, all the more honour to whoever accomplishes it.

J. HERSCHEL

Bangalore, July 29

Jeremiah Horrox

I OBSERVED in a number of NATURE some three weeks ago an inquiry relative to J. Horrox, the astronomer. My wife is descended from Horrox, and I knew that one of her friends had his life, but have not been able to find it till now. The book has just been sent to me here.

The Rev. Jeremiah Horrox was born in Toxteth Park, near Liverpool, in 1619, and died in 1641, aged 22.

The life is by the Rev. A. B. Whatton, published by Wertheim, Macintosh, and Hunt, 24, Paternoster Row, 1859, and includes a translation of his discourse on the Transit of Venus.

HENRY HOLIDAY

Muncaster Castle, Ravenglass, Carnforth, Sept. 20

Millions of Millions

WHY do not Messrs. Ranyard and Co. adopt the late Benjamin Gompertz's most convenient notation of prefixing a circle to the first significant figure, or suffixing a circle to the last significant figure having therein a digit for the number of zeros employed?

Thus: ·⑤718 is ·00000718
And 718⑥ is 718000000

S. M. DRACH

74, Offord Road, N., Sept. 17

Analogy of Colour and Music

IN NATURE, No. 150, p. 393, a letter from Mr. G. C. Thompson is headed "Correlation of Colour and Music." As this letter refers to a paper of mine published some time ago, permit me just to say that Mr. Justice Grove has in your journal objected to the use of the word "correlation" employed in this sense. Entirely coinciding with the opinion of the eminent parent of this work, I wrote as follows in NATURE for February 17, 1870 :—"*Analogy* is certainly far more appropriate to express what is merely a parallelism, and not a necessary or complementary relationship between light and sound." In the subsequent letter on this subject you adopted the word "analogy;" pardon, therefore, my pointing out an obviously accidental "reversion to the primitive type" which appeared in your paper Sept. 12.

Sept. 16 W. F. BARRETT

The Fringes on the Lighter Side of the Rainbow

AT the place referred to by Mr. Thompson in NATURE (No. 150, p. 393) I merely followed Sir John Herschel; expressing myself, it is true, not very accurately, in my anxiety to save space in NATURE at the end of a letter already too long. If Mr. Thompson refers to Sir John's "Meteorology," sections 219 to 224, and still thinks the point requires fuller elucidation, he may possibly supply the deficiency by devising an experiment to prove that the width of the fringes does not vary inversely as the diameter of the drops

In answer to Mr. Thompson's concluding question, perhaps somebody else will furnish the latest intelligence. I do not know what has happened in the last five years, and I do not know what you call violet; but I believe that in 1867 the extreme rate of vibration for visible rays was about 801 million millions a second. C. J. MONRO

A Curious Phenomenon

A VERY curious phenomenon was witnessed here on Wednesday afternoon last, September 4, about three o'clock, in a westerly direction. A somewhat heavy thunderstorm, originating towards the south, had divided its fury before reaching this immediate neighbourhood, one branch passing N.E. towards the Pennine Hills, the other taking the N.W. course, that to the N.E., however, being more violent. As the storm was passing, a stream—*apparently* of water, and fully six inches in breadth—shot with considerable speed from the vicinity of a dark, fiery cumulus across a rain cloud of a very deep blue, murky tinge. Its passage, as witnessed by my boy from its commencement, was similar to that of a rocket, at first assuming a quivering motion, then darting suddenly forward, for some distance horizontally, afterwards obliquely. Its apparent length would be fully twenty yards, being of a very light slate colour. After I saw it the phenomenon remained about two minutes; but its total duration would be not less than five, vanishing gradually during its whole length.

Whatever the phenomenon itself—or its cause, its upward course was certainly very striking, and to me unprecedented—the impression on some people's minds being that it was water drawn up from Lake Ullswater into the clouds by the lightning!! A terrific storm of thunder and lightning occurred on the previous evening at 9 P.M., when several fatal accidents were reported. T. F.

Blencowe School, Cumberland, Sept. 7

APPEAL FOR SKELETONS OF WILD SPECIMENS OF THE LARGER CARNIVORA FOR OUR MUSEUMS

NEITHER in the Museum of the Royal College of Surgeons nor in that of the University of Oxford is there a skeleton of a wild lion or a wild tiger, and it is probable that there is no such skeleton existing in England. The preparations in our Museums, illustrating the anatomy of the larger carnivora, are almost without exception derived from menagerie specimens.

Lions breed well in confinement, and hence an ordinary menagerie specimen may not only itself have been during its whole life confined in a cage, but its ancestors may have suffered a like fate. At all events it has been trapped whilst still young, and reared in confinement, as is usually the case with the menagerie tiger. Now an animal confined in a narrow space from its youth upwards never has free play for its muscles, and as its food is provided for it, is never called upon to exert them in a violent manner. The result is that the bony framework on which the muscles act never attains in such specimens its full development, and the ridges and inequalities on the bones corresponding to the origin and insertion of the muscles are not well marked. Moreover, menagerie animals, as is well known, very frequently suffer from diseases of the bones, and the marks of these diseases may be seen on many of the skeletons in every anatomical museum. Now, it is of great importance to possess perfect skeletons of adult wild large carnivora, both for general study, and more especially for comparison with the remains of similar carnivora which are to be found in the more modern geological deposits in Great Britain. Considering the number of tigers and lions which are annually killed by English sportsmen, it is surprising that this desideratum has not yet been supplied. The reason probably is that sportsmen generally do not know that it exists, or do not understand how a skeleton should be prepared. The sportsman is usually content with preserving the skin of his tiger or lion; but no doubt there are many who would gladly aid the cause of science by preserving the skeleton as well, if they knew how much the result of their labours would be valued at home. I propose here to give a few simple directions for the rough preparation of skeletons for transmission to England, merely premising that I trust that if any sportsman may be induced by reading these notes to send home a skeleton, that he will send it to the Oxford Museum, in which I am especially interested, and I hope some old University man may help us in this matter. Any packages should be addressed to Prof. Rolleston, Museum, Oxford. Skeletons of other wild animals are, of course, of great value, and will be most gladly received; they also are too frequently only to be got from menageries.

Directions for Preparing Skeletons.—The skin having been removed from the animal, the abdomen should be slit open, and all the viscera extracted. The limbs should then be severed from the body, the scapula or blade bone being left attached to the fore limb, the hind limb being removed at the thigh joints, and care being taken that the articular surfaces are not injured in the process. The flesh should now be removed roughly from each of the limbs with knives; the several bones which go to form each limb should if possible be allowed to remain attached to one another. On no account should the small bones of the hind or fore foot be separated from their attachments. Mr. Flower, indeed, advises that the skin be not removed from the feet at all. The limbs being thus roughly cleaned, they should be placed in water for several hours to allow the blood to soak out, and they should then be placed in the sun till dry. The head should be disjointed from the neck, and the flesh cut off it. It is most convenient to commence with the strong muscles of the jaw. After these have been cut through, the ligaments which hold the lower jaw in place may be divided, and it may be separated from the skull. The tongue may now be removed, and search must be made in its base for several small bones constituting the hyoid apparatus, which should be carefully taken out, and tied at once to the lower jaw for fear of loss. A considerable quantity of the brain may be removed by means of a spoon-shaped stick through the aperture at the back of the skull where it joins the neck. The rest may be removed by means of large shot put in at the hole, and shaken up with water. The neck may be cut off close to the trunk, and the tail close to the rump, and the flesh removed with the knife. The chest cavity should be left entire, the flesh being removed as well as circumstances will permit. The whole of the pieces should be treated with water, and then dried, as in the case of the limbs. The skull, limbs, tail, and neck may be conveniently placed inside the chest cavity for packing, and if it be necessary to get the skeleton into a short packing case the back bone may be divided behind the chest cavity, and the hinder vertebræ and hip bones laid along side of it. The tail may also be divided into segments. The skeleton should be well packed in dry hay or straw.

Precautions.—The bones should on no account be boiled or placed in hot water. They should not be allowed to remain in the sun after they are once quite dry. In severing the various portions of the skeleton from one another, great care should be taken that the knife passes between the bones through joints, and that the bones are not cut or injured in the process.

H. N. MOSELEY

NOTICE OF A SUPPOSED NEW MARINE ANIMAL FROM WASHINGTON TERRITORY NORTH-WEST AMERICA[*]

SOME months ago Capt. D. Herd of the Hudson's Bay Company's service, sent me several specimens which at first sight appeared to resemble long thin peeled white willow wands more than anything else. These objects, of which I exhibit examples, are about a quarter of an inch in diameter at their thickest part near the base, and taper gradually to a slender apex. The base also narrows slightly and presents traces of corrugations. The longest are upwards of six feet in length. Capt. Herd merely stated that they had been brought from North West America, and asked me to find out what they were, promising an account of all he knew about them on a future occasion.

Expecting to see Capt. Herd very shortly, I did not myself make any very accurate examination of these objects, but I convinced myself that they were of animal origin, and was inclined to regard them as possibly bones of one of the gigantic Rays. I gave specimens of them to Prof. Flower, Prof. Milne-Edwards of Paris, and several other naturalists,[†] who visited the rooms of the Zoological Society, and who all said that the objects were new to them and that they did not know what they were, but were mostly inclined to regard them as the axes of an unknown Pennatulide animal.

Knowing that Prof. Kolliker of Wurzburg had recently been engaged on a monograph of the Pennatulidæ, I likewise sent him a specimen, in reference to which he was kind enough to write to me as follows:—

"The object you sent me, found near Vancouver Island, is indeed the axis of an unknown Pennatulide, and agrees with none of those described in my monograph. It differs from all axes of Pennatulidæ investigated by me, in showing no radial fibres, not even the very short ones, described by me in *Funiculina quadrangularis* and *Halipteris* (*Virgularia*) *christii*, and may therefore belong to a new genus. Except in this respect the said axis agrees most with that of *Halipteris christii*, but there is also a difference, as the axis of *Osteocella*, as we may call it, is absolutely *quadrangular* in its lowest part for the length of about 3 centimetres.

"I put the four pieces you sent together and found a total length of 1·769 metres.

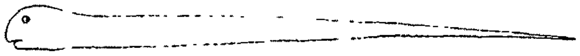
"The thickest part is found at the distance of about 0·210m. from the lower end, and measures 6·3mm. in breadth. Both ends are broken; the lower measures 1·8 mm. in breadth, and the upper 0·5mm. In general the axis is cylindrical and smooth but there are granulations and warty excrescences on the lower end for a length of about 0·20m. The axis is calcareous, and shows after the extraction of the earthy matter fine fibrils and lamellæ like the axes of all other Pennatulidæ.

"I may further add, that no Pennatulide of this size is known from the west coast of America."

Shortly after I had received this communication from Prof. Kolliker, I obtained from Capt. Herd the following account of the manner in which these objects had come into his possession.

"These rods are the back bones of a sort of fish found in great abundance at Barraud's Inlet, Washington Territory, North-West America, whence they have been brought by two Captains in our service. These animals are shaped like a Conger eel, but are quite transparent, their bodies being composed of a mass of jelly—they are about 8 inches in diameter. The head is like a shark's head; it is attached to the thick end of the rod—it has two eyes and a mouth placed low down. The back bone is also transparent in the living animal, but becomes hard when dried on the beach by the sun. These fishes swim about in shoals along with the dog-fishes.

The rods were brought by our ships *Prince of Wales* and *Princess Royal*, Capt. Anderson, who has made me the accompanying sketch of the fish itself."



Found at Barraud's Inlet, Washington Territory, amongst the Dog Fish.

A somewhat similar account of the origin of these objects is given in the subjoined extract from a letter of the Hon. Mr. Justice Crease, of Victoria, British Columbia, who has recently sent a specimen of the same object to the Royal Horticultural Society.

"I send you by this post a specimen, which Mr. Claudet (Superintendent of our Government Assay Office) has sent to me, to inquire what it was, of the bone of a fish taken frequently in Barraud Inlet, near New Westminster, Fraser river, by Messrs. Dick and Nelson at their Saw mills. There has been a great discussion here among brother ignoramuses as to whether it is vegetable or animal production. Though it has a singular breakage it answers to the test as lime. Claudet is a clever man, and thinks with me that it is bone. I have broken it in several pieces for convenience of transmission. Can you tell us what it really is? I have seen several like it and from the same place. Dick and Nelson are both respectable men and Claudet of course is beyond suspicion." (May 10, 1872).

Capt. Anderson being absent from England, I have not been able to ascertain whether the information above given was founded on his own observations or on the accounts given to him by the inhabitants of the district of Barraud's Inlet. Supposing the former to be the case, and that these objects are really derived from such an animal as is described and figured above, I can only suggest that they may be the hardened notochords of a low-organised fish, allied either to the Chimæroids or to the Lampreys, in which the notochord is persistent throughout life. It is quite certain I think, that they cannot be any part of the true vertebral column.

But whether this be the case or the Pennatulide view of their origin be the true one, it is certain that the animal that produces these curious rods is quite unknown to us, and it is highly desirable that specimens of it should be obtained. I have already requested Capt. Herd to communicate with Capt. Anderson on this subject, and trust that on his next return from Barraud's Inlet he will bring us the entire body of this wonderful creature preserved in spirits. I hope also that if any student of "NATURE" in Washington territory may chance to read this article he will not fail to exert himself and assist us in solving this somewhat puzzling zoological problem.

P. L. SCLATER

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[*] The substance of this paper was read before Section D at the meeting of the British Association at Brighton, August 20, 1872.

[†] A specimen given to Dr Gunther was handed by him to Dr Gray, who to my great surprise without consulting me or even ascertaining correctly where I had obtained it, immediately described it in the "Annal of Natural History" (Fourth series, vol ix. p 405) as a "new species" of his genus "*Osteocella*," whatever that genus may be, for its author considers it "very doubtful whether it belongs to the Pennatulidæ" and states that "it may be the long conical bone of a form of decapod cephalopod."!

A GIGANTIC "PLEASURING GROUND": THE YELLOWSTONE NATIONAL PARK OF THE UNITED STATES

II.

LEAVING the Yellowstone Basin, and crossing in a westerly direction the range which divides the drainage of the Yellowstone and the Madesin, we come into the great Geyser Basin of the Firehole river, a branch of the Madesin Forth. Travelling in this region is attended with great difficulties on account of the immense quantity of fallen timber. The uplands, as well as the lowlands, are covered with a dense growth of pines, the majority of which have a trunk not over 6 in. to 12 in. in diameter, but run up to a height of 100 ft. to 150 ft., as straight as an arrow. In crossing this shed the source of the east fork

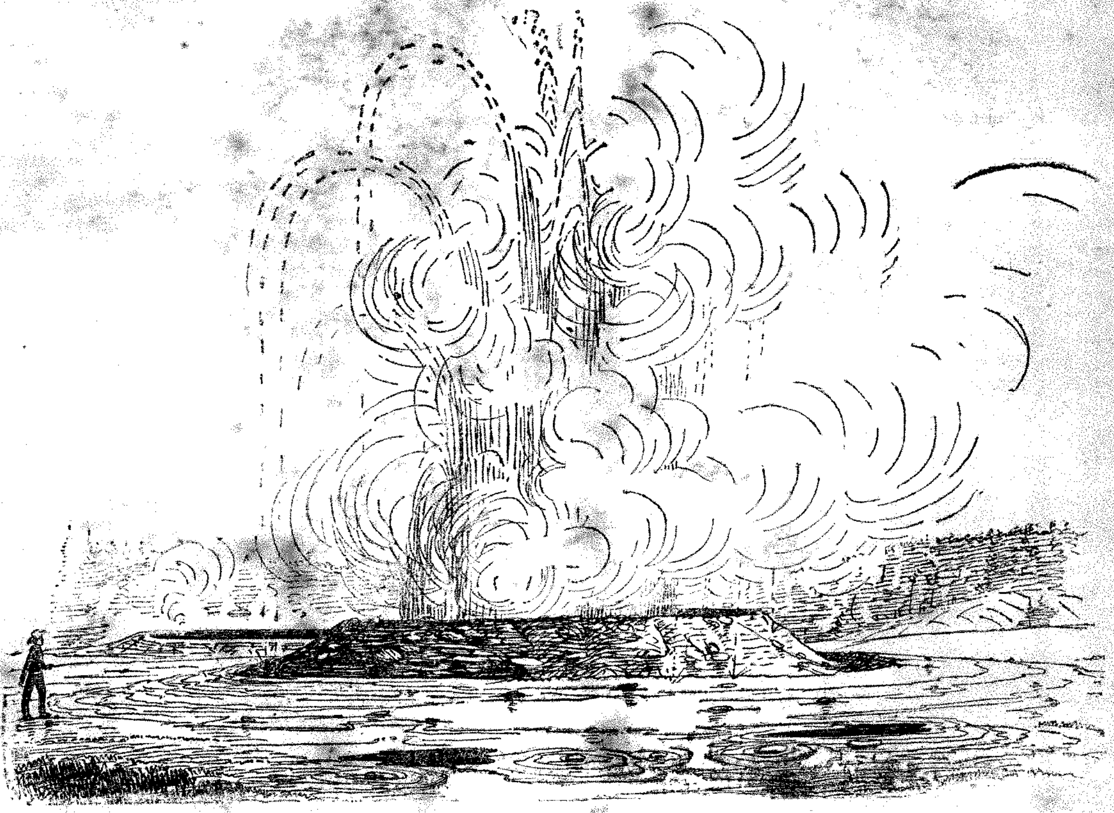

FIG. 5.—Architectural Fountain.

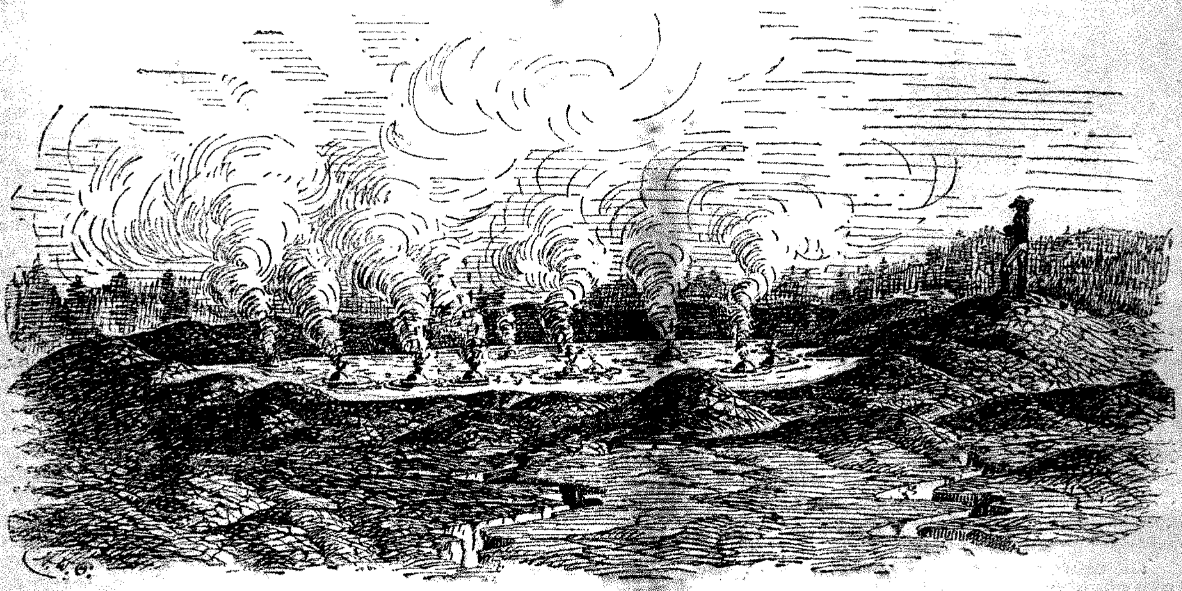

FIG. 6.—Mud Puffs.

of the Madesin is first struck, and every few miles a group of dead or dying springs is met with. In the distant view the appearance of the whole country may be not inaptly compared to a vast limekiln in full operation. The last branch of the Madesin is almost entirely fed by water from the hot-springs, and its temperature is 60° or 80° all the time. The vegetation that grows along the banks, and in the stream itself, is a marvel of luxuriance. The mountains that enclose the valley on either side are composed of basalt and obsidian. As we proceed down the valley, toward the junction of the east fork with the Madesin, the springs grow more abundant, and we soon come to the great basin of the Firehole, in which the most powerful geysers are found.

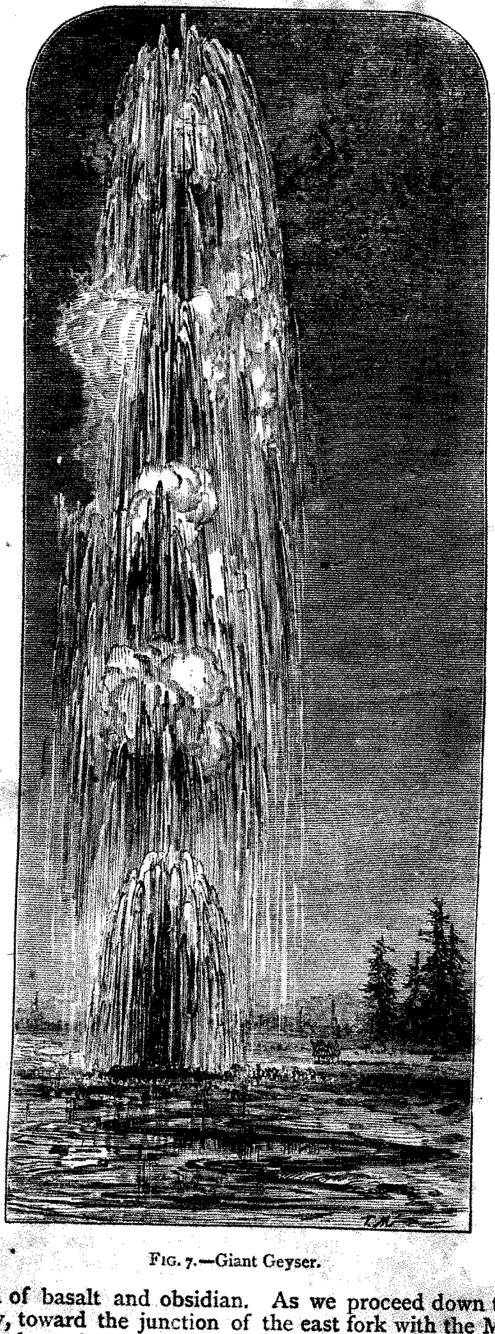

Fig. 7.—Giant Geyser.

The entire valley of the Firehole, averaging about three miles in width, is covered with siliceous crust as white as snow. The elevated mounds and numerous columns of steam reveal where the most important groups of springs and vents are located. In the Lower Geyser Basin, though there are many groups of most interesting springs, none of them can rank as geysers of the first class. Over an area of about three miles there must be at least a thousand active, quiet, dead and dying springs. One of the most remarkable of the springs in this lower basin has built up for itself a cistern, which is so elaborately beautiful that Mr. Hayden calls it the *architectural* fountain (Fig. 5). The whole basin is about 150 ft. in diameter. Near the centre is the rim of the spring, which is about 25 ft. in diameter; the water is in constant agitation, occasionally spouting up a column of water like an artificial fountain, and filling up the reservoir and the sides for a radius of 50 ft. or more. The siliceous accumulation made by this spring descends for several hundred feet in innumerable semicircular steps varying from one-fourth of an inch to two inches in height, and is exquisitely beautiful in all its details. When in active operation, a column of water is thrown 30 ft. to 60 ft. high, the water spreading over a radius of 50 ft., and filling the numerous reservoirs that surround the immense rim of the basin. There are other funnel-shaped basins with elegantly-scalloped rims, covered all over the inner side to the depth of 10 ft. to 20 ft. with bead-like tubercles of silica. Sometimes these siliceous beads are arranged in large numbers like fungi or corals, or like the heads of cauliflowers.

A short distance from this beautiful geyser is a remarkable group of *mud springs*. One of them has a basin 50 ft. in diameter, which is covered over thickly with puffs, like an immense cauldron of thick hasty-pudding (Fig. 6.) The exact symmetry of these puffs, their uniformity of size, and the fineness of the material, render them exceedingly beautiful; and there is among them every shade of colour from a bright scarlet to the most delicate pink or rose, with a base as white as snow. The most fastidious manufacturer of porcelain would go into ecstacies over this magnificent bed of mortar, that has, perhaps, been worked and re-worked for many thousands of years.

These springs occur in small groups all over the basin, and are often in close proximity to geysers or to perfectly quiet springs. They are found in every stage, from simply turbid water, through all grades of consistency, to thick stiff mud, through which the gases force themselves with a suppressed thud-like sound. Each of these mud springs probably commenced as a geyser, or at least a boiling spring. The water is at first clear, then becomes turbid, and grows gradually thicker, until the heat dies out.

The Upper Geyser Basin is located very near the source of the Firehole River, and between it and the Lower Geyser Basin there is an interval of about five miles, in which the hills come close to the river on both sides, and the springs occur only in small groups. Near the centre of the upper basin, which is about two miles long and half a mile wide, there is one of the most powerful geysers of the basin (Fig. 7). The preliminary warning is indicated by a tremendous rumbling, which shakes the ground all round with a sound like distant thunder; then an immense mass of steam bursts out of the crater as from an escape-pipe, followed by a column of water 8 ft. in diameter, and rising by steady impulses to the height of 200 ft. Mr. Hayden compares the noise and excitement it produced to that of a battle charge. He says the fountain continued to play for the space of fifteen minutes, when the water gradually subsided, and settled down in the crater about 2 ft., and the temperature slowly diminished to 150°. There are here two separate basins, one of which is in a constant state of violent agitation, while the other plays only at intervals of thirty-two hours; and although, so far as the eye can detect, there is a partition of not more than 2 ft. in thickness between them, neither of them seems to be affected by the operation of the other. The decorations about these springs are beyond conception beautiful; the most delicate embroidery could not rival them in their wonderful variety and complexity. The surface within and without is covered over with little tubercles of silica, which have a smooth, enamelled appearance like the most delicate pearls; down on the sides of the basin are large rounded masses like corals, formed entirely of silica. There are probably from twenty to fifty geysers of greater or less importance in this valley.

The two kinds of deposits in these regions, the calcareous and siliceous, have been mentioned in the preceding description. According to analysis by Dr. Peale, Chemist of the U. S. Geological Survey, the springs on Gardener's River, known as the White Mountain Springs (Fig. 8), deposit carbonate of lime mostly. There is present also sulphate of magnesia, chloride of calcium, sulphate of soda, and a little silica. In the deposits of the Firehole Basin not a trace of lime could be detected, but about 85 per cent. of silica, 11 per cent. of water, and the remainder mostly chloride of magnesium, and only a slight trace of lime has been found in the water. There are, scattered over the great area, a few patches of the sedimentary rocks, and it is most probable that underneath the deposit of this small group of springs there are portions of the carboniferous limestone.

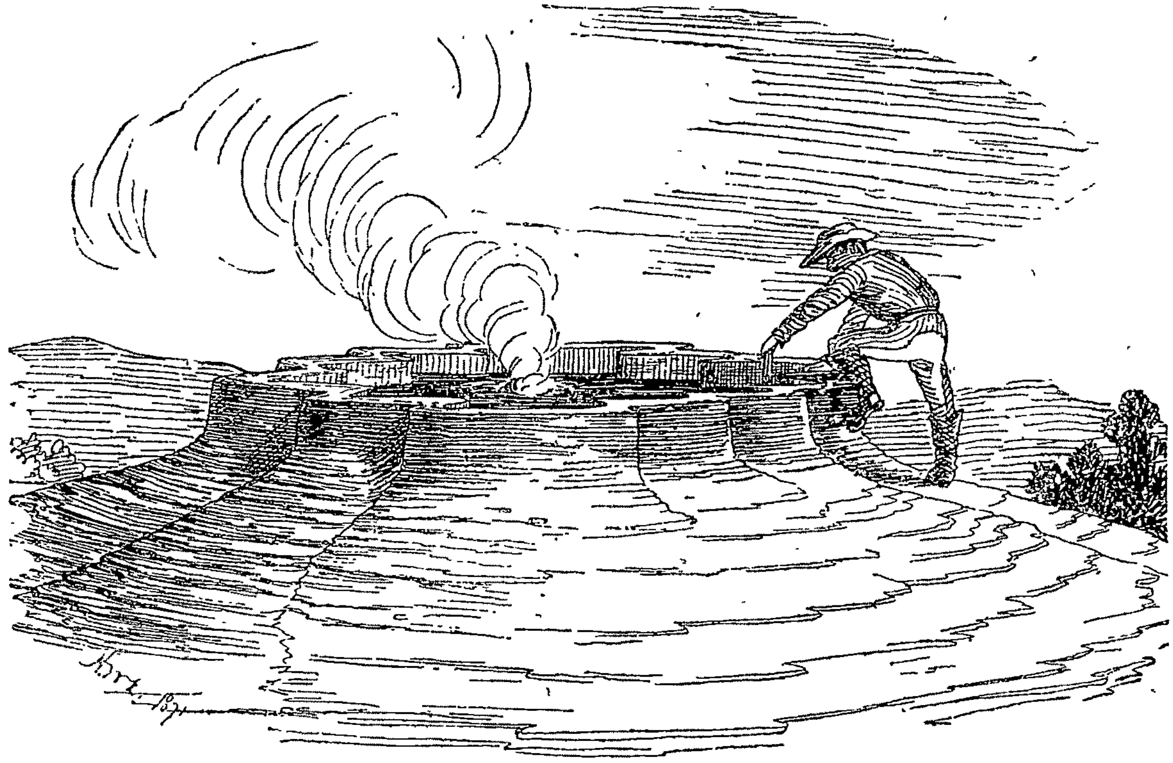

FIG. 8—White Mountain Springs.

So far as Mr. Hayden and party could ascertain, in all the deposits of the Yellowstone Basin proper, and the Firehole Basin, silica is the dominant constituent. The springs are, with very few exceptions, near the borders of the streams below beds of limestone. It is possible that underneath the vast masses of volcanic material which compose the mountains on every side, the sedimentary rocks exist; but Mr. Hayden is disposed to believe that they occur only in isolated and much-restricted patches, if at all.

It may therefore be stated, in general terms, that the great hot-spring region of the Yellowstone and Missouri rivers is covered with rocks of volcanic origin, of comapratively modern date.

NOTES

AT their Statutory Quarterly Meeting on Monday, the Trustees of Anderson's University, Glasgow, elected Mr. George Forbes, B.A., F.R.S.E., of St. Catherine's College, Cambridge, to the vacant professorship of Natural Philosophy. There were originally eleven candidates for the appointment, but one of them died before the day of election arrived. Prof. Forbes is by inheritance, as well as by inclination and education, a man of science. The son of the late Principal, James D. Forbes, F.R.S., of St. Andrews, he received his university training at St. Andrews, Edinburgh, and Cambridge, and has since had the benefit of practical training in physical science at the Royal Observatory under the Astronomer Royal, and on the Continent. In making the appointment, the Andersonian trustees acted on the advice of Sir Wm. Thomson, Sir G. Biddell Airy, K.C.B.; Prof. P. G. Tait, Edinburgh; Prof. Balfour Stewart, Prof. A. S. Herschel, Profs. W. H. Miller and J. C. Adams, Cambridge; Prof. Grant, Glasgow Observatory, and other eminent physicists. It is understood that this professorship is to be established on a more extensive basis than formerly. This would offer to the professor a wider field of useful employment by developing the resources of the professorship, not only through the extension of the system of lecturing, but also by the establishment of a physical laboratory for students, which, it is understood, is in contemplation. Mr. Alexander Lindsay, of Glasgow, has also been elected Professor of Medical Jurisprudence. It was intimated to the meeting that Mr. J. Tennant, of Rollox, has given a donation of 1,000*l.* to the University.

THE following lectures on subjects connected with Physical and Medical Science will be delivered at the University of Cambridge in the Michaelmas Term :—"Heat and Electricity," by the Professor of Experimental Physics (Mr. Maxwell), on Mondays, Wednesdays, and Fridays, at 12 M.; begin October 23. "Chemistry," by the Professor of Chemistry (Mr. Liveing), on Mondays, Wednesdays, and Fridays, at 12 M.; begin October 21. "Practical Chemistry," by the Professor of Chemistry, on Mondays, Wednesdays, and Fridays, at 1 P.M.; begin November 4. "Zoology and Comparative Anatomy," by the Professor of Zoology and Comparative Anatomy (Mr. Newton), on Mondays, Wednesdays, and Fridays, at 1 P.M.; begin October 21. "Anatomy and Physiology," by the Professor of Anatomy (Dr. Humphry), on Tuesdays, Thursdays, and Saturdays, at 1 P.M.; begin October 22. "Practical Anatomy," by the Professor of Anatomy and the Demonstrator of Anatomy (Dr. Wilson), at 9 A.M. daily till October 21; afterwards on Mondays, Wednesdays, and Fridays; begin October 7. "Materia Medica and General Therapeutics," by the Downing Professor of Medicine (Dr. Fisher) or his deputy, on Tuesdays, Thursdays, and Saturdays, at 9 A.M.; begin October 22. "Clinical Medicine," by the Regius Professor of Physics (Dr. Paget), on Mondays, Wednesdays, Thursdays, and Fridays, at 10 A.M.; begin October 10. "Clinical Surgery," by C. Lestourgeon, M.A., on Tuesdays and Thursdays, at 11 A.M.; begin October 22. Attendance on the lecture on Botany, Chemistry, Materia Medica, Anatomy, Physiology, and Dissections is recognised by the Royal College of Surgeons, England, as one of the sessional courses required by the regulations of the Council of that College.

WE understand that Mr. Darwin's new work on "Expression in Animals," a subject to which he has devoted great attention, will shortly be published in this country, as well as a German translation by Dr. Carus.

LORD CATHCART, the President of the Royal Agricultural Society, has offered a prize of 100*l.* for the best essay on the causes and remedy for the potato disease.

ON Saturday morning a conference was held in St. Thomas Charterhouse Schools to inaugurate a system of Science Teaching for Elementary School Teachers. The Rev. Evan Daniel, Principal of Battersea Training College, was voted to the chair, and there was a very large attendance of teachers of both sexes. The chairman spoke strongly of the necessity of immediately starting an institution for the efficient teaching of science. On all sides, he said, there was an outcry for it. All provisions hitherto made by the Government for the teaching of science were wholly inadequate. The school is to be known as the St. Thomas Charterhouse School of Science, and its programme for the ensuing session, which commenced on Monday evening last, shows that, in addition to the evening classes, there will be day classes for general students. The subjects include chemistry, geology, mathematics, animal physiology, acoustics, vegetable anatomy and physiology, magnetism and electricity, botany systematic and economic, plane and solid geometry, music, &c. Several professional lectures will be given during the session, and biology students will have opportunities of making microscopical observations. Any communications or inquiries should be addressed to Mr. C. Smith, the organising teacher.

THE Royal Microscopical Society will hold its first meeting for the session on Wednesday, Oct. 2, at King's College, at 8 P.M.

CAPTAIN BURTON, who has been exploring the unknown interior of Iceland, has returned to England. His collections, &c., we believe, are at present left in the care of the Anthropological Institute, London, as he had to leave England at once in order to take up his appointment as Consul at Trieste, to which he was appointed on the death of the late Mr. Charles Lever.

THE Academy of Sciences of Bologna has resolved to give in 1874 a prize of 1,200 francs *(prix Aldini)* for the best scientific experimental essay on Galvanism or Dynamic Electricity. The competition remains open till June 30, 1874; the works may be either written or printed, but in the latter case they must not be published before 1874; and then they may be written either in Italian, Latin, or French.

THE Academy of Sciences of Vienna instituted in 1869, for the purpose of encouraging astronomers to search for comets, eight special prizes, which it has kept up each year since as part of its programme. Each of these prizes consists of a gold medal of the value of 20 Austrian ducats (between 9*l.* and 10*l.*) They are intended to reward observers who discover a telescopic comet, or a comet visible only by telescope at the time of its discovery. One condition is that the comet has not previously been seen, and that its appearance has not been previously proved with certainty. The discovery should be immediately announced to the Academy by telegraph or otherwise without waiting for further observations, the Academy undertaking to notify at once to the different observatories the fact of the discovery. The place and time of the discovery ought to be indicated, as well as the position of the comet and its orbit as exactly as possible with the first intimation; the data should be completed at leisure by further observations if it be possible to make them. When the comet has not been seen by other observers, the prize will be presented only when the observations of the discoverer have been sufficient to enable the orbit to be determined. The prizes are decided each year at the general meeting of the Academy held at the end of the month of May. If the first announcement of the discovery reaches the Academy between March 1 and May 31, the prize cannot be decided till the following year.

THE Society of Science and Arts of Utrecht offers prizes for papers on the six following subjects :—1. Experimental Researches upon the Inhibitory Nerves. The author must not restrict himself to a mere critical review of existing opinions on the subjects; he must elucidate them by new experiments. 2. Researches on the development of one or more species of invertebrate animals whose history is not yet known; the paper must contain all the illustrations necessary to the understanding of the text. 3. Researches upon the influence which slight variations of external circumstances may exercise upon the evolution of the embryo of one or more species of invertebrate animals. 4. Description of the larva and nymph of the common cockchafer, to complete the monograph on that insect in its perfect state by Strauss-Durckheim. This must be accompanied by the necessary figures. 5. Researches to determine the normal variation of the temperature of at least thirty-five places in the northern regions of Europe. The monthly means of old observations ought to be reduced so as to agree with the time at which the observations are actually made. 6. To investigate and point out how the waters of the rivers which traverse the Netherlands could be purified so as to become drinkable without detriment to the public health; at the same time to indicate the probable expense of their application on a large scale. Each of the prizes will consist of a medal of the value of about 27*l.* sterling, or its equivalent in money. The papers may be written in French, Dutch, German (Roman letters), English or Latin. They ought to be sent to the Secretary of the Society, M. N. F. Van Nooten, before December 1, 1873, the name and address of the author ought to be attached to each memoir in a sealed envelope.

DEMERARA papers record the death, in the early part of the summer, of Mr. C. F. Appun, an enterprising naturalist who had done much to explore both the physical features and the natural productions of the colony. He had previously travelled through Venezuela, Brazil, and the Amazons Valley, and had sent considerable collections of plants both to this country and to Germany. His journal has been printed in the *Georgetown Gazette*, and we hope to give extracts from it on a future occasion.

THE Council of the Vienna Exhibition have decided on having a permanent aquarium erected in that city, and the plans of Mr. H. Driver, C.E., who erected the aquarium at the Crystal Palace last year, have been approved.

WE understand that it is not the intention of the Board of Managers of the London Institution to fill up the vacancy occasioned by the decease of Mr. J. C. Brough, F.C.S. until after November.

THE *Newcastle Chronicle* states that some gentlemen connected with the mining interest have for several days been prosecuting their inquiries in the neighbourhood of Waterford with reference to the existence of coals in that portion of county Kilkenny which lies between the Suir and the Barrow, and has a communication with both of these important rivers. The geological maps give no indication of coals in this locality, but the result of inquiries prosecuted with much intelligence has led to the discovery of a coal bed of immense dimensions in this district, about two miles from Waterford, and within easy access to the river Suir. The coal seam to the thickness of 10 ft. lies immediately under the Old Red sandstone, the lower strata being a very fine outcrop of silicate of magnesia. The coal shales come to the surface at the cross road, about half a mile beyond the chapel of Sheverne. The arrangements are in a state of great forwardness for an immediate start, and a number of English miners are daily expected. If the hopes of these parties—and they appear to be well founded—are realised, it will afford a vast amount of employment, and will give the south of Ireland an almost unlimited supply of fuel. We need hardly point out that our Newcastle contemporary must have fallen into an error in speaking of the coal-seam being found *under* the Old Red sandstone.

WE are glad to see that Professor Piazzi Smyth, Astronomer Royal for Scotland, has at last got the reward of his twenty years' persevering and creditable importunity in the shape of a new equatorial telescope for the Royal Observatory on the Calton Hill, Edinburgh, for which Government last year granted 2,300*l*. Hitherto, as the *Scotsman* justly remarks, the Metropolitan Observatory of Scotland has been in the position of the meanest appointed Government astronomical institution in Scotland. To accommodate the new telescope, it is necessary to raise the dome of the Observatory, and Mr. James Fergusson, author of the "History of Architecture," who was consulted on the matter, has decided that the new dome could not be raised more than fourteen inches—that being the largest increase that could be æsthetically allowed in conjunction with the rest of the Observatory, which, viewed as a piece of architecture, is considered to be the very gem of the works of the late William Playfair. In consequence of this the Astronomer Royal has had a difficult task in endeavouring to arrange a form of equatorial instrument which would give a greater amount of power within a smaller line or compass than was ever attained before. The new telescope will have an aperture of two feet upon a focal length of only ten feet—a larger diameter in proportion to focal length than any astronomical telescope yet introduced into any observatory; and it will no doubt be by far the most powerful instrument ever erected under so small a dome. Although the telescope will be much more powerful than any ever before placed in the Observatory, it is still not such as was desired or considered almost necessary in the present state of science. The instrument, which is being built by Mr. Howard Grubb of Dublin, is to be constructed on M. Foucault's comparatively new principle of having the speculum of glass coated with pure silver. The instrument is to be employed both for photography and spectroscopy. Both these classes of research require the seemingly impossible accompaniments—that the telescope must have the utmost amount of firmness, and also have the most accurate possible movement at the same rate at which the stars change their position in the sky. Notable features in the new telescope will therefore be the remarkably perfect clockwork apparatus, the several devices connected with the prisms of the spectroscope, the means by which not merely celestial objects will be kept in view, but those by which the rays of chemical flames will be brought into comparison with the light of the stars. The extremely delicate measuring apparatus to be applied to the respective subjects as they appear on the spectrum will also be noteworthy. December next is the time fixed for the completion of the new instrument, but meanwhile active preparations are being made in the Royal Observatory for its reception. The new dome, it is expected, will be erected in the course of this month, while the weather is yet fine. This dome, which is also being built by Mr. Howard Grubb, will be of iron instead of wood, and that for two reasons: first, because it will afford a greater amount of space in the interior of the instrument room; and second, because it will enable such an arrangement being made for the shutter as will allow of an opening several feet in breadth, whereas the opening in the old dome was only a few inches wide. Although the arrangements of the Observatory are, during operations, necessarily somewhat upset, observations continue to be made by Prof. Piazzi Smyth and his assistants.

IN accordance with the decision of the Scientific Association of France, systematic and simultaneous observations on shooting stars have been made during August in various places in France, Italy, and at Alexandria. The chronometers of the various stations were compared by telegraph, the signals being given from Bordeaux, Lyons, Marseilles, and Paris. The results of the simultaneous observations at the various stations are recorded in the *Bulletin Hebdomadaire*, from which we learn that on the nights of August 9, 10, and 11, observations were made at twenty-two stations; Alexandria and Moncalieri being those from which the greatest number of shooting stars were seen, 2,042 having been noticed during the three nights at the former, and 2,049 at the latter place. It will be remembered that in November last the whole of the shooting stars did not come from Leo, as it was expected they would, and that the observers noticed radiant points in Taurus, Gemini, &c. Somewhat similar eccentricities, though upon a less scale, appear to have occurred during the August shower. At Genoa, about the half of the stars came from various directions; M. Stephan, at Marseilles, intimates that on the third night the radiant-point was in Cygnus; and at Paris, M. Tremeschini found that on the third night the majority of the stars did not come from Perseus. MM. Le Verrier and Wolf, who have been charged with the arrangement of the various observations, have presented to the Academy the results of the work done last November, and expect to be able to do the same for that of August, soon after they receive detailed reports from the various observers. For the discussion of the common observations, the astronomers at their meeting at Montpelier decided to employ the method proposed by Colonel Goulier, which however can be applied only when the stars are at least 10° above the horizon; when otherwise, the methods followed by MM. Lespiault and Stephan will be used. M. Goulier has been charged with the construction of the charts of his system; M. Lespiault with the preparation of the method to be followed when the trajectories are at a small elevation above the horizon; and M. Wolf with the final arrangement of the complete results.

BIELA'S COMET

MR. J. R. HIND has addressed the following letter to the *Bulletin* of the *Association Scientifique de France*:—

You will probably think me rather sanguine in supposing for a moment that there is a chance of re-discovering either nucleus of Biela's comet this year, when in the ordinary course of things a perihelion passage would be due. I look at the matter in this light. We know that in February 1846 a very remarkable alternation of brilliancy took place: that the second nucleus, barely discernible at first, so greatly increased its light as to surpass what I will term the parent-nucleus, and continued thus several days, when it gradually faded. Again in September 1852, M. Otto Struve's drawings show the same remarkable interchange of light between the 20th and 25th. Whatever the cause may be, each nucleus appears to have a re-vivifying power, so to term it, and I think it is just possible this may be exercised at one time or other to such an extent as to bring the comet again within our grasp, though its condition in 1865-1866 may have been such as to render it quite invisible from the earth. In this idea of the subject, I have prepared sweeping ephemerides for September and October, part of which (that applying to next absence of moonlight and longer) I now do myself the honour to send you. The mean motion in Dr. Michez's orbit from 1866 would bring the comet into perihelion 1872, October 6·4, and I have calculated places on this supposition, also with variations of ± 8^d in perihelion passage. Clausen carried forward the perturbations of both nuclei in 1866, and his elements for that year would indicate (of course, neglecting perturbation 1866-1872) the following times of perihelion passage.

Nucleus I . .	1872 Oct.	4·776	Greenwich
Nucleus II . .	,, Oct.	7·256	,,

and hence these differences of R. A. and N. P. D. between the two nuclei, which, if one is only found, might be useful in bringing to light the other.

	R A. (I—II)	N. P D. (I—II)
	° ′	° ′
Sept. 12 . . .	+ 1·59 . . .	+ 0·55
,, 20 . . .	+ 1·50 . . .	+ 0·57
,, 28 . . .	+ 1·42 . . .	+ 0·55
Oct. 6 . . .	+ 1·34 . . .	+ 0·50
,, 14 . . .	+ 1·26 . . .	+ 0·44
,, 22 . . .	+ 1·18 . . .	+ 0·37

Perhaps there may yet be time to interest some of your many correspondents in a search for Biela during the first ten days of October. Here the weather has been exceedingly unfavourable, and, though I have watched assiduously, there has not been a single opportunity of examining the eastern heavens in the morning during the last period of absence of moonlight.

I remain, etc.

J. R. HIND

Observatory, Twickenham, near London, Sept. 17

SWEEPING EPHEMERIDES FOR BIELA'S COMET

1872 9^h 36^m	Perihelion. September 28,4		Perihelion. October 6,4.			Perihelion October 14,4.	
Greenwich.	R.A.	δ	R A.	δ	Δ	R A	δ
	h m	° ′	h m	° ′		h m	° ′
Sept. 28	10 4·0	+6 27	9 43·0	+9 15	1 345	9 20 3	+12 22
30	10 13 0	5 21	9 52 4	8 6		9 30 2	11 9
Oct. 2	10 21·9	4 15	10 1·7	6 56	1 365	9 40 0	9 56
4	10 30 7	3 9	10 10 9	5 46		9 49 7	8 42
6	10 39·3	2 4	10 19·9	4 37	1·388	9 59 2	7 28
8	10 47 8	+1 0	10 28 8	3 28		10 8 6	6 15
10	10 56 2	−0 3	10 37 6	2 20	1·414	10 17 8	5 2
12	11 4 5	1 4	10 46·2	1 13		10 26 9	3 50
14	11 12 6	2 5	10 54·8	+0 7	1·442	10 35 8	2 38
16	11 20·6	3 4	11 3·2	−0 58		10 44 6	1 27
18	11 28 5	4 2	11 11·4	2 2	1·472	10 53·3	+ 0 18
20	11 36·3	4 59	11 19 6	3 4		11 1·8	− 0 50
22	11 44 0	5 55	11 27 6	4 5	1 503	11 10·2	1 57
24	11 51·5	6 49	11 35 5	5 5		11 18·5	3 3
26	11 59 0	−7 42	11 43 3	−6 3	1·535	11 26·6	− 4 7

Mr. Hind, in a letter with which he has favoured us, states that M. Stephan, the Director of the Observatory of Marseilles, will employ the large Foucault mirror of that establishment in a search for the comet.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE session of this Association opened at Dubuque, Iowa, on August 21. At the end of last year's meeting it was expected that the Association would meet this year at San Francisco, which place it was hoped Prof. Agassiz would have reached in the *Hassler* by the time of the opening, and added an unusual brilliancy to the proceedings by an account of some of his discoveries in the Southern seas. The great naturalist, however, as we know, has not yet reached the Californian coast; and partly on this account, but mainly, we believe, on account of the great expense to most of the members of a journey to San Francisco, that city has not this year been honoured with the presence of the associated *savans* of the United States. Indeed, if we may judge by the somewhat diminished attendance, and the unusually small number of papers read, even Dubuque seems to be too far away from the homes of many of the members. Some of the prominent members of the American Association were absent this year, the many, however, who did attend, met with a very liberal reception from the citizens of Dubuque.

The retiring president of the last year, Prof. Asa Gray, of Cambridge, Mass., made, as is the custom, a valedictory address. He took the opportunity to sketch his recent experience at the West as a botanist, for, strange to say, until the present summer he had not seen the Mississippi nor set foot upon a prairie Prof Gray touched lightly upon the history of the Sequoias; that their age must be counted by hundreds of years we cannot doubt; but also we cannot doubt that they did not antedate the glaciers whose icy expanses have left their indisputable evidences everywhere around. The main portion of Prof. Gray's address was devoted to showing the probability that certain trees of the present day, the *Sequoias* of California, the *Taxodium*, or Bald Cypress of the Atlantic, and the *Glyptostrobus*, a Chinese tree, the only ones of their tribe at present existing, were not only closely allied in structure and general characteristics, but were the lineal successors by gradual modification of the fossil trees of geological ages; and that all our existing vegetation was a continuation of that of the, Tertiary period. Three hypotheses are open to account for their present existence: either they were created in the detached regions where they are now found; or they are the first members of a new and increasing race; or they are nearly the last representatives of a once powerful and widely-spread tribe. After discussing the first two alternatives, Prof. Gray gives his reasons, which are supported by palæontological evidence, for believing that the "big trees" of California are the last of their race, and that a little further drying up of the climate, which is now in progress, will speedily precipitate their doom. The oldest of the trees now standing he considers to possess an antiquity of about 2,000 years.

A paper was read by Prof. E. S. Morse "On the Oviducts of the Brachiopods." He brought forward for the first time a few facts regarding the development of a species of Brachiopod from the coast of Maine. The various stages in their development, he believed, fully supported the position he maintained, that the Brachiopods were true worms and not molluscs.

Prof. Benjamin Peirce, Superintendent of the United States Coast Survey, gave an exceedingly interesting account of the measures taken by that Bureau with reference to stations for astronomical observations at great heights, such as Sherman, on the Rocky Mountains. Prof Young, of Dartmouth College, has been appointed to examine and give an opinion as to the effect of taking observations from the Rocky Mountains and Sierra Nevadas. Prof. Peirce's opinion was that observatories should be placed on the summits of both, if not permanently at least temporarily, the Rocky Mountains being favourable to some, the Sierra Nevadas more favourable for other observations. In a higher position you get rid of absorption of light by getting rid of half the atmosphere. Prof. Young reports the whole number of lines in the chromosphere seen from Sherman as 150, which is three times as great a number as have been observed before. This fact alone shows that higher points should be resorted to for astronomical observations. The next element of success depends upon the steadiness of the atmosphere. It can be said in reference to this, that a star has been recognised at these high altitudes as having a companion, or being a double star, not previously known as such. An observer on the Pacific coast reports to Prof. Peirce that he can see the companion of the star Polaris from a high point on the Sierra Nevada. It is well known that this is a test of great nicety, requiring the utmost purity of atmosphere. The best work in astronomy is done in the few best nights at any place, and by these alone the value of the position must be determined.

Prof. Peirce showed that the necessities of the Coast Survey extended its operations to all parts of the United States. To prove the paths by which vessels can best traverse the ocean, to test the best methods by which 2,000,000,000 dollars of values shall be carried from the West to the East, from the East across the Atlantic, or from the shores of China and Japan to the Pacific coast, and thence across the country, was the business of the Coast Survey. All the United States is deeply interested in every part of the subject. Every ship that is lost by imperfectly surveyed harbours is a loss to the whole country. If the coast survey were thorough, and maps were fully representative of ascertained facts, a pilot would scarcely be necessary, but yet never could be entirely dispensed with, especially in bad weather. The pilots discovered that by putting down every rock that they knew of, they made maps that frightened the captains of vessels into employing them. Hence these practical observers have added immensely to the number of facts accumulated by the Coast Survey.

Prof. Peirce explained why he considered it unnecessary to carry out at present so thorough a survey of the Pacific as has been made of the Atlantic coast. The needs of the commerce of the coast is the standard by which the work of the survey is determined. He took occasion to mention that the *Hassler* expedition was at the expense of private individuals, principally of Boston, and was not at the expense of the Government. A general geodetic connection has been effected in these observations, so that the whole United States will benefit by them; and the points will be taken so as eventually to procure a complete survey of the whole continent, passing through each State and the large cities. It is a work that may take a century. It is the hope of Prof. Peirce that this survey will not only be the best in the world, but that its details will be such that before long there will be no necessity for railroad surveys—the facts will be spread everywhere. As to the higher operations of the Coast Survey, their ultimate expression will be an accurate determination of the figure of the earth. Observations in America he thinks are more successful and free from local irregularities than in Asia or Europe. Yet there are some such local irregularities here—notably one near Boston, where there is some strange deviation of density from the surrounding country.

Prof. J. W. Foster, of Chicago, read a paper on what he considers a new species of fossil elephant, called by him *Elephas Mississippiensis*. He presented to the association a fossil tooth found in Indiana, and which he regards as differing specifically from that of any other fossil elephant found in America or on the Continent. The differences are so great that he holds them sufficient to constitute the new species.

Prof. J. E. Parry, who had been in a good position for studying the subject, made a few remarks upon the glacial deposits of Northern Ohio. Prof. Foster took exception to several of his positions.

A very interesting paper was read "On the Mounds of Dubuque and vicinity."

A somewhat warm discussion took place on the 23rd between Dr. F. H. Day of Wisconsin and Prof. Morse of Salem, occasioned by the former sending a paper to prove that the trilobite not only resembled the lobster in some respects, but was actually closely related to that crustacean, and must have been in structure and movements almost similar. Prof. Morse stoutly maintained that the congener of the trilobite at the present day was the *Limulus*, not the lobster.

On the same day Prof. F. W. Putnam of Massachusetts, editor of the *Naturalist*, read a brief and well illustrated paper on certain "Stone Carvings of the New England Aborigines."

Prof. E. B. Andrews of Ohio, the State Geologist, presented a paper on Coal, of both scientific and practical interest. The universal law of the accumulation of coal strata seems to be on horizontal lines, determined by water levels. The proof of this is in the marine organic remains found in the coal itself and its associated slates. In addition, there is a distribution through seams of coal of horizontal layers of fine sedimentary matter which constitute the clay partings of the seams. The subsidence which carries down the area of the coal-marsh, if it may be so called, is wide-spread in its character and equable in its movement, having a tendency to a continental rather than a local character. The result of this gives a horizontal parallelism to all the seams of coal at the time of their formation; and this is equally true where one coal-marsh is formed over another, making a series of coal measures. So far as Prof. Andrews's observation goes, a coal seam has never been accumulated on high grounds or ground above water-level; and such formations could not take place, because it would be impracticable to establish the conditions of accumulation on the side of a hill. Hence it is impossible in the nature of things that two distinct seams of coal could ever coalesce, since their subsidences must take place in parallel lines. To suppose otherwise would involve a very unequal subsidence over very limited area, amounting, indeed, to a convulsion of nature, which is almost incredible.

It has hitherto been to some extent conceded that the solidification of coal is an exceedingly slow process; and the popular notion is that there has been a complete accumulation of a series of layers of coal, &c., long before thorough hardening takes place. Prof. Andrews has, however, found instances indicating that the process of solidification, making complete coal, is comparatively rapid. Thus it appears that where a gulley has been torn out of a coal seam by a rapid current of water, the small boulders washed by it over the covering stratum of sand a few feet above, are complete coal, having an angular fracture, some being still sharp on the edges and some being slightly water-worn. These boulders in turn have again been covered by subsequent depositions, and are found at considerable depths, near the base of the coal measures.

There are three leading varieties of bituminous coal; the ordinary resinous or caking coal, the splint, and the cannel coal. These pass into each other by almost imperceptible gradations. The resinous coal seems to be the normal condition which the buried vegetation first assumes, and splint and cannel are modified forms, the cannel coal having lost all trace of structure, and containing no organised forms, except stigmaria, which is very abundant. The ash of coals is the original inorganic matter of vegetation, often increased by sedimentary matter in the marsh during the formation of coal.

Prof. C. A. White, of Iowa City, gave a general sketch of the geology of the State of Iowa, in which he stated that the extent of its coal measures was greater than the entire area of Massachusetts. The occurrence of quartzite in the north-eastern portion of Iowa he regards as of the Potsdam formation. There is a remarkable area of drift in the north-western part of the State, covering an area of at least 20,000 square miles so deeply that no rock crops through. Prof. Swallow thinks that the waters receding from this State went to the Pacific.

In the discussion which took place next day, on Prof. Andrews' paper, several geologists controverted his position that coal seams seldom or never diverged from each other within small areas, adducing instances to prove their statements. Prof. Andrews did not doubt that there were occasional instances of the sort, but in general he did not believe that there were frequent cases where seams separated widely. Seams of coal, as a rule, with rare exceptions, were of parallel levels from uniform subsidences.

On the evening of the 23rd, Prof. Morse delivered to a large and delighted audience a popular lecture on "The Locomotion of Animals," beginning with the lowest forms of animal life, and working his way upwards through all grades to man, illustrating his statements by a series of clever and rapidly-executed illustrations on the black-board.

On the 24th, the Association passed a resolution with regard to what is known in America as "the Chinese Indemnity Fund," the name given to a large sum which China overpaid in settling for damages to United States citizens, and which, when informally tendered to the Chinese Government, was declined, with the intimation that pressure on the subject would be hurtful to Asiatic pride. This fund now amounts to 450,000 dols., and a bill is now pending in Congress which proposes to appropriate this surplus for the education of Americans and Chinese "in the languages, literatures, and sciences of the respective nations; to facilitate commercial, diplomatic and scientific intercourse between the two peoples; and for the increase and diffusion of knowledge among men." It is apparently intended to accomplish this purpose by establishing an American college in China, and the resolution of the Association "heartily endorses the purpose of the aforesaid Bill."

In expatiating upon the propriety of this resolution, President Smith alluded to the insecurity of the present position of the fund in the hands of the United States Government, since it offered a premium for trumped-up claims, and already 37,000 dols had been thus abstracted from it; that if it remained much longer unappropriated, Japanese as well as Chinese claims would be introduced against it, and it might eventually suffer the fate of the Smithsonian funds, which the Government had to make good after the loss of a considerable portion.

During the course of the same day a scene of intense excitement and disorder took place on the reading of a paper by Prof. B. C. Swallow, the Missouri geologist, entitled "Good Wine a Social and National Blessing,' which was devoted principally to the details of wine making. It was, however, made an opportunity for the discussion of the causes of drunkenness, and the best means for preventing it, which had far more of a social than of a scientific interest.

A very interesting and valuable paper was that of Mr. C. V. Riley of St. Louis, entitled "Insects Shaped by the Needs of Flowers," with especial reference to the fructification of the American Yuccas. Dr. Engleman of St. Louis had this year discovered that these plants must needs rely on some artificial agency for fertilisation. The flowers are peculiarly constructed, so that it is impossible for the pollen to reach the stigma, it being glutinous and expelled from the anthers before the blossoms open. Prof. Riley discovered that there was a small white moth that did the work, and demonstrated on the blackboard how wonderfully well the insect was adapted to the purpose. This little moth, which he calls *Pronuba Yuccasella*, has been unknown to entomologists, and forms the type of a new genus. It is most anomalous from the fact that the female only has the basal joint of the maxillary palpus wonderfully modified into a long prehensile-spined tentacle. With this tentacle she collects the pollen and thrusts it into the stigmatic tube, and after having thus fertilised the flower she consigns a few eggs to the young fruit, the seeds of which her larvæ feed upon. He stated that the Yucca was the only entomophilous plant known which absolutely depended for fertilisation on a single species of insect, and that insect so remarkably modified for the purpose. There was a beautiful adaptation of means to an end, and a mutual interdependence between the plant and animal; and Mr. Riley explained how on Darwinian grounds, even this perfect adaptation was doubtless brought about by slow degrees. He alluded, in closing, to a practical phase of the subject. The plant and its fructifier are inseparable, under natural conditions, and the latter occurs throughout the native home of the former. In the more northern portions of the United States, and in Europe, where our Yuccas have been introduced and are cultivated for their showy blossoms, the insect does not exist, and consequently the Yuccas never produce seed there. The larva of *Pronuba* eats through the Yucca capsule in which it fed, enters the ground and hibernates there in an oval silken cocoon. In this stage the insect may easily be sent by mail from one part of the world to another, and our transatlantic florists may by introducing it soon have the satisfaction of seeing their American Yuccas produce seed without any personal effort on their part.

Among other papers read at subsequent meetings of the Association was one by Dr. J. W. Foster on the "Bird-shaped Skulls found in Indian Mounds," in which he tried to show that the peculiar shape of these skulls was not produced by compression, as in the case with the heads of many modern Indian tribes, but really belonged to a very early and comparatively low type of man intellectually.

Another paper was by Prof. Daniel Kirkwood on "Binary Stars with Extraordinary Orbits," with special reference to Mr. Wilson's communication to the Royal Astronomical Society, as to the orbits of the stars constituting Castor.

Mr. W. W. Wheildon of Concord, Mass., advanced in opposition to what is known as the Gulf Stream theory an atmospheric theory to account for ameliorations of climate and an open sea in Polar regions. He thought that there could be no question that if the whole Arctic region were of open water that fact would not account for all the atmospheric phenomena and warmth of temperature experienced by polar navigators. The theory of Prof. Wheildon is that open water, melting ice, rain after snow, and other phenomena of the sort in Arctic regions, are not caused by winds warmed by an open sea, but by a circulation of air in which warm winds descend from upper atmospheres; being a circulation by which winds heated at the equator reach the poles. The brief discussion which followed this paper did not indicate much difference of opinion on the subject.

Of course, there were the usual numbers of excursions to places of interest, including one to the curious "Picture" or "Calico" Rocks near the town of Macgregor, which are composed of Potsdam sandstone. Far up in a narrow glen, outcropping sandstone rocks, partially denuded, exhibit in narrow stripes and patches, but principally in linear horizontal directions, the greatest variety of colours. Red is predominant, but black, blue, yellow, and intermediate shades are not wanting, each being distinct and not unfrequently contrasting sharply with the adjoining stripe, or with a gray that is almost white. Probably the original sand was white; the colours indicate varying admixtures of iron oxides and carbonates. Another was to the Mississippi, apparently for the purpose of seeing and collecting specimens of the large lily or lotus of that river (the *Nelumbium luteum*), closely allied to the Egyptian lotus. The last excursion was to Sioux City, and was to last for three days.

Altogether this year's meeting seems to have given general satisfaction, and the hospitality of the Iowans was unbounded. Next year's meeting is to take place at Portland, Maine, on the third Wednesday of August, Prof. Joseph Livesing, of Harvard University, being the president-elect.

THE FRENCH ASSOCIATION MEETING AT BORDEAUX

THE first session of the French Association for the Promotion of Science, closely modelled after that at Britain, was held at Bordeaux from the 5th to the 12th of September, and seems in all respects to have been successful and satisfactory. As is usual at similar meetings everywhere, the citizens of Bordeaux lavished their hospitality upon the members, who well deserved this as well as the gratitude of the French generally for inaugurating a movement to spread among that nation a knowledge of and love for science, and thus inform and temper their often misleading enthusiasm; in the words of M. Quatrefages, "to renovate our country by the scientific spirit and scientific studies." The meetings were well attended both by French and foreign *savans*, though the only two English ones whose names we notice were Prof. Odling and Dr. Gladstone. The Society already numbers 800 members, and, as will be seen by M. G. Masson's paper, its finances are in a flourishing condition. The first general meeting was presided over by M. de Quatrefages (the president-elect for next year), in room of M. Claude Bernard, the state of whose health prevented him from attending.

The opening address of M. de Quatrefages, as acting president, was a very stirring and noble one, full of sound sense as to the recent humiliation and present condition of France, enthusiasm towards science, and faith in it as one of the most powerful regenerators of the country. "Science is at present supreme," he said; "she is becoming more and more the sovereign of the world;" and he believes that it is only when all ranks and classes of the people, rulers and ruled, are thoroughly imbued with the scientific spirit, and are guided by scientific knowledge, that France will ever again take and maintain the supreme place in the world which she ought to hold. M. de Quatrefages concluded with a graceful allusion to "our elder sister, the British Association."

After an enthusiastic speech from the Mayor of Bordeaux, M. Cornu gave a brief sketch of the history of the Association. The first idea of the Association, he tells us, originated among a group of French Alsatian students gathered around M. Combes, the late lamented director of the School of Mines of Paris, and it has been nourished by a patriotic desire to contribute to the moral condition of the dejected country. After the death of M.

Combes, M. d'Eichtal came to the assistance of the embryo association, and on January 17 of this year a provisional council was appointed, with M. C. Bernard as president, and on April 22 the French Association was constituted, and already numbers upwards of 700 members. The British Association when it started in 1831 had only 370 members, whereas now it numbers many thousands, and can spend 2,000*l.* yearly for the progress of science. M. Cornu hopes that the French Association may become similarly prosperous, as indeed it seems to bid fair to do, for already, as M. Georges Massen, the treasurer, intimated, it has a capital of 140,000 francs, and can dispose of an annual revenue of 16,000 francs.

M. Laussedat gave an interesting lecture on "The Services which Modern Science can render to the Art of War," in which the starting sentence is that "Germany triumphed over France by numbers, discipline, and science." This was followed by a paper from M. Le Fort, on the Reform of Military Surgery. In the meetings of sections which followed, medical subjects received permanent attention, and called forth many papers of great value.

In the Anthropological Section, M. Lagneau read a careful paper on "The Ethnology of the Populations of the south-west of France, particularly of the basin of the Garonne and its affluents," M. Paul Lopenard one on "Prognathism," in which he gave some new measurements, and described a new method of measuring skulls; M. Parrot gave some details of the Cave of Excideuil (Dordogne), the variety of bones and other remains in which are of the greatest interest to anthropologists. Most of the other papers in this section were on subjects similar to the last, the prehistoric remains of various kinds found in caves, dolmens, &c., a department of Archæology to which much more has been done in France than in this country. M. Cartailhac went the length of saying that two populations belonging to prehistoric times could be distinguished as inhabiting the south of France; one warlike and given to the chase, who knew the beds of flint, and could cut it to perfection, and which was armed with the bow; the other pastoral, seldom feeding on wild animals, ignorant of the arrow and the flint weapon, and which used quartz, ophites, and other stones, but seldom flint for making axes.

But little seems to have been done in the Botanical section, one of the most valuable papers having been that of M. Van Tieghem, expounding the result of his researches into "Germination," which had for their object to determine experimentally the connection existing between the various organs of the embryo, and the degree of dependence of the latter upon the albumen. Another elaborate paper was by M. Baillon, "On the Floral Organisation of the *Amentaceæ*, and especially the Hazel."

Some of the most valuable and interesting papers of the meeting were read in the Section of Physics; the first, and one of the most attractive, was by M. A. Lallemand, President of the Section, "On the Polarisation and Colouration of the Atmosphere." The researches of M. Lallemand have been conducted on the basis of a theory which explains at once the origin of aerial polarisation, the formation of the neutral points indicated by Arago and Babinet, and the blue colour of the atmosphere. According to him, atmospheric illumination is only a particular instance of the phenomena of illumination of transparent bodies by a pencil of non-polarised solar rays. The generation of neutral points is explained simply by the intervention of the dust and atmospheric corpuscles which abound in the lower layers of the atmosphere, in the centre of which the observer finds himself placed. This paper was followed by another by M. Soret, of Geneva, "On the Influence of the Atmosphere upon the Intensity of Solar Radiation." The new experiments of M. Soret have been made by means of two calorimeters made upon the same principle and of analogous construction. The first, which is described under the name of "Actinomètre Transportable," is of small size, and consists simply of a metallic tube open at one end surrounded by a second concentric envelope. The interval between the two is filled with ice. At the bottom of the tube is fixed a thermometer, whose stem crosses the two envelopes, and upon which the sun's rays are allowed to fall. The thermometer, zero at the outset, rises in proportion as the quantity of heat which it receives equals that which it loses by radiation. The temperature which it finally reaches enables us to deduce the intensity of the solar radiation, after a certain number of corrections, the principal of which relate to barometric pressure. The Fixed Actinometer differs from the preceding only in its greater size, which allows of four thermometers being used instead of one: the inclosed interval is maintained at a constant temperature by a current of water. The experiments made by means of these instruments have enabled M. Soret to ascertain, in the first place, that the more moisture there is in the air, the less is the intensity of solar radiation for the same height of the sun and the same atmospheric pressure.

In the same section, M. Cornu presented a detailed sketch of the dark rays in the ultra-violet part of the solar spectrum, following the scale of the wave-lengths adopted by M. Angstrom in his memoir on the normal spectrum of the sun. These sketches have been made from the micrometric measurement to the microscope of photographic plates, forming two series. The first series has been obtained with the assistance of Nobert's network (of 1801 lines), of which the second spectrum was very perfect and very luminous; the proof obtained contained the rays G, H_1, H_2, L, M, N, O, P, and even Q, although the object glasses of Gonionde were of crown and flint glass. M. Cornu has verified the correctness of the measures of M. Mascart; the comparison of the results has always shown four common figures in the numerical expression of the wave-lengths. The second series, intended to furnish details of the rays of the photograph, proceed from a spectrum much dispersed and of great delicacy; more than twenty-five were counted between the two rays H_1 H_2. This spectrum was obtained with a flint prism of 60°; the moist collodion had taken an impression even up to the ray O, in spite of the absorbing power of the two object-glasses. M. Cornu strongly recommends the use of these ordinary achromatic object-glasses for obtaining photographs of the ultra-violet region, when it is desired to go no farther than ray O; the inconvenience of the absorbing power is largely compensated for by the ease of setting in position and by the angular size of the region where the lines are clearly distinguished. With regard to the process itself, it differs but little from that of M. Mascart. M. Cornu, however, advises the adoption of a small dark chamber exterior to the telescope, in place of the photographic camera of M. Mascart. The advantage of employing collodionised surfaces sufficiently large to make room for the spots and other defects, arises from the thinness of the plate. M. Cornu further indicates upon his sketches the principal lines of magnesium, lime, manganese, and iron, which, as is known, compose the greater part of the groups, L, M, and N.

M. Saint-Loup followed with a paper on a proposed modification of Holz's machine. Other papers of interest in this department were by M. Potier on "The Elliptical Polarisation produced by Vitreous Reflection;" by M. Petit on a Modification of the Ordinary Telegraphic System, by means of which the telegraph may be made to print directly; M. Descloizeaux a very curious paper on some of the Optical Phenomena of Crystals; and M. Gariel on the Distribution of Magnetism in Magnets.

In the Chemical Section, presided over by M. Balard, M. Berthelot read a paper of considerable practical value "On the State of Bodies held in Solution," in which he brought much discrimination and research to bear upon some of the ordinary problems of Chemistry, especially upon molecular mechanics. Other papers in this section were by M Jungfleisch "On the Transformation of Tartaric Acid into Racemic Acid," and by M. Filhol "On the Nature of the Sulphuric Composition of the Mineral Waters of the Pyrenees."

In the Section of Zoology, under M. Soubeiran, various interesting papers were read, including one by M. Chatin, "On the Odoriferous Glands of some Mammiferous Animals;" one by M. Jobert, "On the Organs of Touch in Fishes;" one of considerable interest by M. Soubeiran, on the Oyster Culture at Arcachon; and one by M. Pouchet on the Colourisation of Fishes.

The Geological Section was presided over by M. Raulin, Professor at the Faculty of Sciences at Bordeaux, and in the section devoted to Mathematics, Astronomy, Geodesy, and Mechanics a paper was read by Madame Hureau de Villeneuve "On the Mechanism of the Flying Apparatus of Birds." She was preceded by M. Respighi, who read an able paper on the Scintillation of Stars. In accordance with a long series of observations made by him, he showed that the changes in brightness and colour presented by stars near the horizon are caused by momentary and successive deviations of luminous rays of different colours, by which these rays are carried outside of the object-glass of the telescope or the eye of the observer. These deviations are produced by extraordinary refractions or irregularities in layers of air condensed or rarified, placed at great distance from the observer, and at the precise spot where by atmospheric dispersion the rays of the different colours directed by the object-glass are separated from one another, so as to be only partly contained in the irregularly refracting layers of air. The most important result of M. Respighi's observations is this :—the layers of heterogeneous air are not reached by the luminous rays of different colours by means of the internal movement of atmospheric masses, but by their general movement caused by the rotation of the earth; which shows that the rotation of the earth is one of the principal elements in causing the twinkling of the stars. M. Respighi next described a very ingenious zenith telescope by means of which he can obtain the zenith distance of stars in their passage across the meridian.

In the Section of Geography, Political Economy and Statistics, various papers were read on methods of education.

The first session of the Association lasted eight full days, during which excursions were made to Arcachon, and to the Troglodyte Caves of Eyzies. On the condition and civilisation of the people whose remains they contain, M. Broca gave a very interesting lecture, in which he concluded that these men were savages, but in a state of partial civilisation, having at their disposal abundant food, and consequently leisure, applying themselves to the arts, and already exhibiting the perfectibility of the race. Another excursion was made to the Pointe de Grave, and one, which lasted three days, to the Industrial and Scientific establishment of Landes as far as Bidassoa. The Monday, Wednesday, Thursday, and Friday were devoted to *séances*, the morning for the sections, and the afternoons for general meetings. These public meetings were well attended, especially the evening lectures of MM. Broca and Cornu, who had audiences numbering about 800. Much interest was also manifested in the narratives of MM. Janssen and Respighi, who recounted the results of their researches into the constitution of the sun, and of their visit to India last year.

The reports of the Congress speak in lavish terms of the hospitality and considerateness in all respects of the Bordelais, whose city seems to be one of the foremost in France in respect of educational and scientific institutions. There can be no doubt about the success on the whole of the first meeting of the French Association; and we only hope that by the time it re-assembles at Lyons next year it will have advanced to the same ratio as it has done from its foundation till now, and that ere very long it will have taken as firm root as a recognised and universally beneficial French institution, as the British Association has done among ourselves.

ON PULSE FREQUENCY AND THE FORCES WHICH VARY IT[*]

THE circulation of the blood is an uniform circulation, the pulsations being neglected, and a uniform circulation is one in which the quantity of fluid flowing through all segments of the circulating system is the same; otherwise there would be a tendency for the fluid to accumulate at certain points, which is contrary to the premises.

To arrive at precise conclusions respecting the circulation there are two points which must be considered—1st., The laws which regulate the flow of fluids through capillary tubes. 2nd., The variations in the capacity of the circulating system under different pressures. These will be considered separately. Poiseuille found that the flow of fluids through capillary tubes varies directly as the pressure and as the fourth power of the diameter of the tubes. The author has verified the former of these results on the vessels of the animal system by a different method. Respecting the capacity of the arteries and ventricles under different blood pressures, it is evident that the capacity of the former must depend on the pressure only, for they are simple elastic tubes, and must be more capacious under high than under low pressures; reasons are given below for a more precise statement of this relation. To maintain a uniform circulation with a pulsating motor, like the heart, it is evident from the above considerations that variations in the resistance at the small arteries must produce variations in pulse-rate; and that unless the capacity of the arteries and heart vary directly as the pressure, variations in blood pressure must be also attended with change in pulse frequency. That the capacity of the ventricles is dependent on the arterial blood pressure can be proved by the varied amount of opening up of the ventricular cavities which follows different fluid pressures in the coronary arteries.

Next, the different forces which vary the pulse-rate must be considered. It can be shown that any change in the resistance to the flow of blood through the capillaries varies the pulse-rate, increased resistance rendering the pulse slower and the reverse. As instances of these effects may be given, the pulse-slowing effects of stripping in a cold air, of a cold bath, and of compression of large arteries; the pulse-quickening effects of a hot bath, whether air or water. Numerous experiments by the author prove that the effect of copious blood-letting is not to modify the pulse-rate at all, thus showing that the law given by Marey respecting pulse frequency is not correctly based. The above points, namely the law of Poiseuille, the dependence of the capacity of the arteries and ventricles on the pressure of the blood, the dependence of the pulse-rate on the peripheral resistance and its non-dependence on the blood pressure, can all be correlated by only one theory, namely, that the heart always re-commences to beat when the tension or pressure in the arteries has fallen at invariable proportions, which also assumes, that the capacity of the heart and arteries varies directly as the pressure. The facts that the arteries are generally empty after death, and that the cavity of the heart is sometimes found to be obliterated on *rigor mortis*, show that absence of pressure and capacity go together.

This theory explains the known peculiarities in pulse rate attending change in position, by showing that while standing all the pressure of the body weight is borne by non-compressible rigid tissues and so the circulation is normal, but while lying, the soft parts are compressed and resistance introduced into the circulation, reducing the rapidity of tension-fall, and therefore the frequency of the pulse; an intermediate condition attends the sitting posture. The pulse quickens during inspiration, and becomes slower during expiration; for during the former act the reducing pressure in the chest lowers the aortic blood pressure, and makes the tension-fall more rapid, while in expiration the reverse occurs.

This theory also is the only one which throws light on the cardiograph law published by the author (see *Journal of Anatomy and Physiology*, 1870–73), which may be thus stated —For any given pulse-rate the first part of the heart's revolution has a constant length, but it varies as the square root of the length of the complete pulsation. The pulse-rate not depending on the blood pressure, and the length of the first cardiac interval not varying when the rate is constant, its length also does not depend on the blood pressure. The first cardiac interval may be divided into the systole and the interval between that and the closure of the aortic valve (the diaspasis); these combined not varying as the blood pressure, it is almost certain that separately they do not do so either; so it may be said that neither the length of the systole nor of the diaspasis depends on the blood pressure. But the fall of tension between the pulse beats being but small, and the diaspasis length not depending on the blood pressure, there is no reason why it should vary in length with different pulse-rates; and assuming this in connection with the measured diaspasis length in a particular case (·00183 of a minute), it can be deducted from the above cardiograph law, that the systolic length varies as the square root of the diastolic. From these facts the relation of the nutrition of the heart to the time of heart nutrition (diastole); and to the blood pressure, may be deduced; for the systolic length not varying with the blood pressure when the pulse rate is constant it is evident that the cardiac nutrition must vary directly as the blood pressure in the aorta; and the systole varying as the square root of the diastolic time, shews that the nutrition of the heart varies as the square of the time of nutrition (diastole), for with a quadruple resistance to the peripheral circulation, the heart would be four times the time in emptying itself, but it is only double that time, which demonstrates the statement.

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[*] Paper read before the British Association at Brighton in Section D, Department of Anatomy and Physiology,) by A. H. Garrod.

A complete logical explanation of the action of the pneumogastric can be given on this theory, by assuming that its function consists in diminishing the calibre of the small arteries of the coronary system, and always keeping them somewhat contracted.

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*PHENOMENA OF COAGULATION IN FROG'S BLOOD**

I WAS endeavouring in the autumn of last year, at Prof. Sanderson's instigation, to demonstrate upon the frog some of Brucke's fundamental experiments on the coagulation of the blood, which he performed on the tortoise; I was surprised at the apparent failure of some of them. For instance, having tied a glass tube into the animal's aorta and allowed it to fill with blood, I expected that which was in the tube speedily to coagulate, that which remained in the heart to continue liquid for a considerable time. But no such contrast was observable, both portions of blood remained perfectly fluid for an indefinite time. I say apparently; for, in fact, on subsequently turning out the blood, a slight film of coagulated fibrin was observable attached to the walls of the tube. Of course the corpuscles being the heavier gravitate to the bottom, and the blood thus becomes divided into two portions, a clear fluid above and a mass of red corpuscles below, with a thin filmy stratum of white again on the surface of the latter.

To show that the clear fluid is plasma and not merely serum, that is to say, that it fully retains its coagulability, it is sufficient to take a little up into a very fine, almost capillary, glass tube. The extent of surface to which it is thus exposed very quickly determines its coagulation.

Following up the subject still further, I found the same thing to happen when the blood is allowed to drop into a glass vessel, the whole remaining fluid, except that portion in immediate contact with the sides, the corpuscles subsiding as before, and the supernatant liquid being readily coagulable in a capillary tube.

But frog's blood does not always behave in this manner. It is not unfrequently the case, especially at this season of the year, that the blood of these animals behaves to all appearance precisely as we are in the habit of expecting that blood should behave, that is to say, the commencing subsidence of the corpuscles is arrested, the fluid solidifies, seemingly throughout. And, indeed, in rare instances, the coagulation is complete to the centre, and the mass soon separates into clot and serum, which latter, in these cases, never yields a coagulum in a capillary tube. More frequently, however, on breaking the surface with a knife, the interior of the coagulated mass is seen to be occupied by still fluid blood.

In either case, the coagulated fibrin soon begins to contract; and this contraction proceeds to such an extent that not only is the serum of the blood expressed from it, but it comes to pass that there is no longer room in its meshes for the involved corpuscles, which consequently begin to be squeezed out and to fall to the bottom of the glass. This diminution in volume of the clot may proceed so far that in the course of a few hours the blood may present an appearance precisely as if it had not undergone coagulation at all, there being a mass of corpuscles at the bottom of the vessel, and a clear supernatant fluid. The contracted remains of the clot may however be always found, although often obscured by the liberated corpuscles. Now, this disappearance of the clot of frog's blood under certain circumstances was noticed some years ago by v. Recklinghausen, and ascribed by him to a re-liquefaction of the fibrin; and not unnaturally, if we consider the astonishing diminution in bulk which it undergoes, and the fact that the serum in such cases is frequently found to yield a further coagulum.

But in every case of the latter kind, *i.e.* in every case in which the supernatant fluid yields a coagulum in a capillary tube, it will have been found that the primary coagulation was incomplete, *i.e.* that the central parts of the blood remained fluid, whereas on the other hand it is certain that when the primary coagulation has been complete, no further coagulum is *ever* obtainable, although, in this case also, the clot may have contracted to a relatively exceedingly small bulk, in fact, may have almost disappeared.

A further proof, if one were needed, that the diminution of the clot is due merely to contraction and not re-liquefaction of fibrin, is to be found in the examination under the microscope, using an immersion objective, of the process as it occurs in a very thin walled and fine capillary glass tube.

The phenomena here observed are wholly those of contraction; first simply serum, then white corpuscles, and finally red corpuscles being expressed, until a mere thread of fibrin remains, almost obscured by the corpuscles and still including a few.

Throughout the whole process, however, there is no trace of a re-liquefaction of fibrin; this would of course involve the dropping away of the corpuscles from the sides; on the contrary, they are most evidently squeezed out, some of them being actually ruptured in the passage and appearing on the exterior of the clot as small reddish spheroids. The facts then, briefly, are these: that frog's blood, especially if taken during the winter months, exhibits but very little tendency to coagulate, with the exception of the portion in immediate contact with a foreign surface; that, when apparently coagulated throughout, the central portions are very apt to remain fluid, and to impart coagulability to the expressed serum; that the clot when formed frequently tends to attain a relatively very small bulk; and, finally, that this diminution in bulk is due to contraction merely, not reliquefaction of the fibrin.

PHYSICS

Acoustic Experiments on the Seine during the Siege of Paris

IN the experiments made by Colladon and Sturm on the Lake of Geneva, in 1827, to determine the velocity of sound in water, the source of sound was a bell, weighing sixty-five kilogrammes, fixed to a boat immersed in the water near Rolle. Another boat, moored near Thonon, carried the observers, who employed a long acoustic tube made of metal, one extremity of which, widened and closed with a membrane, was thrust into the water. The distance from Rolle to Thonon is about 13,500 metres, so that the range of the sound was considerable. The water in that part of the lake is of great depth.

During the siege of Paris, the idea arose of establishing an acoustic telegraph by means of the Seine, between the invested city and provinces that had not been invaded. The Geneva experiments appeared to favour the proposal.

M. Lucas was charged by the Minister of Public Works to make some experiments on the subject, which he accordingly did in November, 1870. He gives an account of these to the Paris Academy.

In the first series, a bell weighing forty kilogrammes was lowered by a windlass from the bow of a barge, to a position twenty or thirty centimetres from the bottom. It contained a clapper, which was moved by means of wires carried up to the barge. Two workmen were charged to ring the bell at certain fixed intervals, while the observers, in another boat, marked the effect at different distances, being carried along by the current. The acoustic tube employed was 1·50m. long, and the membrane of its orifice, immersed in the water, was turned towards the bell. At the distance of a few metres, a dull sound was heard (like that of a drum beat with a drum-stick), at each stroke given to the bell. The intensity diminished with the distance, and the sound ceased to be perceptible at about 1,800 metres. The result was constant for experiments repeated at different parts of the river.

In a second series of experiments, a bronze bell, weighing 354 kilogrammes, was used. This was hung in a wooden frame weighing 446 kilogrammes, constructed in the form of a quadrangular pyramid. The hammer of the bell weighed sixteen kilogrammes, and was moved by wires, as in the other case. The frame and bell were suspended by chains from the four corners, between two barges, and then lowered into the water. The mode of observation was the same as in the former case.

A few metres from the barge a slight metallic sound was heard, doubtless from the acoustic tube vibrating with the membrane. The sound soon became dull, its intensity decreased rapidly with the distance; at 1,400 or 1,500 metres there was no perception of it.

Comparing these experiments with those of the first series, we have the unexpected result that the very intense sound of a bell weighing 354 kilogrammes has a less range than the weaker sound from a bell of forty kilogrammes.

In a third series, a small bell, twelve centimetres diameter, was sounded in the water alternately with the bell of forty kilogrammes; the range of the latter extended, as before, to 1,600 or 1,800 metres; that of the little bell was small, it exceeded one kilometre, however. M. Lucas concludes from the experiments that the range of sound in a river, even in the direction of motion of the water, is much less than that of sound in a lake; and that by increasing considerably both the intensity and the gravity of the sound, the range is but little increased, and may even be diminished. It further appears that, with equal intensity, the range of sound in a river will increase with its acuteness. If so, a considerable range ought to be obtained, he thinks, with a blast of compressed air for the sonorous source. A. B. M.

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* Paper read before the British Association at Brighton in Section D (Department of Anatomy and Physiology), by E. A. Schafer, M.B.

SCIENTIFIC SERIALS

THE *Geological Magazines*, Nos. 97—99 (July to September 1872).—One of the most important contributions contained in these three numbers is a translation of Prof. Nordenskiold's account of his expedition to Greenland, which runs through the whole of them, and is not yet completed. With a good deal of general matter, this paper includes a vast amount of interesting geological information, and it must be welcome to English geologists, few of whom could make much out of it in its original Swedish dress. This translation is illustrated with the original plates, maps, and woodcuts.—Another highly important memoir, which appears in two parts in the July and August numbers, is Dr. H. B. Holl's essay on those most puzzling objects, the fossil sponges.—In the August number Prof. Allman gives us a note on a fossil *Hydractinia* from the Coralline Crag, the *Cellepora echinata* of Michelin.—Mr. S. V. Wood, jun., has some further remarks on Mr. Geikie's correlation of the glacial deposits; and Mr. J. Lucas a paper on the Permian Beds in Yorkshire, one portion of which calls forth a note by Mr. J. Clifton Ward, on rock-staining, in the September number.—The latter further contains a notice of the occurrence of the genus *Cupressocrinus* in the English Devonian, near King's Teignton, the first part of a paper, by Mr. Alfred Tylor, on the formation of Deltas, and some other papers, among which we may mention especially Mr. Woodward's description of a new species of Phalangiiform Arachnide (*Architarbus subovalis*) from the Lancashire Coal Measures, which is especially interesting from its generic identity with a North American form.

THE *Memoirs of the Natural History Society of Bremen* for 1872 (Abhandlungen herausgegeben vom naturwissenschaftlichen Vereine zu Bremen, Band iii. Heft 1) contain some exceedingly valuable papers for the study of zoological and botanical geology. In the former department we have Dr. O. Finsch's contribution to the ornithology of North-western America, from which inhospitable region the author records about 120 species of birds, many of them possessing a very wide geographical range. Singularly enough the author does not describe a single new species, but of some variable forms detailed descriptions are given, and with regard to many others we find valuable synonymic and descriptive remarks.—The flora of high northern latitudes receives a contribution in the shape of a list of the vascular plants of Spitzbergen and Bear Island, by Dr. T. M. Fries.—The student of the geographical distribution of European plants will find most valuable information in M. C. Noldeke's Flora of the East Frisian Islands (including Wangeroog) which treats of the vascular plants in a most exhaustive fashion, and is supplemented by a short notice of the mosses of the islands, by M. C. E. Eiben.—From Drs. Buchenau and Focke we have an important paper on the *Salicorniæ* of the Baltic coasts of Germany, which includes a discussion of the views of previous authors on the species of that genus.—Dr. Buchenau has also a paper on the variation of the curious bracts of the lime tree.

SOCIETIES AND ACADEMIES

SAN FRANCISCO

Academy of Sciences, May 20.—Dr. Blake, on presenting some stone implements that had been found in stratified rock, at an elevation of 1,700 feet, observed there can be no doubt that up to the present time the earliest traces of man's existence on the earth have been found in this country. The skull found in the Pliocene deposits in Table Mountain is certainly the oldest human skull that has yet been discovered. Many stone mortars have also been taken out from the same deposits. I have examined the spot where one of these mortars was taken out ten feet beneath the surface of the Pliocene gravels on the Sierras and as this was some six hundred feet above the valley and near the top of a rolling hill, there was no possibility of the strata having been disturbed. The objects I now have to present to the Academy furnish important evidence on this point. They are two implements cut in serpentine, and evidently fashioned by the hand of man, or of some animal capable of using its anterior extremities so as to fashion objects to meet its wants, and apparently possessed of sufficient intelligence to use lines or nets for catching fish, as it would seem that these instruments must have been used as sinkers. They are cut in serpentine, the surface of which is slightly weathered to the depth of about $\frac{1}{20}$ of an inch. One is pear-shaped, $\frac{3}{4}$ in. long, and $5\frac{1}{2}$ in. in circumference at its largest part; near the smaller end is a hole passing through it, and immediately above this is a notch passing across the end. The other instrument is cylindrical, thicker in the middle than at the ends; it is $5\frac{1}{2}$ in. long, and $3\frac{1}{2}$ in. in circumference at its thickest part. There is a hole through it at about an inch from one end, and above the hole is a notch passing across the end. The spot where they were found is on a rolling hill within a few feet of the summit of the coast range, and at an elevation of about 1,700 feet. They were met with in digging away the side of the hill, at about eight feet from the surface, four feet being alluvium, and four feet argillaceous shales, in which they were found embedded. These, and four other instruments of the same form, were found in a space of about two square feet. A great deal of alluvial soil had been removed whilst levelling the ground for a house, but no instruments had been found until the shales were dug into. In company with Prof. Whitney I visited the spot, and we have not the slightest doubt but that they were taken out at the place indicated. As to the geological age of the rock in which they were embedded, this is to a certain extent undetermined. It certainly cannot be later than the Pliocene, and Prof. Whitney is of opinion that it is still older. Dr. Blake then made some remarks as to the absence of anything like a rim to the Great Basin, north of the Puebla Mountains. In going north from the valley of the Humboldt near Mill City to the Puebla Valley, the highest elevation crossed between the Rattlesnake and Vicksburg ranges did not exceed three hundred feet, whilst the country to the east of this, between the Rattlesnake and Umsham ranges, is at a still lower level. North of the Puebla Buttes nothing but some low ranges are found until we reach the head waters of the Owhyee, a tributary of the Columbia, so that there can be no doubt but that the larger part of the waters of the Great Basin must have escaped through the valley of the Columbia. The quantity that was left for evaporation did not probably exceed from 100 to 150 feet, as he had not found concretionary deposits at a greater height than 100 feet above the present level of the Humboldt.

BOOKS RECEIVED.

ENGLISH.—The Vegetable World L. Figuier, English edition (Cassell and Co.).—A Manual of Microscopic Mounting: J. H. Martin (Churchill).

FOREIGN.—(Through Williams and Norgate).—Thesaurus Ornithologiæ, 1er Band, 2te Heft: Dr. C. G Giebel.

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THURSDAY, OCTOBER 3, 1872

BOTANICAL MUSEUMS

THE question of the future relations of the national herbarium at the British Museum to that at Kew is at present engaging the attention of the Royal Commission on Science. The minute of 24th July last shows that it will presently be dealt with by the Treasury. On the motion of Messrs. Bentham and Ball, at the recent meeting of the British Association at Brighton, the Committee of the Biological Section secured an instruction to the Council of the Association to take action in connection with this question in the interests of botanical science; and the question has been submitted to the readers of NATURE in an anonymous article on "Botanical Museums," published on the 23rd March last year.

The authorship of this article was unknown to me until I was summoned to give evidence before the Royal Commission on Science. At the request of the President of the Commission, the proof of Mr. Bentham's evidence was placed before me; I then saw that the article alluded to was incorporated in it, and in the statement with which Mr. Bentham introduced it I read his history of its preparation and publication.

In dealing with this part of Mr. Bentham's evidence, I informed the Commissioners (Q. 7,739-40) that the opinions and arguments stated in it had been, two years before, submitted by Dr. Hooker, through the Board of Works, to the trustees of the British Museum, and had been answered by my predecessor, Mr. Bennett, to the satisfaction, as I then understood, not only of the trustees, but of the authorities at the Board of Works, and that the article on "Botanical Museums" was merely a reproduction of this official paper, without any reference to its answer.

After supplying the date of this official document, my examination, under the guidance of the Commissioner who was then dealing with me, took another direction. On subsequent reflection I therefore felt it necessary to ask leave to present a detailed reply to the Commissioner, which was granted. This reply is printed with the other evidence in the recently printed Blue Book (p. 44).

That the readers of NATURE who have already perused Mr. Bentham's article may have the opportunity of considering my answer, I submit it to them, earnestly desiring that this question now raised should be fully and exhaustively considered, and that no hasty or one-sided judgment should be arrived at; being thoroughly convinced that the action to be taken by the Treasury, whatever it may be, will seriously influence the whole future of the science of botany in England.

I desire to submit to the Commissioners my views:—(I.) On the statements contained in Mr. Bentham's paper, and (II.) on the matters naturally flowing out of these statements.

I. *The statements contained in the paper*

1. The views expressed by Mr. Bentham regarding the main purposes of a botanical museum and herbarium, and the requirements of a collection for such a close study of plants as would supply a "sound foundation upon which the science of botany can be usefully established," arise from his estimating the science of botany as limited to that particular department of it to which he has devoted his life, and in which he has done important service. The profound study of plants is, in his view, "their accurate determination and practical classification," and he states that he requires for its prosecution nothing more than an exhaustive herbarium of the fragments of plants supplying the diagnostic characters at present employed for distinguishing genera and species, with a complete library and staff of officers. This is, in my opinion, a very defective estimate of the science of botany, and of the materials required for its advancement.

Robert Brown took a very different view of the profound study of plants, and in the Botanical Department of the British Museum he tried to develope that masterly grasp of the science which is to be found in his works, by illustrating, as far as possible, the structure of all plants from the lowest to the highest, both existing and extinct. Accordingly, the National Herbarium, large as it is, forms but a part of the botanical collections. The specimens placed in the outer rooms, which exhibit chiefly the form and structure of the stems and roots of plants, are as necessary a part of the purely scientific collection as the dried foliage and flowers in the herbarium. While such specimens "excite the interest," and "gratify the curiosity" (and, what is more important, instruct the minds) "of the general public," these are very far from being their principal, still further from being their only purpose in a botanical museum, as Mr. Bentham appears to imply. The scientific investigator whose notion of systematic botany is somewhat larger than ascertaining the technical name and order of a plant, consults these specimens as he does the herbarium. It is, therefore, a mistake to suppose that they, "when once placed, require no further handling."

The purely scientific collection of the British Museum consists of:—I. The herbarium, comprising (*a*) the general herbarium, (*b*) the British herbarium, (*c*) various separate small and complete herbaria of historical interest. II. The structural series, comprising (*a*) the fruit collection, (*b*) the collection of gums, resins, and other natural products, (*c*) the general collection, exhibiting the form and structure of plants, and consisting of the larger specimens chiefly exhibited to the public; and (*d*) the microscopical preparations, illustrating the minute structure of recent and fossil plants.

2. The limitation of the science of botany to the plants now existing on the earth is another grave defect. No subject has recently received more attention from biologists than the relation between existing and extinct plants and animals. Every philosophic estimate, or systematic classification of the one kingdom or the other must include the fossil as well as the recent. This is fully acknowledged and acted upon by zoologists, and no better illustration can be adduced than Prof. Huxley's "Introduction to the Classification of Animals" (1869). In botany also, in the standard and only complete *Genera Plantarum*, by Endlicher, the fossils are ranged in their systematic position with the recent plants. It is true that the *Genera Plantarum* now in progress, of which Mr. Bentham is one of the authors, ignores all extinct plants. This retrograde step is in entire accordance with the views expressed by Mr. Bentham in NATURE. A systematic account of the *Lycopodiaceæ* which took no notice of the arborescent forms of palæozoic age, or of the *Cycadeæ* which ignored the numerous forms and remarkable variations of this order in the secondary rocks, would be obviously very incomplete and unsatisfactory. In forming a collection to supply a sound foundation for the science of botany, it would be as reasonable to exclude the plants of any existing botanical province—say Australia—as to omit those which have existed at any particular period of the earth's history—say that of the Wealden.

3. The distinction which Mr. Bentham draws between a herbarium "for the close study of plants" and one for their "rapid determination without dissection" is most

undesirable, and, in my opinion, practically impossible. No botanist has so extensive an acquaintance with the vegetable kingdom as to be able to make "a close study," in his necessary work, of every group of plants he may be naming or arranging; he must in many groups make a "rapid determination without dissection." If Mr. Bentham's distinction were in force, and the two herbaria he proposes existed, he would himself, when rapidly naming some of the important collections which have passed through his hands, have often been driven from the great scientific collection to work in his single specimen herbarium with the "general naturalist," "the palæontologist," and "the mere amateur." Every systematic botanist is at first, and more or less all along, a "comparer" of plants. The man who begins as a mere comparer naturally becomes a close student under the influence of the collection he is consulting, and the workers he encounters in that consultation.

4. Mr. Bentham's single specimen herbarium is chiefly intended for the palæontologist, and in addition he proposes to provide him with "separate collections of leaves and fruits, . . so arranged as to enable them to be rapidly glanced over," and these, it is added, "would be most useful." No better testimony to the utter worthlessness of such materials for the purpose proposed can be adduced than the criticisms of Mr. Bentham himself, on the evidence for the existence of the natural-order *Proteaceæ* in Europe, from leaves found in Tertiary strata. Mr. Bentham was specially fitted to deal critically with the hundred fossil species referred to this order, as he had just made the analysis and detailed descriptions of between five and six hundred *Proteaceæ*. The Order is also the best fitted to test the value of the leaf characters on which the fossils had been referred to it, because, as he testifies, it "is one of the most distinct and most clearly defined amongst phanerogams," and is without "a single plant intermediate in structure between that and the nearest allied orders." With regard, then, to the leaves of this order, Mr. Bentham says, "I must admit that there is a certain general *facies* in the foliage of this order that enables us in most, but not in all cases, to refer to it with tolerable accuracy—leafy specimens known to have come from a proteaceous country, even without flowers or fruit—but as to detached leaves, I do not know of a single one which, in outline or venation, is exclusively characteristic of the order, or of any one of its genera." I cannot reconcile this declaration by Mr. Bentham to the Fellows of the Linnean Society as their President in May 1870, with the statement published by him within a year thereafter, that such a collection of detached leaves not for a limited and exceptionally defined order, but for the whole vegetable kingdom, "would be most useful."

I must further observe that Mr. Bentham has overlooked the fact that a large proportion of fossil plants have been determined from their internal structure, that is, on evidence which no mere herbarium, however extensive, can supply, far less one for rapidly determining plants without dissection, or a collection of detached leaves. The palæontologist requires the most extensive collections possible for his work, and he must be a working zoologist or botanist. All such work done by mere "geologists" and on such data as Mr. Bentham proposes to supply would always deserve strong condemnation.

II. *The matters flowing out of these statements*

In considering the matters naturally flowing out of Mr. Bentham's paper, and the views I have now expressed, I venture, *firstly*, to submit the reasons which make it desirable in my opinion to retain the two herbaria as separate and independent institutions.

1. The two herbaria already exist, and are to a considerable extent parallel collections. Mr. Bentham, whose extensive private herbarium formed the foundation of the public herbarium at Kew, declared, in 1858, "that a great portion of the additions to the Banksian herbarium since Sir Joseph's death are duplicates of those already at Kew." As the Banksian plants form less than a quarter of those now existing in the British Museum herbarium, the duplicates would be, according to Mr. Bentham, about three-fourths of the whole. Sir William Hooker, also, whose large collections form the great bulk of the Kew herbarium, testified, in 1858, that "the Museum specimens are to a great extent duplicates of those at Kew." And the present Director of Kew Gardens corroborated this statement at that time. In 1860 Sir William Hooker further said, in reference to the transfer of the National Herbarium to Kew as affecting the herbarium there, "To Dr. Hooker and myself it literally and truly can be a matter of no consequence."

2. The two herbaria have been under different management, and to some extent express different results of "the close study of plants." The important bearing of this consideration on botanical science in Britain can scarcely be overestimated. One practical illustration may be adduced The most varied views are entertained by botanists as to the limits of a species, and consequently as to what constitutes a duplicate. Thus, in the case of the indigenous flowering plants of Britain, Mr. Bentham considers them to form 1,274 species; Dr. Hooker, in his recent Flora, makes 1,473 species; Prof. Babington increases the number to 1,648 species; while a botanist adopting the views which Jordan and some continental authors have applied to local floras, would make them three or four times more numerous than even the last estimate. It is quite obvious that these different botanists have each very different notions as to "duplicates," and that a distribution undertaken by Mr. Bentham would certainly result in the loss to the herbarium of plants which Dr. Hooker would consider good species, and the "duplicates" distributed by Mr. Bentham or Dr. Hooker would include numerous plants which would be of the utmost value in M. Jordan's eyes. The two herbaria, existing, as they do, under different directors, to a considerable extent counteract these and other analogous evils.

3. The objects of the two herbaria are fundamentally different, and in as far as they fulfil these objects, they are employed for totally different purposes. The National Herbarium at the British Museum was founded in 1827 for the use of the scientific botanist; while that at Kew was, as Dr. Hooker says, "originally maintained expressly for the use of the gardens." This was the primary object for which Sir W. J. Hooker accepted the private herbarium of Mr. Bentham in 1855. Before that year the gardens had been fulfilling their proper functions without a scientific herbarium attached to them. The two editions of the "Hortus Kewensis" are the best testimony to the efficiency of the gardens, and to the value of the collections brought together there under the Aitons. No herbarium of any kind, I believe, existed at the gardens during their time. The Banksian Herbarium was often and for a long time systematically used for naming the Kew plants; and the strictly scientific portion of the "Hortus Kewensis" was the work of Solander, Dryander, and Brown, the successive curators of the Banksian Herbarium. Even Sir W. J. Hooker, the successor of the younger Aiton, who raised the gardens to their present eminence, had no public herbarium from the time of his appointment in 1841 to 1855. It is, therefore, evident that a great scientific herbarium is not a necessity to the efficiency of the gardens at Kew.

It is, however, certain that such a herbarium as Sir W. J Hooker and Dr. Hooker desired, that is, one sufficient to enable the officials to name the plants in the gardens, would be a most useful adjunct at Kew, as it would save the great waste of time which would be incurred in consulting a herbarium at a distance. Inasmuch as growing plants are, to the extent that they are developed, perfect, and permit thorough examination, it is obvious that the single specimen herbarium proposed in NATURE would meet all the requirements at Kew, and this could be kept up as suggested by Mr. Bentham from the duplicates not required in the great National Herbarium, all being accurately named before being sent.

4. The practical difficulties in the administration of two separate, and to some extent independent, herbaria would be numerous and serious, and in the course of time a condition of things similar to what at present exists would result. It is needless to speak of a London herbarium, consisting of single specimens of each species, because such a herbarium, if practicable, would, as I have already shown, be utterly worthless for the purposes to which it is proposed to be applied. If the London herbarium were to contain only specimens sent by the keeper of a herbarium, whose notion of the science of botany was confined to the "accurate determination and practical classification" of herbarium specimens, it is obvious that the palæontologist would not find there the materials for prosecuting his work. If, on the other hand, the London herbarium were constituted to be of real use to the palæontologist, the keeper must have the power of acquiring as opportunity offered the suitable materials, and he would necessarily secure collections which a future agitator might demand to be transferred to Kew, with as pertinent reasons as those Mr. Bentham now employs.

5. It is not an unimportant consideration that the continued separate existence of these two great herbaria is a great security against their destruction by fire.

6. The expense of the two herbaria is very small. I am unacquainted with the amount granted for Kew herbarium, but it cannot greatly differ from that required by the National Herbarium, which amounted for the financial year lately completed to 1,767*l.* I know of no way in which the country can at once advance the interests of science and encourage its students, at a smaller cost and with more important results than by maintaining in their full efficiency the two botanical collections at present existing.

But, *secondly*, it must be admitted that the formation of a single great national botanical establishment, comprising the two public herbaria now existing within a comparatively small distance from each other, is a very attractive scheme, and should the Commissioners think that its realisation is desirable, I submit the following considerations as in my opinion essential:—

1. It must form part of the National Museum of Natural History. Such a museum, as far as it is an exhibition of biological science, will consist of animals and plants, both existing and extinct. It is absolutely necessary in the study of geology that the plant remains should not be separated from the animal remains; and further, it is as necessary for the satisfactory interpretation of the fossil plants, as well as for forming a true estimate of the vegetable kingdom that the recent plants should not be separated from the fossil. The separation of any one department would be a serious injury to all.

2. It must represent the whole science of botany, and not consist of only dried foliage and flowers, which constitute a herbarium properly so called; and consequently it must be formed on the principle adopted by Robert Brown, and exhibited in the Botanical Department of the British Museum, and not on the imperfect plan advocated by Mr. Bentham.

3. It must be placed in the position in which it will be most serviceable to the public and most accessible to botanists, and that place is beyond all question London. The statistics which I submitted on the occasion of my former examination establish this, by showing the extent to which the botanical collections at the British Museum are made use of. Further, it is universally acknowledged that a herbarium for scientific use must exist in London. The long experience of Mr. Brown and Mr. Bennett in the National Herbarium made them entertain and express very decided views as to this necessity. My shorter experience has been long enough to convince me that its removal to Kew would be practically placing it out of the reach of the busy men who frequently use it to the advantage of science. Of course the working botanist who devotes himself exclusively to the science would follow the collections wherever they went; but the active professional man, and the man of business, who devote their spare hours to botany, would be deprived of the assistance necessary to their work which they now obtain at the British Museum. That such men do a large proportion of the scientific work of the country may be shown in many ways, as for instance, by the fact that out of the nineteen botanical memoirs contained in the last two volumes of the Linnean Transactions, four are produced by professional botanists, and fifteen by others.

The late Prof. Henfrey, as representing the botanical teachers of London, Sir Charles Lyell, for the palæontologists, and Dr. Falconer, Mr. Bentham, and Dr. Hooker, have recorded it as their decided opinion that the interests of science require that a public herbarium should exist in London. Such a herbarium, even if used only by palæontologists, must be, as I have shown, as extensive as possible; otherwise, it will tend to mislead, like all other imperfect sources of information

I would further add in favour of London being the proper site for the national botanical collections, that important collections of plants, both recent and fossil, accessible to students, but not to the general public, now exist and must still remain in London. These are: (1) the Linnean herbarium, containing the plants described by Linnæus; (2) the great Wallichian herbarium; (3) the Smithian herbarium of British plants; all belonging to the Linnean Society; (4) the collection of fossil plants belonging to the Geological Society; and (5) the extensive public collection of fossil plants in the Museum of Practical Geology. The removal of the National Botanical Collection from London would so separate them from these collections as seriously to injure their value to scientific investigators.

4. The accommodation provided for the Botanical Department in the New Museum of Natural History, the plans of which have been accepted by the trustees of the British Museum, will be in every way superior to any that exist in the world, and will be amply sufficient to accommodate the proposed single national herbarium, as well as fully to display the structural, histological, and palæontological departments of the science. All the requisites specified by Mr. Bentham for the close study of plants, excepting the connection with a garden, exist to a greater or less degree at the British Museum, and some of them in a greater degree than at Kew. That living plants are a requisite adjunct to a herbarium is in opposition to the testimony of Mr. Brown and Dr. Falconer, to the effect that there is no necessary connection between a herbarium and a garden; and is opposed moreover to the testimony of Mr. Bentham himself, as well as to his declaration that his extensive systematic labours have all been based on herbarium specimens, although they have been carried on in close proximity to the finest scientific garden in existence.

In the event then of its being resolved to maintain only one great national botanical collection, I would submit that it should not be cut off from the allied biological collections, but be placed with them in the same building in London. And that for this end the collections presented by Mr. Bentham to the public, and all that have been added to them by purchase or presentation, be removed to London and incorporated with the national herbarium; and further, that the extensive botanical library formed at the national expense at Kew be made, with the Banksian library, the foundation of that national natural history library which will be required for the National Museum of Natural History.

It is necessary, in dealing with Mr. Bentham's printed and publicly expressed views on this matter, to bear in mind that he cannot be considered an unprejudiced witness. I have frequently referred to his relations to the herbarium attached to the Royal Gardens at Kew. He has thus stated the reasons by which he was influenced in presenting his herbarium and library to the public in 1855:—"I thought that at that time there was no herbarium and library in London sufficiently open for the use of botanists, and I presented them on condition that they should form the nucleus of a national herbarium and botanical library, to be kept at the expense of Government, and open to the free use of botanists." I can assert in opposition to Mr. Bentham's belief—and a similar opinion has been, I understand, recently expressed—that at that time the National Herbarium and the national library, as far as it is an adjunct to the herbarium, were fully and freely accessible to botanists, and were largely used by botanists; and this I am able to maintain from the contemporary records of this department, as well as from the testimony of botanists who were then in the habit of consulting the collections. Under the influence of this erroneous supposition, Mr. Bentham made his own herbarium a national institution, and a rival to the Banksian herbarium, and under the influence of this same spirit of rivalry, he now believes that there exists "a state of continual competition" between the two herbaria. I am sure that Dr. Hooker and the authorities at Kew will as strongly repudiate this statement as I do now, if it is meant to imply a competition in any way to the injury of science or the public. It is only in keeping with the motives which actuated him at the first that Mr. Bentham now agitates for the incorporation of the Banksian herbarium with that of which his own forms the nucleus.

WILLIAM CARRUTHERS

FRESENIUS'S QUALITATIVE ANALYSIS

Qualitative Chemical Analysis. By Dr. C. Remigius Fresenius. Eighth edition. Translated from the 13th German edition, by A. Vacher. (London: J. and A. Churchill)

THE present edition of Fresenius is one which will be looked at by chemists with interest. In the last so-called edition of Fresenius's Chemical Analysis, published in 1869, the text had been so altered and curtailed that the volume could scarcely be recognised as Fresenius of old, it having been reduced to about one half of its original size. It appears, however, that this condensation did not coincide with the author's views, and in this edition we have simply a translation of the original text. The editor in his preface confesses that he then, in the last edition, took too broad a view of his duty. It certainly seemed strange that a work which had passed through twelve German editions with a gradual and steady enlargement, could be condensed to about one-half without losing a considerable amount of its clearness and usefulness. We confess that we heard with pleasure of the appearance of the eighth English edition of this work, but, unfortunately, our pleasant anticipation has been to some extent marred.

In criticising this book, it will be necessary to consider it from two points of view; in the first place, to consider the work of the translator; and in the second, the author's responsibilities, and the book itself. To commence with the translator's work: it appears, on the whole, to be very well executed, although by far the greater part of the book is identical with the sixth edition, published in 1864, and edited by J. Lloyd Bullock; in fact, at first sight, there does not appear to be very much difference between the two editions; but on a closer acquaintance with the present edition, there is found a considerable amount of new matter, and some little alteration in the old. As is almost certain in a book of the size, we have found some sentences which would have been better for a little more attention; to quote one instance, p. 51, "Solution of ammonia, although formed by conducting ammoniacal gas ($N H_3$) into water, and letting that gas escape on exposure to the air, and much quicker when heated, may also be regarded as a solution of oxide of ammonium ($N H_4 O$) in water, the first acceding equivalent of water ($H O$) being assumed to form $N H_4O$ with $N H_3$." This sentence cannot certainly claim precision and clearness as its chief characteristics, and we have much doubt whether a young student would understand its meaning. Taking the translation, however, as a whole, it is clear and well expressed.

It will be seen from the above quotation that there are some points about this edition which will not recommend themselves to the generality of English teachers; we refer of course to the nomenclature and notation, which have not been altered since the edition of 1864. Of course Mr. Vacher is not responsible for this; if Dr. Fresenius said the work was to be literally translated, there was no help for it, the old notation must be used, but still we must consider that it is a great mistake. Generally speaking, at the present day for a book to be published in the old notation is sufficient to limit its use to a very small number of students. It certainly seems a great pity that this, which until lately has been looked up to as the best and most reliable text-book on qualitative analysis, should not have progressed side by side with modern chemistry; for English chemists, almost without exception, have adopted the new system of atomic weights, and the new and more systematic nomenclature now in use. The adoption of the old notation in the present volume will add a considerable amount of trouble to the teacher's work, and in many cases may probably lead to the adoption of another text-book. Looking at the very general, in fact almost universal, use of the new notation both in England and on the Continent, it certainly appears that this book is about five years behind the times. On the other hand, the present edition will, perhaps, on this account prove more useful to our manufacturing chemists, who seem very loth to adopt the new notation.

Although we do not feel satisfied, and, in fact, are disappointed, with the book in this respect, we cannot help feeling pleased with the substance matter itself. As might be expected from the numerous editions of his manual which the author has already published, he continues to keep the information contained in the work quite up to the progress of the science. Many parts of the work show alterations, though the very fact of the accuracy of the author's work precludes any very great change. It is in the parts on the rare metals, and on the alkaloids, that there appears to be the greatest amount of new matter. Thus the sections on berylla, thoria, zirconia, tellurium, vanadium, iridium, and didymium, show a considerable increase in our knowledge of these substances; whilst indium, which is not to be found in the edition of 1864, has here received a very good notice. Again, in the edition of 1864, only one method is given for the separation of lanthanum and didymium, but in the present there are no less than four distinct methods given. On the subject of the alkaloids, we notice three new articles on digitaline, picrotoxine, and atropia, which in the previous edition are not noticed. In the portion which treats of the acids, and in Part II. "On the Course of Analysis," there does not appear to be much alteration; but we must not omit to mention that the index to this edition is far more complete, and in every way better than in the previous editions. The general plan of the work is too well known to need any detailed account, and the number of editions through which it has passed is a sufficient guarantee of its usefulness and trustworthiness. A few new illustrations have been introduced, and a new table of spectra, but in the general style and plan of the book there has been no alteration. For our own part, although we have a great admiration for Fresenius's book, more especially as a work of reference, we scarcely think that his system is perfect for educational purposes, and perhaps not so good as those of some others, such as Valentin's or Galloway's; no doubt a student working conscientiously through the work under review will be able to make good and correct analyses, but we doubt whether he will learn much beyond the mere analytical details, for in this book there seems little room for the student to use his powers of originality, and nothing to stimulate him to reason, from his accumulation of facts, to general principles. There appears about the book almost too much of the system of telling this and showing that, for the book to be perfect as an educational agent, and we fancy that better results in this direction may be obtained from works which give more opportunity and encouragement for original and individual reasoning; and this we believe is the case in the two other works we have mentioned, as they tend to exercise and strengthen the student's originality, and will at the same time give him as full and complete a knowledge of qualitative analysis as he would obtain from Fresenius's book.

OUR BOOK SHELF

Verhandlungen des naturhistorischen Vereines der preussischen Rheinlande und Westphalens. 28^{er} Jahrgang. 1^{te} u. 2^{te} Halfte. (Bonn, 1871.)

THE volume of these Transactions for 1871 opens with biographical memoirs of Wirtgen and Haidinger. S. Simonowitsch contributes a paper on the Bryozoa of the Greensand of Essen, illustrated by four lithographic plates, which is introduced by a critical account of the anatomy and systematic position of the Bryozoa. From Prof. Förster we have a Review of the Genera and Species of the Family of Plectiscoideæ. F. G. Herrenkohl follows with a list of the Phanerogams and Vascular Cryptogams of Cleve and the neighbourhood. R. Bluhme gives a series of analyses of the water of different wells in the vicinity of Bonn, compared with that of the Rhine. In addition to these papers printed at length, a large number of other subjects connected with medical and natural science are treated in the Reports of the Proceedings of the Lower Rhine Society for Natural History and Medicine. Among these we may refer especially to a valuable paper by Dr. Brandis on the climatic conditions which principally affect the growth of forests in the British East Indies. The Indian climate is characterised by its long period of uninterrupted drought; and where the rainy season falls in spring or autumn, the summer heat is excessive. Where, however, the rainy season falls in the summer, as is the case in Burmah, Bengal, and a portion of Central India, the climate presents the peculiarity that the hottest period is in the spring, from March till May and the commencement of the monsoon, and again in the autumn, Calcutta having again a comparatively cold winter. The great obstacle to the growth of forests is the prevalence of fires towards the close of the dry season, which do incalculable damage every year; but of late years something has been done to limit their ravages. The growth of tree vegetation is extraordinarily rapid in India when young, but the forests do not eventually attain such luxuriance as in Ceylon, Brazil, and some extra-tropical countries.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Oceanic Circulation

THE letters of Prof. Everett and Mr. Wallace (NATURE, Aug. 22) establish one point which must go a good way towards the settlement of the disputed question of the cause of oceanic circulation, viz., that in order to maintain the bare mechanical possibility of the gravitation theory, it is necessary to assume that water is so nearly quite devoid of molecular resistance to motion that, were it not for the impediments offered by continents, water flowing from a low to a comparatively high latitude would be revolving eastwards with the velocity of an arrow. In the southern hemisphere, where continents are "few and far between," and where a comparatively open channel exists through which the waters may circulate round the globe at any velocity without much impediment, this rapid general eastward motion of the ocean ought to be developed to a large extent. But the fact remains that no such motion has ever been observed. Dr. Carpenter says:—"It is well known to navigators that there is a perceptible 'set' of warm surface water in all the southern oceans towards the Antarctic Pole; this 'set' being so decided in one part of the Southern Indian Ocean as to be compared by Capt. Maury to the Gulf Stream of the North Atlantic" (NATURE, March 24, 1870). This general motion of the water in the southern hemisphere Dr. Carpenter adduces as strong evidence in favour of his theory. But why is not the "set" as much to the east as to the south? If the presence of the Antarctic continent does not hinder the motion of the water polewards, why should the presence of the continents of Australia or the southern portion of South America hinder the motion of the water eastward, seeing that rotation performs about 1,500 times more work in deflecting the water eastward than the difference of specific gravity performs in impelling the water southward? The very fact that the water does not turn to the east but moves straight towards the Antarctic continent, shows that the waters must be impelled by a force immensely greater than that derived from difference of specific gravity, because it must be greater than that derived from rotation, or else the "set" would be as much to the east as to the south. There are, it is true, a few currents in the southern hemisphere with an eastern motion, but these the advocates of the gravitation theory would call "mere surface drifts produced by the winds." Besides the majority of the currents in that hemisphere move in wrong directions to be explained either by difference of specific gravity or by rotation.

That the explanation given by Prof. Everett and Mr. Wallace does not even touch the difficulty which besets the gravitation theory, far less removes it, will, I trust, be further evident from the following considerations, viz., a current in mid-ocean a thousand miles from land, flowing from a low to a higher latitude, has its eastward motion due to rotation as effectually checked and diminished as though it abutted against a continent. This retardation cannot be attributed to the presence of continents, for it occurs equally the same whether the land be one thousand, two thousand, or five thousand miles to the east. It is the resistance of the molecules of the water through which the current moves that destroys the eastward motion. No matter how slow the current may flow polewards, by the time the water reaches, say latitude 60°, each pound has lost at least 9,000 of the eastward velocity which it possessed when it left the equatorial regions. It is a matter of indifference in what way this energy is consumed by the molecules of the water, whether it be in friction in rotation, or whether it becomes potential in the raised water through which the current flows; for in either case it is the resistance offered by the stationary molecules which causes the moving molecules to lose their velocity. The resistance being molecular, that which holds true of eastward holds equally true of westward motion. This is proved also by the fact that a current flowing from a higher to a lower latitude has its westerly motion due to rotation as effectually checked and diminished as a current flowing from a lower to a higher latitude has of easterly. And what holds true of motion to the east or to the west, holds equally true of motion to the south or north, for there is no reason why the resistance should be less in one direction than in another.

It therefore follows that it is impossible that 6 foot-pounds could impel a pound of water from the Equator to latitude 60° against the molecular resistances to its motion, when during the passage of the pound of water it requires 9,000 foot-pounds to overcome the resistance to the easterly deflections which take place. Or if the molecular resistance of water be so infinitesimal that 6 foot-pounds is sufficient, then it is impossible that molecular resistance could consume 9,000 foot-pounds during the easterly deflection which takes place.

I respectfully submit that this is a clear and obvious demonstration of the mechanical impossibility of the gravitation theory of oceanic circulation.

Prof. Everett says that Mr Ferrel's argument from the tides is quite conclusive in showing that the forces arising from difference of temperature are of sufficient magnitude to keep up an oceanic circulation. If. Prof. Everett, like Mr. Ferrel, really supposes that a slope produced by the moon is the same as one produced by difference of density, and that the process by which the water tends to regain its level is the same in both cases, I am not surprised he should consider Mr. Ferrel's argument conclusive.

I beg to refer Mr. Wallace to the *Philosophical Magazine* for October 1871, p. 244, for an explanation of the fallacy of Dr. Carpenter's famous experiment to which he alludes.

Had the present state of my health permitted, I should have entered somewhat more fully into some of the above points, but in the meantime I must withdraw from any further discussion.

Edinburgh, August 27 JAMES CROLL

The Aurora of Feb. 4

THE Hon. Rawson Rawson, Governor of Barbadoes, has favoured us with the following note:—

"Memo. for his Excellency the Governor-in-Chief.

"The aurora of the 4th of February last, to which you allude, and notices of which you kindly sent me in NATURE, was seen here, and caused much concern.

"I was not myself an eye-witness, but I have descriptions of it from trustworthy persons. I was first informed of it by a servant of mine, who has the overlook of Mont Grace.* He was returning to his home near the Fort after 7 P.M., when, about a mile away from Mont Grace, his attention was arrested by what he imagined was a 'great fire'—the trees at Mont Grace and all about the yard were lighted up and clearly seen. My brother, who leased a sugar plantation in the neighbourhood of Government House, and Mr. Gordon, who owns one in that immediate neighbourhood, happened to be both in Scarborough at the time, and, seeing the 'great glare,' were both seized with the notion that their respective properties were on fire, and hastened out to them to find that the supposed fire was farther away.

"Mr. Taylor, master of the barque *Tobago*, was riding at anchor at Courland Bay,† and was a witness of this aurora. He described it to me as of 'a dark-red colour, extending half way up to the zenith, and very brilliant, its situation being about N.W. by W.' The labourers exclaimed that St. Vincent was on fire.

"This aurora lasted till half-past nine. Such a phenomenon, if not altogether unknown in this latitude, is at any rate very rare.

"Tobago." "DUGALD YEATES"

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* Mr. Yeates' property, three miles from Scarborough, Island of Tobago. (Tobago is in 11° 9′ N. lat., and 60° 12′ W. long.)

† On the opposite side of the island, i.e., the northern side.

The Solar Spectrum

I HAVE lately obtained and read "Schellen's Spectrum Analysis," translated by the Misses Lassell, and edited by Mr. Huggins, and feel at length constrained to dissent from a statement which I there find—in this the present standard work on the subject—distinctly and repeatedly made, as I have seen it made elsewhere before, a statement belief in which has tended and must always tend to deter many from prosecuting independently a most interesting study. I refer to the passage beginning "The Possibility of Observing" (p. 382) to end of paragraph, italicising the words "ordinary" in l. 7 of p. 383 'by increasing the number of prisms" three lines below, "highly dispersive power" in line 22] The italics are mine, and are intended to indicate that to which I object, not that the particular passages in which they occur are explicitly incorrect, but that they implicitly convey the incorrect notion, that the "highly dispersive power" is essential to the primary success of the observation "of the lines of the prominences in bright sunshine."

The reason of my objection will be found in the following extract from an unpublished letter dated May 3, 1869:—

"I think it will surprise you to hear that I have just seen Mr. Lockyer's three bright solar lines at several parts of the sun's circumference with the Royal Society's telescope and spectroscope *without any appliances or devices whatsoever (sic)*, and that with the greatest ease and certainty. Had I merely *looked* for them, or for anything of the kind, a twelvemonth ago, I do not see how I could have failed to see them!

"When the slit is placed parallel to the limb, the red line is vivid across a bright solar spectrum, and the line near D (there is no doubt about its position when seen in connection with the solar spectrum) which is less prominent, as also the line at or near F, are easily seen."

Also in another letter of the same date:—"Before I went into camp last December (while still rumours only of Janssen's observations were current) I resolved to try with coloured glasses. They were not received till too late; the instrument was packed up, and I was away. By the time I returned the question had passed on; but I still wished to carry out original intentions, and prepared accordingly, and was proceeding to direct the telescope this morning when I saw the red line in the undefended part of the slit, where I was focussing on the sun's limb. Of course, I saw at once that I could do without my coloured glass, which, practically, limited my field of view, and accordingly removed it, and examined various parts of the limb with no screen at all. At nearly all I could see the same three lines. At one place the red was so bright that an outsider looking in at the spectrum would certainly have carried away the impression of a coloured ribbon with a bright line of red near the end." As a matter of fact my wife had no difficulty in seeing at any rate the red line.

Now, the application of this is to be found in the fact that the spectroscope in question contained a single equilateral prism and no more.

Were there any merit in a fortuitous discovery of the kind, it would suffice for me to declare that I was in complete ignorance of the methods and appliances by which MM. Janssen and Lockyer had succeeded in doing long before that which I now found so obvious. What I do insist upon is that the visibility not of the prominence *lines* only (see also Proc. R.S., No. 113, 1869, in this connection), but of the prominences themselves does *not* require a high dispersion. I have a fair acquaintance with prominence forms; but it has been derived almost entirely from a study of them with *an open slit*, the use of which I had learnt long before, and a *single prism* spectroscope.

Of course, I do not contest that the power of extended examination depends directly upon increase of dispersive power, only that the *lower* limit is fully attained by a single 60° prism.

That high telescopic power is not essential either is proved by the fact that I have examined prominences by applying a $1\frac{3}{4}$ in. object glass to the end of the sliding tube of the spectroscope at solar-focal distance, and using the instrument on its "soirée" stand on a table—by way of experiment—with an amount of success which, in 1868, would have made some sensation.

It is obvious to remark in reply that probably the explanation of the ready visibility asserted is to be found in more favourable climatic conditions. I cannot admit it. By night, it is true, there is often a remarkable translucency; but the dusty, agitated state of the atmosphere (in May) under a tropical sun, and at an inland station, can certainly not be considered favourable for observations of this particular character. And even were such an explanation in some measure correct, clear skies are not so unknown in Europe and America (nor indeed in England) that the illumination of the atmosphere can be broadly said to "completely overpower" that which "in an ordinary instrument" is by no means overpowered in India in ordinary states of the sky.

The erroneous notion to which I refer has been promulgated again and again. It is unnecessary that I should specify the various passages—in lectures and elsewhere—and ultimately in Mr. Proctor's work on the Sun*: but having now found it so distinctly enunciated in the above cited passage, and elsewhere, in "Schellen," the time seemed to have come when it ought no longer to pass unnoticed; the more so as I have never been able to understand the real reason why the momentous discovery was not made earlier. It has been said that *knowing where to look*, the main difficulty was overcome. But two days after my first experience of these three known lines, I recognised the presence of three more hitherto unknown ones—and subsequently of a seventh. From that time to this however I have not seen any others, with the same dispersive power.

If asked how it was that, with the very same power at command, I had not myself seen them before; I need only reply that I had small leisure by day, and was under the impression that the experiment had been fruitless in more experienced hands—the identical reason which P. Secchi has given for a like remissness, in this very matter.

Bangalore, Aug. 25 J. HERSCHEL

Botanical Terminology

I VENTURE, as no one else has done so, to make a few remarks on Mr. Kitchener's letter.

I suppose the necessity will not be denied of employing some technical terms in studying subjects which do not fall under ordinary observation, and for the discussion of which ordinary language is consequently insufficient. When these technical terms are first devised, it is natural, indeed unavoidable, that they should reflect the scientific ideas current at the time. But inasmuch as knowledge progresses, we find ourselves, sooner or later in every branch of science, in the predicament of having to give effect to new views in terms which are an inheritance from old ones. We are able to do this because things themselves remain the same though our ideas about them change, and the names they once received with an intelligible meaning have now become purely arbitrary. No man bearing the name of, say, Baker, would probably change his name because he did not make bread. Nor do chemists discard the term oxygen because there are acids of which it is not a constituent. In the same way the morphological analogy implied in the use of the term "ovule," in the case of plants, is undoubtedly incorrect, but any one must have a singularly tender conscience who would object to it on that ground.

To save, therefore, confusion, and preserve uniformity in scientific literature, there is a tacit convention to treat in a great many cases as arbitrary terms words which once implied acquiescence in a theory. That a word in common use belongs to "a pre-Adamite stage of botanical knowledge," as Mr. Kitchener calls it, is not, I take it, sufficient ground for replacing it with another if there is no ambiguity in its application.

Next I would remark that Mr. Kitchener appears to me to have an exaggerated notion of the copiousness of botanical terminology. The number of terms really indispensable is not large. For example, he speaks of the troop of words ending in "tropous." Was this particular noun of multitude suggested by the termination? because as a matter of fact the troop consists of three. Prof. Henslow found no difficulty in teaching the terms contained in Prof. Oliver's Lessons in Elementary Botany to girls in a village school. Surely the Rugby boys cannot be less apt.

That Professor Henslow succeeded seems to dispose of the objection that a knowledge of Greek is "a necessary *open sesame* to the correct remembering and spelling of botanical terms." To teach these terms as "unintelligible gibberish" is only what in any case must be done with whole hosts of words not very different in form. Why should it be insuperably difficult for a boy, even if ignorant of Greek, to remember spell and apply the term hypogonous when he cannot possibly evade sometime or other having to face hypothesis, hypochondria, and hypocrisy, to say nothing of hydrostatics, hydraulics, hydrogen, and hydrocephalus?

I can see no reason why, as Prof. Henslow was in the habit of doing, technical terms carefully reduced to the smallest number absolutely required (and text-books bristle with unnecessary ones) should not be taught to boys as mere arbitrary names. Syngenesious, as a mere matter of taste, seems to me preferable to "united by dust-pouches."

If this be done, Mr. Kitchener's further difficulty as to "gamogenetic analogy" disappears.

The teacher, of course, may himself reasonably exercise some liberty. Thus no one would, I suppose, object to quincuncial being expressed by $\frac{2}{5}$, though quincunx is to be found in any dictionary, and is a word for which botanists are not responsible. Again, the suggestion to express by a fraction the depth of leaf-incision is really commendable, even to technical descriptive botanists.

October 1 W. T. THISELTON DYER

The Hassler Expedition

UNDER this heading in your number for August 29, p. 354, is this sentence, "One lesson I must confess to having learned at Indefatigable Island (Galapagos). I saw there indisputable proof that the surf of the sea is capable of rounding angular fragments of lava into pebbles somewhat resembling in shape (but not at all in polish and grooving) glacial boulders. I had always from boyhood doubted the power of the sea to make angular fragments round. I had supposed that the action of the surf upon such fragments would be simply to pack them into a sort of McAdam's roadway. And even now, having had the proof that under peculiar circumstances the sea can make a tolerable imitation of drift, I am not a whit more ready to believe that the sea made the drift itself. You may prove to me experimentally that flour can be made from wheat with a pestle and mortar, but that will not convince me that the flour markets of the world are thus supplied."

If the countless myriads of tons of beach on the shores of this globe could be passed through the hands of this writer, he would not detect a single "angular fragment" (McAdamised) among them. On the shore each lump of rock is successively worn into a boulder, each boulder into a pebble, and finally each pebble into sand. This is the main source of the sand which lies between the beach and the ooze-bed of the ocean.

But the sea-shore factory of boulders and drift is not the only factory, or even the largest factory of boulders and drift. The rocky gullet is the main boulder factory. Lyell (Principles), speaking of Etna, attributes "the enormous rounded boulders of felspar, porphyry, and basalt, a line of which can be traced from the sea from near Giardini, by Mascali and Zapharana to the Val del Bove" to one flood of melted snow. The valleys of the low part of Teneriffe, away from the Peak and near Santa Cruz, are almost all dry except in rain. The beds of the upper parts of these valleys are sheer rock, the middle parts wear the appearance of torrents of boulders, the lower parts are *alluvial plains* of boulders, and opposite the mouths of these valleys are very commonly deltas and bars of boulders. Behind these bars, after each rain, large deposits of earth and sand are formed which the people collect diligently. Where permanent streams exist, they are usually lost at a considerable distance above the mouths of the valleys. That is, except in rains, they percolate to the sea beneath the plains deltas and bars of boulders.

From the sides, hundreds or thousands of torrents of boulders fall into these rivers of boulders. Sometimes these lateral shoots have formed barriers of boulders across the main valley behind which large beds of boulders and earth have accumulated, again to be cleared out and thrust down to the sea-shore by heavy longitudinal rain floods.

So, in Madeira, who does not know the sea-shore boulders of the Praya-Formosa? and for fresh water boulders, the stream at Funchal brings down such a crop at every flood as to choke the channel through its delta of boulders, and unless the channel is kept clear of them artificially, the lower town is subject to the most disastrous inundations.

I mention Teneriffe and Madeira because, like the Galapagos, they are deep-sea volcanic islands. Their surfaces have been ejected when they were already above the sea, and they have been coated and re-coated thousands of times by floods of melted rock when they had long been *sub dio*. So that I conclude that even Agassiz would not attribute the moulding of their surface to the "Glacial epoch." But leave volcanic islands or volcanic mountains out of the question, there is not a mountain stream or streamlet in the wide wide world which is not at this moment a boulder-factory. Take Europe; in Scotland, Switzerland, and Norway you may see the whole of the hill-side streaked with streams of boulders. They are hurled into Romsdal now by every cascading river and rivulet or dry gullet which scores its magnificent mountain sides when flooded by rain or its equivalent melted snow. Every cascade of water above forms a cascade of boulders below ending in a somewhat vertical triangle or delta of boulder talus. That is, these triangles or deltas of boulders are horizontal where there is room, while they approach the vertical directly as the narrowness of the valley and the consequent steepness of its sides.

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* See particularly p. 286, footnote.

Distinct from the clays resulting from atmospheric disintegration, this inland grinding of rock into boulders and pebbles is the main source of the sand which is found mingled with boulders in the parallel terraces into which raised *marine* alluvial plains are cut, and of the *inland* parallel terraces on the opposite sides of each soft valley above each hard gorge. It is the source of the sand of the Scottish Kames and of the Irish Eskers. The so-called northern drift and glacial drift are the combined result of atmospheric decomposition and marine and inland grinding of rock, sized and sorted by water.

This is all going on now, as it ever has gone on *qualis ab incepto* and according to the *fortuna locorum*. That is according to the circumstances of the *place*, not the *period*. And nothing can be more absurd than the expression a "drift period" or a "boulder period" or a "pluvial period" or a "diluvial period" or a "gravel period" or a "period of invertebrates" or an "age of reptiles," or other mistakes between place and period.

George Greenwood

Brookwood Park, Alresford, Sept. 14, 1872

P.S.—Since this was written, I have had the honour to receive from the Smithsonian Institution the Report of the Survey of Wyoming, by Mr. Hayden, United States Geologist.

As far as I have read, he appears to attribute the moulding of the earth's surface, after upheaval, not to glacial but to atmospheric agency and the erosion of rivers. With regard to the inland grinding of rock into boulders, pebbles, and sand, he describes, page 14, the "worn masses of iron ore" "in the bed of the Chugwater," and ends thus: "thousands of tons have been washed down to the valley of the Chug and distributed among the superficial drift. As we leave the ore beds themselves, these strong masses are larger and more angular, and as we pass down the Chug they dwindle to minute pebbles and disappear."

An Entomological Query

I find the subjoined note in the recently-published "3^{e} Livraison of Fauvel's Faune Gallo-rhénane; Coléoptères, p. 11. Will some entomologist kindly say whether Fauvel's observation has been since verified or not? If correct, it is one of the most curious of the many curious phenomena connected with beetle-life in formicaries.

"J'ai remarqué ailleurs (Bull. Soc. Linn. Norm. 1861, v. 252), que, sur un assez grand nombre d'individus capturés dans les fourmillières, il ne se trouvait pas un seul ♂. J'engage les entomologistes à vérifier ce fait, si l'occasion s'en présente. Il peut avoir de l'importance au point de vue des mœurs peu connues de nos espèces myrmécophiles."

The note has special reference to *Micropeplus staphylinoides*.

W. W. Spicer

Itchen Abbas, Alresford, September 28

Cats' Teeth

Doubtless the case mentioned by Mr. Lydekker is somewhat unusual; but the mere fact of an animal possessing an extra tooth can hardly upset Prof. Owen's theory. It is by no means an uncommon thing to meet with examples of supernumerary teeth in man, and these rarely disturb the arrangement of the others, (mostly occurring on the palatal or lingual sides of normal teeth), I do not therefore see why (judging from analogy) it should be very unusual for the lower animals to possess like peculiarities, although they are not often met with on account of the limited number of skulls examined. Mr. L. does not tell us whether the extra tooth occurs in the superior or inferior maxilla.

4, Finsbury Square W. G. Ranger

PHOSPHORESCENCE IN FISH

While off the Land's End, Cornwall, or between the "Wolf Rock" and "Longships" Lighthouses, in the screw steam-ship *Cumbrae* (*ex* Plymouth for Belfast and Glasgow), on the night of Thursday, August 27, my attention was directed to one of the most beautiful marine phenomena that could well be imagined. At some distance ahead of the vessel the sea appeared quite luminous over large portions of its surface. This luminosity, observed at intervals, on a nearer approach proved to be nothing more nor less than the phosphorescence of immense shoals of fish—mackerel or pilchards, probably both—which could be distinctly seen near the surface; they of course appeared somewhat large, owing to the light which they emitted.

It was a dark, rough night, a strong breeze blowing off the Atlantic at the time; and as a consequence, the vessel, as may be supposed from her description, was lurching and pitching considerably—in fact more so than I had ever experienced in any other vessel, or on any previous occasion.

Having taken up a position on the forecastle, and secured myself by a tight hold on the stanchions immediately over the bow, I watched these fish with intense interest—so much so that at times I could scarcely restrain myself from a loud burst of laughter, so exceedingly interesting were their movements. As the vessel rolled and dipped, these fish, evidently startled by her movements, could be seen near the surface, ahead and on the starboard and port bow, darting forward in quantities as close as I should think it was possible they could well swim together. It was a sight long to be remembered.

I may add, that as the sea broke over the fore part of the vessel, the spray rested on me in drops or globules of, as it were, fatty matter, and much resembled in its luminosity, which lasted for some time, the appearance of so many glow-worms; doubtless this was given off by the fish themselves.

Some interesting particulars of the nature of phosphorescence in fish appeared in Nature (Notes) vol. iv. p. 287 (Aug. 10, 1871), as presented in a memoir to the Association of Naturalists and Physicians at Turin, by Sig. Panceri of Naples, from which I extract the following:—

"The phosphorescent substance in fishes, in whatever part of the body it may be situated, is always fat" (this bears out my former remarks) "and the phenomenon is due to its slow oxidation in contact with air."

Further particulars appeared in Nature, vol. v. p. 132 (Notes), December 14, 1871, as derived from the same author, of which the following is also an extract:—

"In all cases the phosphorescence is due to matter cast off by the animal—it is a property of dead separated matter, not of the living tissues.

"He" (Sig. Panceri) "also finds that this matter is secreted by *glands*, possibly special for this purpose, but more probably the phosphorescence is a secondary property of the secretion. Further, the secretion contains epithelial cells in a state of fatty degeneration, and it is these fatty cells and the fat which they give rise to which are phosphorescent. It is due to the formation in decomposition of a phosphoric hydro-carbon, or possibly of phosphuretted hydrogen itself."

Are there any special conditions of weather, or season, during which this phenomenon of phosphorescence is more readily observed than at others? Although by no means a stranger to the sea, I have never, on any occasion, seen anything approaching to it.

I made a trip from Plymouth to the Eddystone Lighthouse and back on the previous night, and although nets were out (as known by their floats) at some distance from land for the purpose of securing the fish that I have mentioned, no phosphorescence of the kind was to be seen; the sea on this occasion was comparatively smooth.

John James Hall

ON THE RETENTION AND COLOURING OF EGGS, AND THE PROTECTIVE MIMICRY OF SOUNDS

1. IS it known for how long a time a bird possesses the power of retaining its egg?

Last summer, from the number of nests in this neighbourhood, the writer was able to study the habits of kingfishers (*Halcyon vagans*) with more facility than usual. The movements of one pair excited much interest. On the 19th October this pair were observed to be busily engaged in excavating a home in the back of the turf chimney of an empty cottage. After many days spent in hard labour, this was abandoned; subsequently several tunnels were commenced, in some of them considerable progress was made; then they were in like manner deserted. The seventh resting-place, begun November 26 (there must still be a witchery about number seven even at your Antipodes), was finished, occupied, and therein, on December 24, a brood was hatched. Can there be reason to doubt that the eggs in the ovary of the female must have been in a forward state in the third week in October? At the close of that month the first egg to be laid must have been ready for extrusion. From personal observation we know that our kingfisher lays nearly every morning till the clutch of eggs is completed; the number of eggs to a clutch varying from five to seven. Here we have a bird engaged in laborious, almost incessant exertion, for quite six weeks, physically in a condition analogous to that of a pregnant animal. Three of the homes excavated and abandoned were so far finished that the chamber was hollowed out, so that a deposit of eggs must have been imminent on three occasions during that period of six weeks. It is well known that the domestic fowl, on a change of quarters, will, in its strange home, sometimes retain the egg for hours beyond the usual time of laying, often depositing what is called a double-yoked egg, but we have to do with the freedom of wild nature. It is easy to suggest that our kingfisher relieved itself by dropping its egg; obviously that would be opposed to the marked instinct of so persevering and painstaking a nest-builder; besides, would that mode of acquiring ease be twice repeated by a bird that endured such toil to make a hiding-place for its progeny—toil only to be appreciated by those who have watched its daily work?

2. Can a bird influence the colour of its eggs protectively?

A proposition that few physiologists would answer in the affirmative, yet naturalists have held, perhaps still do maintain, diverse opinions as to the cause of abnormally coloured eggs. The following facts are laid before your readers for information:—Rather late in last summer a female bittern (*B. poicilopterus*) was slightly wounded and secured. It was kept within a *grassy* enclosure. While thus confined it laid an egg of a pale bluish green colour, precisely like that of a heron. The egg of our bittern is about the same size; its normal colour of a similar olivaceous buff as that of your *B. stellaris*. This buffy olivaceous tint harmonises well with the half-faded leaves of aquatic plants of which the nest is often built, such as those of *Typha angustifolia*, *Carex virgata*, &c.; in fact, a bittern's nest is by no means an offensively obtrusive object.

Having had eggs from several nests under observation, I have noticed that bittern's eggs do now and then vary in tint from buffy brown to pale olivaceous; but in no case approximately to that blue green of the heron's egg.

In the instance cited, was the peculiar colouring used as a means of securing for the egg the protection of the *verdure of the grass* in which it was deposited? or was it merely the effects of a brief confinement and a slight local wound? The conditions under which this egg was laid may be considered as somewhat analogous to those under which the cuckoo laid No. 26 specimen in the famous series of eggs formed by Herr Baldamus (see vol. f.p. 508); nor is the occurrence of this peculiar-looking bittern's egg without its use in estimating the value to be accorded to certain abnormally coloured eggs as illustrating and supporting a theory not adverse to the proposition—Can a bird influence the colour of its eggs protectively?

3. Are the eggs of the cuckoo ever approximately coloured like those of its dupe for protective purposes?

In vol. v. p. 501, may be found a brief note, stating that the eggs of our whistler, or small cuckoo, were not coloured approximately to those of its dupe, nor, indeed, would such precaution appear necessary, when the form of the nest of its victim was considered. Last season one of the writer's children brought in a nest of the blight bird (*Zosterops lateralis*) containing four eggs, one of which was a puzzle indeed; it was found on comparison, that although a shade darker in colour, it resembled the rest of the eggs in the nest, pale green-blue, spotless and unstained as those of the homely russet-clad hedge-sparrow. In size and shape it was like that of the small cuckoo. Hundreds of eggs of the *Zosterops* (a new colonist, yet already one of our commonest birds) have passed under the notice of the writer, but none have resembled the specimen alluded to. That it might possibly have been laid by a whistler seemed at length the only solution of the problem, how an egg of that size and shape came into that nest. The *Zosterops* does not belong to the purely indigenous genera of New Zealand; like the *Chrysococcyx* and its usual dupe, the *Gerygone*, it is to be found in other colonies far beyond the bounds of ferny Maori-land. It builds a suspended nest, another indication of its foreign origin. It is quite likely that in warmer climes the small cuckoo may readily avail itself of the advantages presented by this mode of construction, as ensuring a greater degree of safety from reptilian egg-robbers. The open cup-shape of the *Zosterops'* nest would disclose to its owner the marked contrast between its own clear blue-green eggs and the large, greenish-dun egg of the parasite; hence the effort at protective mimicry. This would be unnecessary, as before pointed out, in the dimness of the domed structure of the *Gerygone*. If the egg described is that of the small cuckoo, it is the first instance known to the writer of the *Zosterops* being used as a dupe. It should be noticed that last season the small cuckoo appeared in greater numbers than usual in this neighbourhood, where the nests of the blight bird, in the aggregate, now outnumber those of every other species of bird.

These facts are communicated under the impression that they may be of interest to ornithologists, and in fairness should not be withheld, the rather as the writer does not yet give in his adhesion to the theories of Herr Baldamus.

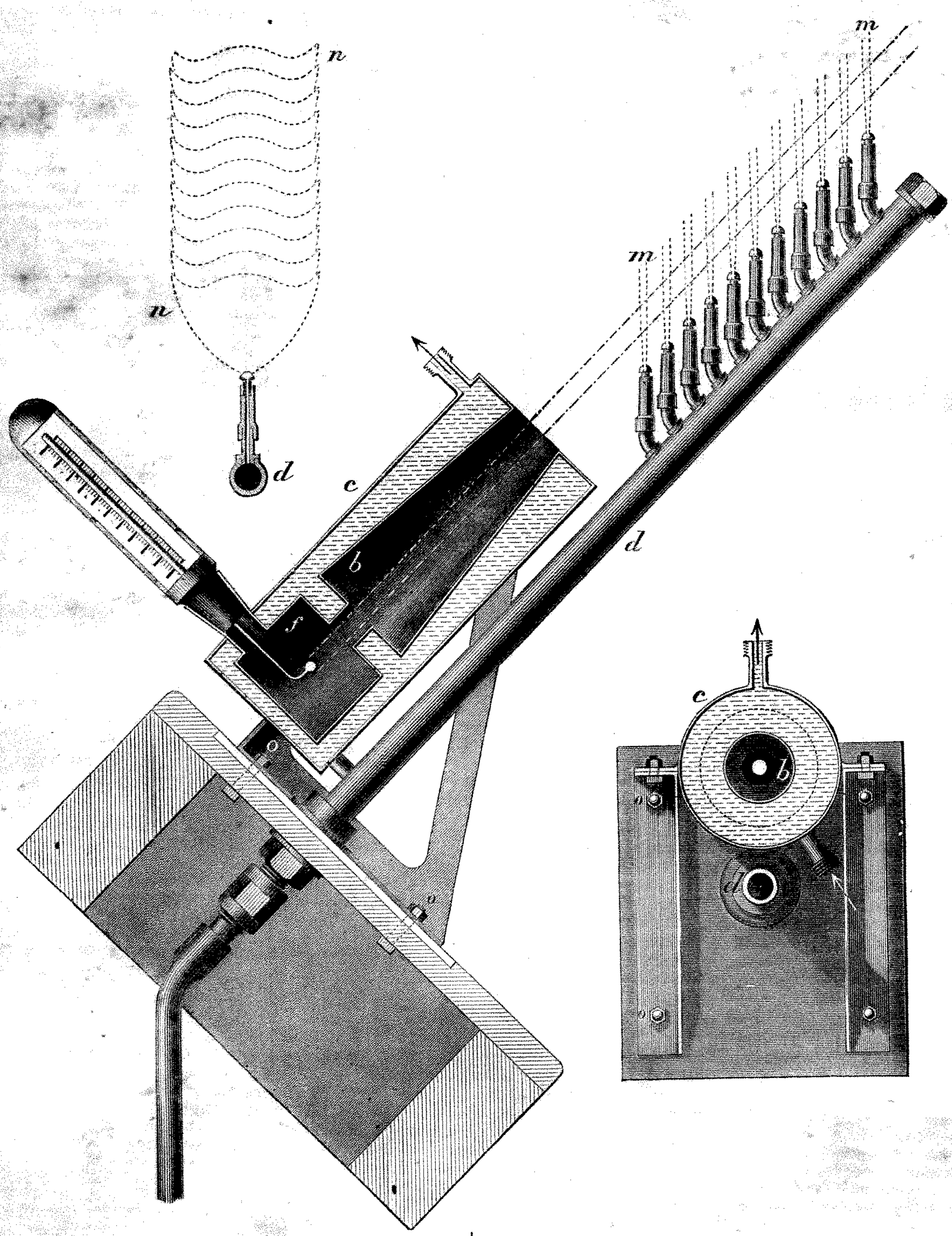
4. On the Mimicry of Sounds.

When camping for some days on a river-bed, where many species of birds abounded, the writer and one of his sons (well acquainted with bird voices) frequently heard what they took to be one of the notes of the *Hæmatopus*, but that wader was nowhere to be seen; at length we traced the call to the Piopio (*Keropia crassirostris*) a bird with feeble powers of flight, yet one that delights in the open glades of river-beds. The mimic cry was always given when near to a stream, just where the red-bill (*Hæmatopus*) would be likely to be found. A pair of red-bills can drive away a hawk; now a hawk, "from his place on high," perceiving something near the water, might forego its swoop on hearing the mimicked note of the wary, yet bold red-bill. We have observed our grey warbler give an exact imitation of the cry of our common tern (*S. antarctica*) one of the boldest birds in defence of its young. THOMAS H. POTTS

Ohinitahi, June 29

THE SUN'S RADIANT HEAT

THE readers of the *Comptes Rendus* are aware that Père Secchi addressed a letter to the Academy of Sciences at Paris, some time ago (Comp. Rend. tome lxxiv. pp. 26–30), containing a review of my communications to NATURE, published July 13, October 5, and November 16, 1871, in which he questions the correctness of the reports which I have published containing tabulated statements of the temperature produced by solar radiation. His reason for questioning the reliability of my tables, appears to rest on the supposition that my instruments do not furnish correct indications. "It is astonishing," he says, "that Mr. Ericsson should find with his instrument a higher stationary temperature in winter than in summer. This (even bearing in mind the greater proximity of the sun in winter) makes me think that there must be something very singular in his apparatus, possibly making all its indications deceptive. Even under the beautiful sky of Madrid, M. Rico y Sinobas found, in



December, for the solar radiation, 12^{div}, by his actinometer, and, in June 25^{div}, 56″." It is not my purpose to prove the fallacy of M. y Sinobas' actinometric observations; but I think "that, there must be something very singular in his apparatus," since in North America in lat. 40° 42′ (the latitude of Madrid is 40° 24′), solar intensity at noon during the latter part of June is 64°·5; while when the sky is clear at noon during the latter part of December, the temperature under similar atmospheric conditions, reaches 58°·7. But observations made in the morning or evening during the month of June at the hour when the sun's altitude is the same as at *noon* in December, show that the intensity of the radiant heat in June is only 53°·08 against 58°·7 in December. Actual observations have thus established the fact that for *corresponding zenith distance*, the temperature produced by the radiant heat when the earth has nearly reached perihelion, is 58·7—53·8=4°·9 *higher* than at midsummer. Referring to the table published in NATURE, vol. v. p. 47, it will be seen that, owing to the greater proximity of the sun, the increase of absolute intensity of solar radiation is 5°·88 Fah. during the winter solstice. Père Secchi will do well to examine the subject more carefully, and make himself better acquainted with the character of the investigations which had led to an exact determination of the temperature produced by solar radiation.

The readers of the *Comptes Rendus* who have examined the review referred to, ignorant of the contents of the articles in NATURE, will be surprised to learn that I have not, as the reviewer asserts, questioned the power of vapour to diminish solar intensity. Having stated the result of numerous observations of the sun's radiant power at corresponding zenith distance, and proved that the temperature during midwinter is higher than at midsummer, I made the following remark in NATURE, Nov. 16, 1871: "In the face of such facts it is idle to contend that the temperature produced by solar radiation under corresponding zenith distance and a *clear sky* varies from any other cause than the varying distance between the sun and the earth." It is absurd to suppose that a person having devoted many years to the investigation of solar radiation should deny the retarding influence of vapour, since not one observation in a hundred indicates maximum solar intensity, owing to the presence of vapour in the atmosphere.

The following brief description of the actinometer will show that there is not, as Père Secchi supposes, anything very singular in this apparatus tending to render its indications deceptive. The principal part of the instrument consists of an air-tight cylindrical vessel, the axis of which is directed towards the sun, the upper end being provided with a thin lens covering an aperture of 3in. diameter. The bulb and part of the stem of a mercurial thermometer is inserted through the upper side, at right angles to the axis; a small air pump being employed for exhausting the air from the cylindrical vessel. The latter is surrounded by a casing through which water is circulated by means of an ordinary force pump and flexible tubes, connected with a capacious cistern containing water kept at a constant temperature of 60° Fah. The bulb of the thermometer is cylindrical, 3 ins. long, its contents bearing a very small proportion to its convex area. The upper half is coated with lamp black, while the lower half of the bulb is effectually protected against loss of heat from undue radiation. The diminution of energy attending the passage of the sun's rays through the lens is made good by the concentration effected by its curvature; hence the entire energy of the radiant heat will be transmitted to the cylindrical bulb. The inclination of the latter, it should be observed, promotes a rapid upward current of the mercury on the top side, and a corresponding downward current on the lower side, thereby rendering the indication prompt and trustworthy. The water in the surrounding casing being maintained at a constant temperature of 60° Fah., it will be evident that the *zero* of the thermometric scale of the actinometer must correspond with the line which marks sixty degrees on the Fahrenheit scale. It scarcely needs explanation that the height reached by the mercurial column after turning the instrument towards the sun, will be due wholly to solar energy, since the radiation of the exhausted vessel towards the bulb of the thermometer is only capable of raising the column to the actinometric zero (60° Fah.)

The readers of NATURE will remember that one of the articles reviewed by Père Secchi, the one published in vol. v. pp. 449-452, contained a demonstration accompanied by several diagrams, proving that the radiant heat emitted by the chromosphere and outward strata of the solar envelope is inappreciable at the surface of the earth. It will be remembered also that the mode adopted in deciding the question whether the solar atmosphere is capable of emitting heat rays of appreciable energy, was that of shutting out the rays from the photosphere, and collecting those from the chromosphere and envelope in the focus of a parabolic reflector. Scarcely any heat being produced notwithstanding the great concentration by the reflector, we proved the fallacy of Père Secchi's remarkable assumption that the high temperature at the surface of the photosphere is caused by *radiation* "received from all the transparent strata of the solar envelope." It is surprising that notwithstanding the completeness and positive nature of the demonstration referred to, no allusion whatever is made to the same in the review put forth as a careful examination of the contents of the article under consideration. Ignoring the evidence furnished by actual trial, in proof of the extreme feebleness of the radiating power, the reviewer proceeds to state "that the outward strata might be less hot, and that the effect which we measure is the aggregate of the quantities of heat which are added, emanating from the various transparent strata." How the outward colder strata cause an elevation of temperature by their radiation towards the solar surface, is not explained; but reference is made to the result of an experiment with three small flames, in support of the assertion that the high temperature of 10,000,000° C., assigned to the surface of the sun, is owing to radiation received from all the transparent strata surrounding the photosphere. The reviewer states: "a very simple experiment, made at my request by P. Provenzali, has shown that, if a heating of 2°·5 can be obtained with one flame, with two flames placed one before the other 4°·5 are obtained, with three flames 5°·4; a result, which of course could be easily foreseen, for everybody knows that flames are transparent."

My practical demonstration establishing the feebleness of the radiating power of the matter composing the solar envelope having received no consideration, while the reviewer, in support of his singular theory of solar temperature, points to the result of the rude experiment conducted by Père Provenzali, I have deemed it necessary to show that transparency of flames is too imperfect to warrant the inferences drawn.

The accompanying illustration represents an apparatus by means of which the exact degree of transparency of a series of flames has been ascertained.

Description: *b* conical vessel open at the top, the bottom communicating with a cylindrical chamber, *f*, by an intervening narrow passage, the whole being enclosed in an exterior vessel *c* charged with water kept at a constant temperature, precisely as in the actinometer. A thermometer is applied near the bottom of the cylindrical chamber, the centre of the bulb coinciding with the prolongation of the axis of the conical vessel. A gas pipe, *d*, provided with a series of vertical burners, is firmly secured to a table, in a position parallel to the axis of the conical vessel. The burners are provided with caps in order to admit of any desirable number of jets being ignited at one time. When gas of ordinary pressure is admitted into the pipe *d*, the side view of the flames will be as indicated by the dotted lines at *m*, *m*, the thickness of each flame being nearly 0·20 ins, while the width, shown by the dotted lines *n*, *n*, somewhat exceeds 3 inches from point to point. It will be observed that the prolongation of the axis of the conical vessel upwards passes through the central portion of the flames at the point of maximum thickness and intensity. Supposing that the instrument (attached to a table turning on declination axis within a revolving observatory) is directed towards the sun, it will be evident that all the rays of a beam the section of which corresponds with that of the bulb of the thermometer, will pass through the flames before reaching the said bulb. Now the temperature of the flames at the point pierced by the solar rays, is fully 2000°. Fah., while the intensity of the rays does not exceed 60°. The illustrated device enables us to ascertain whether the rays thus entering at a temperature 1,940° lower than that of the incandescent gas, have their intensity augmented or diminished during the passage through the heated medium. But before we can determine this question, it will be necessary to ascertain what temperature is communicated to the thermometer by the radiant energy of the flames, independent of solar heat. Accordingly, a series of experiments have been made, the result of which is recorded in the annexed table. The nature of the investigation will be readily understood from the following explanation. The instrument being turned away from the sun and the upper flame *m* ignited, while the external casing *c* is kept at a constant temperature of 60°, the column of the thermometer at *f* slowly rises to 61°·76. The radiant heat, therefore, of a single flame produces a differential temperature of 61·76 − 60 = 1°·76. The second flame being ignited, the temperature rises to 62°·88, thus increasing the differential temperature to 2°·88. The ignition of the third flame augments the differential temperature to 3°·80. The remaining flames being ignited in regular order downwards, their combined radiant energy elevates the temperature to 67°·90. Deducting the temperature of the enclosure *c* (60°), the trial shows that although the single flame at the maximum distance from the bulb, is capable of producing a differential temperature of 1°·76, the energy of the *ten* flames together produces only 7°·90. This fact furnishes conclusive evidence of the imperfect transparency of the flames. Assuming that the heat rays are capable of passing freely through the incandescent medium, it will be perceived that the entire series of flames should produce a differential temperatue of 1·76×10= 17°·60, showing a retardation of 17·6−7·9=9°·7. And if we take into account the diminished distance of the lower flames from the bulb of the thermometer, it will be found that the actual retardation greatly exceeds this computation. We have thus demonstrated that flames are most transparent, as supposed by Père Secchi. Consequently, the inferences drawn from the experiment to which the distinguished *savan* refers in his letter to the French Academy of Sciences are wholly unwarrantable.

Having disposed of the question of transparency, and ascertained the degree of temperature communicated to the thermometer by the radiant energy of the flames alone, let us now suppose that the instrument has been turned towards the sun. The temperature produced by the combined energy of solar radiation, and the radiation of the flames, after directing the instrument towards the luminary, will be found recorded in the fifth column of the table. Our space not admitting of a detailed statement, we must dispense with an examination of the energy transmitted for each flame separately, and at once consider the effect produced by passing the sun's rays through the entire series. It has already been stated that the radiation of all the flames combined imparts a differential temperature of 7°·90 to the thermometer. By reference to the table it will be seen that, the temperature produced by the sun's rays is 21°·69, when the flames are extinguished.* Consequently the temperature, after lighting the whole series, ought to be 21·69+7·90=29°·59, since solar heat, under analogous conditions, is capable of increasing definitely the temperature of substances whatever be their previous intensity.† Referring again to the table, it will be found that the maximum increase of temperature attending the passage of the comparatively cold solar rays through the incandescent gas, is 2°·31, while the radiant energy of the flames produces a differential temperature of 7°·90. This extraordinary discrepancy points to an increase of molecular energy within the incandescent gas, notwithstanding its temperature being fully 1,900° higher than that produced by the sun's radiant heat.

J. ERICSSON

The Instrument turned from the Sun.			The Instrument directed towards the Sun.		
Number of flame from the top.	Distance of flame from bulb.	Temperature produced by radiation of the flames.	The sun's rays acting directly on the bulb.	The sun's rays passing through the flames	Increment of temperature attending the passage of the solar rays through the flames.
	Inches.	Deg Fah.	Deg. Fah.	Deg. F h.	Deg. Fah.
1	24·8	1·76	21·60	21·90	0·30
2	23 8	2·88	21·61	22·20	0·59
3	22 8	3·80	21·62	22·49	0·87
4	21·8	4·58	21·63	22·75	1·12
5	20·8	5·24	21·64	22·99	1·35
6	19·8	5·84	21·65	23·22	1·57
7	18·8	6·38	21·66	23·43	1·77
8	17·8	6·91	21·67	23·63	1·96
9	16·8	7·42	21·68	23·82	2·14
10	15·8	7·90	21·69	24·00	2·31

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* Notwithstanding the low temperature indicated by the thermometer of the experimental apparatus, 21°·69, the actual solar intensity during the investigation, ascertained by the actinometer, has at no time been less than 50°, a very instructive fact proving the futility of attempting to measure solar intensity by thermometers the bulbs of which are exposed to the refrigerative action of surrounding air. For the purpose in view, however, that of measuring the comparative radiant power of flames and solar heat, the unavoidable exposure of the bulb to atmospheric influence, is unimportant, provided the enclosure be kept at a constant temperature during the experiments.

† Père Secchi reminds us, in "Le Soleil," that Mr Waterston found by his solar intensity apparatus, that, when the thermometer was enclosed in a heated vessel imparting upwards of 400° Fah. to the bulb, the same degree of differential temperature was reached by exposure to the sun, as when a cold enclosure was employed which reduced the indication of the enclosed thermometer to that of ordinary atmospheric temperature.

NOTES

MR. DARWIN's forthcoming work on "Expression in Man and Animals" bids fair to be of a more popular character than any of his other publications. It will commence with a statement of the general principle of Expression;—that serviceable actions become habitual in association with certain states of the mind, and are performed, whether or not of service, in each particular case. This will be illustrated in the case of expression of the various emotions in man and the lower animals. The means of expression in animals will then be discussed, and the special expressions of animals and man, such as the depression of the corners of the mouth in grief, frowning, the firm closure of the mouth to express determination, gestures of contempt, the dilatation of the pupils from terror, the causes of blushing, &c. In conclusion, the bearing of the subject will be spoken of on the specific unity of the races of man, the part will be discussed which the will and intention have played in the acquirement of various expressions, and the question of their acquisition by the progenitors of man will be referred to. Seven heliotype plates reproduced from photographs will illustrate the work.

HISTORY is said to repeat itself. It is singular to find that six months ago the colony of Victoria was involved in a similar controversy to that which has recently agitated the scientific world at home. Baron Friedrich von Mueller is the Dr. Hooker of Australia. The Botanic Gardens at Melbourne have become under his management as truly scientific an institution as those at Kew, and their Director has performed similar eminent services both to the colony and to the mother country in spreading a knowledge of the value of the indigenous vegetable products of Australia. After twenty years' service, however, it is discovered that von Mueller is "not a landscape gardener," and an agitation is set on foot—we do not, however, hear that it originates in the Board of Works, if there is such a department in Victoria, nor do we know who is the Ayrton of the Antipodes—to deprive him virtually of the control of the Botanic Gardens. Next to the removal of Dr. Hooker from Kew, botanical science all over the world could receive no severer blow than the deposition of von Mueller from the position he occupies at Melbourne.

WE believe that, in addition to the 4,000*l*. which we recently noted, another 8,000*l*. will be voted in the next French Budget, to be devoted to the preparations necessary for the observation of the forthcoming Transit of Venus.

THE Californian Academy of Sciences experienced a genuine sensation at its meeting on September 9, in welcoming Prof. Agassiz returning from the *Hassler* Expedition, on his first arrival on United States' territory, where he was received by Prof. Davidson, the president of the Academy; Prof. Gillman, the principal of the University of California, Prof. Torrey, of Columbia, the Nestor of American Botanists, and others. In his address on the occasion, Agassiz alluded in the following terms to the growth beneath his eyes of the great Cambridge Museum:—"I went single-handed to Cambridge, to teach natural history, twenty-five years ago. When I delivered my first lecture there was not in the University a single specimen which I could use to illustrate what I had to say. And yet a little band of students, feeling an interest in what they could learn in the lecture-room, and others, thought such a pursuit was worth encouraging, and by-and-by the idea arose that a museum would be of use, and the means were gradually forthcoming, at first sparingly in small contributions, but gradually more liberally in larger sums, until at this moment, after fourteen years only, the museum at Cambridge stands in my estimation without parallel in the world." This he followed up by a statement of what could be done in a new country like California for the advancement of science:—"What would you think of the man who would raise himself his food when he is engaged in the law business or in the medical profession? You would think he was wasting his time. Now I say the scientific man is wasting his time or is obliged to waste his time when he is not provided with the appliances with which he can work, and which he is capable of producing. And I hold that it is one of the duties of those who have the means to help those who have only their head, and who go to work with an empty pocket. So I think that one of your duties, besides fostering and nursing the interest you individually feel for science, is to arouse that general interest in the community, which will make every true patriot, every lover of his State, every philanthropist, every man who has the heart to leave a good repute and an honourable memory, desirous of contributing to your progress." We know there are men in California who both can and will respond to this generous appeal.

PROF. TYNDALL left England on Saturday last by the *Russia* from Liverpool, on a visit to the United States.

THE Council of the Bedford (Ladies') College, Bedford Square, have decided on attempting to introduce some branch of Natural Science into the regular programme of the college studies, and will make a commencement with a class of Vegetable Physiology Mr. A. W. Bennett, who will conduct the class, will also give the Introductory Lecture of the session on Wednesday, October 9th, on "The Place of Natural Science in a Liberal Education."

A VACANCY has occurred in the staff of the Royal Observatory, Greenwich, by the resignation of Mr. James Carpenter, whose services have been transformed to a private engineering firm.

FROM the second report of the Cambridge Natural Science Club we learn that seven meetings have been held during the long vacation, at which the following papers have been read:—"On Turacin," by Mr. R. M. Lewis, B.A., Downing College; "On the old Glaciers of Wales," by Mr. G. E. Paget, Caius College; "On Colour," by Mr. C. T. Whitmell, B.A, B.Sc., Trinity College; "On Sponges," by Mr. A. F. Buxton, Trinity College; "On Thermo-Magnetism," by Mr. J. E. H. Gordon, Caius College; "On *Peronospora infestans*," by Mr. H. M. Martin, M.B., D.Sc., Christ's College; "On Absorption Spectra," by Mr. R. M. Lewis, B.A., Downing College. The club consists of twelve members, of whom seven or eight were in residence during the Long. The attendance at the meetings was good, averaging six or seven members, and two or three visitors. The rule, requesting that members shall give "such practical illustration as the subject admits of," has been very well observed, as almost all the papers have been illustrated by experiments or specimens.

THE opening (public) lecture of the Literary and Scientific Society attached to the Whitechapel Foundation School, was given on Tuesday evening last. Among the lectures to be delivered during the session, will be one on the "Early History of Man," by E. Clodd, F.R.A.S., and on one of the "Divisions of Light," by W. Spottiswoode, F.R.S.

MR. R. PRATT, late Master of the Queen's School of Art, and gold medalist of the Department of Science and Art, has been elected Art-master in the Hartley Institute, Southampton; and Mr. J. R. Brittle, Associate of King's College, London, and late Whitworth Scholar, has been appointed Lecturer on Engineering at the same institution.

THE following classes in connection with the Manchester Mechanics' Institution have been commenced for the season, under the management of Mr. Robert Routledge, B.Sc.:—Applied Mechanics, Steam and the Steam Engine, Acoustics, Light and Heat, Magnetism and Electricity, Organic Chemistry, Inorganic Chemistry, and Practical Chemistry.

THE management of the Islington Youths' Institute has just issued the programme for the winter session. Amongst the various subjects taught at this Institution, those in connection with the Science and Art Department form a conspicuous feature, and six classes in Art and eight in Science are announced. Among these we may mention Drawing (Freehand, Model, and Geometrical), Building Construction, Machine Construction, Geometry (Plane, Practical, and Solid), Electricity and Magnetism, Physical Geography, Physiology, Acoustics, Light and Heat, and Inorganic Chemistry. At the examinations held in May last, out of 129 who competed for the Government prizes, only 13 failed to pass.

THE Council of the Institution of Civil Engineers have awarded the following premiums and prizes for work done during the session 1871-2:—A Telford medal and a Telford premium in books to each of the following gentlemen: Bradford Leslie, for his "Account of the Bridge over the Gorai River, on the Goalundo Extension of the Eastern Bengal Railway;" Carl Siemens, for paper on "Pneumatic Despatch Tubes: the Circuit System;" W. Bell, for paper on "The Stresses of Rigid Arches, Continuous Beams, and Curved Structures;" J. H. Latham, for description of "The Soonkêsala Canal of the Madras Irrigation and Canal Company;" G. Gordon, for paper on "The Value of Water, and its Storage and Distribution in Southern India;" A Telford premium in books to F. A. Abel, F.R.S., for paper on "Explosive Agents applied to Industrial Purposes;" and the same to Bashley Britten, for paper on "The Construction of Heavy Artillery, with reference to Economy of the Mechanical Forces Engaged." The Manby premium in books to C. Andrews, for paper on "The Somerset Dock at Malta." A Miller prize each to Oswald Brown, for paper on "Sewage Utilisation;" A. T. Atchison, for paper on "Railway Bridges of Great Span;" J. Addy, for paper on "The most suitable Materials for, and the best mode of Formation of, the Surfaces of the Streets of Large Towns;" A. E. Preston, for paper on "Wood-Working Machinery;" W. P. Orchard, for paper on "The Education of a Civil Engineer."

WE are informed of the early publication of the first number of the *Telegraphic Journal*, a Monthly Illustrated Review of Electrical Science. It will be edited by the Rev. William Higgs, M.A., sometime assistant to Sir Charles Wheatstone.

THE present autumn has been remarkable for the appearance in scattered localities all over the country of one of our rarest and most beautiful butterflies, the Camberwell Beauty, *Vanessa Antiopa*, very few British specimens of which exist in our cabinets. The *Entomologist* records the capture of upwards of 200 specimens in all parts of the country, from the Channel Islands to Aberdeen. It is very remarkable that they nearly all differ in colouring to a perceptible extent from the Continental variety, the border being creamy white instead of buff-coloured. If they are genuine natives their spasmodic appearance in this manner is very singular, and worthy of careful observation. Several other rare butterflies, especially *Argynnis Lathonia*, *Pieris Daplidice*, and *Colias Hyale*, have also been unusually abundant this season.

A PLAGUE of butterflies is a rare occurrence. A short time ago, however, the town of Florence was invaded by a prodigious quantity of these insects. All the distance of the Long'arno between the Piazza Manin and the Barriera and in all the adjacent streets the passage was almost obstructed by an extraordinary quantity of butterflies that had swarmed in such thick clouds round the gaslights that the streets were comparatively dark. Fires were immediately lighted by order of the Municipality and by private citizens, in which the butterflies burnt their wings, so that half an hour afterwards one walked upon a layer formed by the bodies of the butterflies an inch thick ! ! ! They were of a whitish colour, and some of the streets appeared as if covered with snow, at least so say the Italian papers.

DR. PATERSON, of Bridge of Allan, writing to the *Scotsman* of September 9, says that on Saturday last he captured in his garden on the flowers of *Lilium auratum* a fine specimen of the Striped Hawk Moth (*Deilephila Livornica*). Dr. Paterson believes this insect has been captured only once before in Scotland.

THE REV. M. J. BERKELEY describes in the *Gardener's Chronicle* a very remarkable instance of luminosity in fungi. It occurred in the mycelium of an unknown species growing on a trunk of spruce or larch, and was so powerful as to make a perfect blaze of white light in the track where the trunk had been dragged, and vividly illuminating everything in contact with it. It gave almost light enough to read the time on the face of a watch, and continued for three days.

THE Brighton Aquarium has lately received two pair of beautiful specimens of the Paradise or Peacock fish. This fish came first from China, and has been acclimatised by M. Carbonnier, the great pisciculturist of Paris; they are very lovely little creatures. Some of their habits are singular; thus M. Carbonnier states that "as the eggs are laid the male carries them away in his mouth, and deposits them in a nest which he builds for them. He will not allow the female to come anywhere near the nest, and if she ventures to approach he swings himself round, and drives her away."

THE Secretary of the U.S. Navy has recently received, by way of Copenhagen, a letter from Captain C. F. Hall, of the *Polaris*, written on the 24th of August, 1871, at Tossak, North Greenland, latitude 73° 21′, longitude 56° 5′ west. Although this is but a few days later than the despatch brought home by the frigate *Congress* nearly a year ago, it renews the assurance of the harmony existing on board the vessel between the members of the expedition, and the perfect satisfaction of all with the equipment and preparations for the coming winter. It is well known that no efforts were spared by the Navy department to render this expedition the most perfect and complete in its equipment of any ever sent to the North; and the success of these endeavours must, therefore, be a source of great gratification to it. Governor Elberg, of the Upernavik district, accompanied the *Polaris* as far as Disco, and brought back the despatches, which have thus been a year in their journey to Washington. Through his help Captain Hall obtained sixty strong, healthy young Esquimaux dogs, and a large supply of reindeer furs, seal-skins, &c. At Upernavik Hans Christian, well known to the readers of Kane's narrative, joined them as hunter and dog-driver, and was accompanied by his wife and three children, who with Joe, and Hannah, and their child, Captain Hall's faithful companions in previous years, made up quite a party. It will be remembered that Captain Hall met the returning Swedish expedition at Holsteinbourg, and that its commander supplied him with charts and copies of such of his notes as promised to be of service to him. Partly in consequence of the suggestions of the commander, Baron Von Otter, and of other scientific men whom he met in Greenland, Captain Hall concluded to abandon the Jones Sound route, and intended to cross Melville Bay to Cape Dudley Digges, and thence to steam directly to Smith Sound, with a view of finding a passage on the west side of the sound from Cape Isabella to Kennedy Channel. Captain Hall speaks very favourably of the steaming qualities of the *Polaris*, her passage having been perfectly satisfactory from port to port. The entire steaming time from New York to Disco was twenty-seven days, seven hours, and thirty minutes.

MR. HULL, of the Irish Geological Survey, has published a letter in the *Dublin Morning Mail* in reference to the quantity of coal available for use in the Irish coal-fields, in which it is stated that the net tonnage available in Ireland is 182,280,000 tons—in Ballycastle, County Antrim, 16,000,000; in Tyrone, 32,900,000; in Queen's County, Kilkenny, and Carlow, 77,580,000; in Tipperary, 25,000,000; in Clare, Limerick, and Cork, 20,000,000; and in Connaught (Arigua district) 10,800,000 tons.

WE are glad to observe from the eighth Report on the Melbourne Observatory that the southern half of the heavens is being observed to good purpose, though both the Board of Visitors and Mr. Ellery think that several improvements in details are much needed in order that the work may be done with anything like sactisfactoriness. The great telescope continues to give increased satisfaction, though there seem to be serious defects in the Magnet House, and a great want of clerical assistance in reducing the great number of stars observed. The number of stars observed up to the period of the report was 48,672, the number reduced being only 36,917. It appears that a large number of drawings of nebulæ and other celestial objects observed with the Great Melbourne Telescope has accumulated, and we earnestly hope that the Board's request to Parliament to supply the funds necessary to publish these may meet with a favourable reply. Naturally the Board and the Government Astronomer express regret that the Eclipse Expedition should have turned out a failure from the unfavourable weather. Still the colony deserves the greatest commendation for the gallant endeavour it made. We are glad to see that vol. iv. of the "Melbourne Astronomical Observations" is now in the press, that a General Catalogue for 1870, containing the results of all the transit work at the observatory, is in preparation, and that in January last the publication of a series of monthly meteorological observations was commenced. Moreover, photographic pictures of the moon are being taken, and promise, we are told, to be both excellent in themselves as works of art, as well as useful in aiding the scientific observations now taking place in Europe. Altogether the Report reflects the greatest credit on Mr. Ellery, the Government Astronomer, and his too limited staff.

THREE slight shocks of earthquake were felt on the evening of July 27 at Valparaiso and at Caracas.

A SLIGHT shock of earthquake was felt at Chopea in Khandeish on the evening of Friday, July 12, at about seven o'clock. The shock lasted about a minute, and appears to have been felt at the same time at Amamer, Dhurrangaon, Dhulia, and Julgaon. Its course was from west to east.

ON the 15th of April a very violent volcanic eruption took place from the volcano Merapi in Java, which had been quiet since 1863. Great destruction of lives and property occurred, many villages being totally destroyed. The outburst was entirely unexpected, and the showers of stones and ashes and the streams of lava were very destructive. At Solo and other places the showers of ashes lasted for three days, and it became so dark that the lamps had to be lit. By the last accounts some 200 dead bodies had been found on one side of the volcano.

THE BIRTH OF CHEMISTRY

I.

Introduction—Ancient Science—Origin of Chemistry—Derivation of the Name—Definitions of Chemical Science—Early Ideas relative to the Formation of the World.

THE history of a natural science resembles in many respects the history of a nation. In each instance the object is first to obtain a knowledge of causes, then to frame laws. The first are those causes which most promote the well-being of the nation, the second those causes which produce the phenomena of the Universe. In each instance we start with an absence of all law, and we may observe the slow efforts of the human mind to trace each effect to its proper cause, to group together causes, and finally to connect them by one bond. The main difference is this, that in the case of the nation man has to deal with laws which must be founded upon a just study and close observance of every phase of that particular community, influenced as it is by numberless external causes, such as race, climate, religion, habit of thought, tradition; while in the case of the science he has to evolve pre-existent laws, also by the close observance of facts, which are hidden from him by the complex mechanism of nature. M. Taine would tell us that the laws which influence the development of peoples are just as absolute, definite, and pre-existent, as those which govern the affairs of nature; but we are quite disinclined to admit this, even in regard to one particular race, in one particular locality. In both histories we have similar forms of government, similar assemblies of lawgivers; we have our aristocracies, oligarchies, democracies, republics: we have at some period or other Conservatives and Liberals of every shade. We know not what Conservative rule can compare with the dominance of the science of Aristotle for twenty centuries, and we cannot be too ready to welcome the Liberal-conservative era of Copernicus and Giordano Bruno, the Liberal era commenced by Galileo and Francis Bacon, which by easy stages is passing, if it has not passed, into the right Radical era of modern scientific thought. The "Republic of Learning" is no empty phrase.

No one would venture to deny the value of a knowledge of the history of nations, and we are inclined to believe that the history of the natural sciences is not without its uses. It is neglected because during the last century new discoveries have quickly succeeded each other, old sciences have augmented, while new sciences have arisen; in fact, the progress of science has been so extraordinarily rapid that we have scarcely time to turn aside and look at its past history; the present is sufficient for us, and if we once get out of the main current of thought we have difficulty in regaining lost ground. Yet we may no more forget that we owe our present wise laws and great constitutional system to the labours of ten centuries of men, than that our science of to-day represents the accumulation of the scientific thought of twice ten centuries. Intellectual revolutions have not been less frequent than social revolutions, nor battles of the pen than battles of the sword; the crash of a fallen philosophy has often been louder than that of a fallen throne; the wail of the last Phlogistians rent the heavens; the Aristotelian physics died with groanings and gaspings and a discoloured visage.

In tracing the history of a science, we are first led to inquire whether the Ancients possessed any knowledge of it, and whether it originated among them. Now the Ancients made but little progress in any of the natural sciences. They divided all human knowledge into three parts: Logic, or mental philosophy; Physics, or natural philosophy; Ethics, or moral philosophy. Some placed logic first, some ethics, but no one physics. Philosophy was compared to an egg—logic the shell, physics the white, ethics the yolk; or, again, it was compared to a living creature—logic the bones, physics the flesh, ethics the soul. Plato separates logic as the knowledge of the immutable, from physics the knowledge of the mutable. The Cynics sought a complete freedom from any object or aim in life, and renounced all science. Sokrates aimed at logical definition, and affirmed that the true nature of external objects can be discovered by thought without observation. The knowledge of one's self (γνῶθι σεαυτόν) is the true object and aim of all philosophy. Knowledge obtained from external sources is worthless; there is nothing to be learned from fields and trees. A certain philosopher is said on this principle to have put his eyes out, in order that his mind might not be influenced by external objects, and might be left to pure contemplation. (How curiously this contrasts with the plaint of Galileo just before his death, "*Proh dolor!* the sight of my right eye, that eye whose labours, I dare say it, have had such glorious results, is for ever lost. That of the left, which was and is imperfect, is rendered null by a continual weeping.") Others of the ancients allowed that geometry might be employed for the measurement of land, and astronomy cultivated so far as it might be of use to sailors, but on no account as serious subjects of mental occupation.

Thus it happened that natural science made but little progress among the ancients; thus it happens that a schoolboy of twelve knows more about earth, and fire, and water, than was dreamt of in the philosophies of the greatest thinkers of antiquity. Let us, however, give them their due; let us confess that Plato possessed the "finest of human intellects, exercising boundless dominion over the finest of human languages;" that Aristotle was the greatest genius the world has ever seen; that as pure intellectual evolutions they have handed down to us a mass of grand philosophy; ten thousand noble efforts of the human spirit. Everything favoured the exercise of the unaided intellect, while it is hard to estimate the difficulties which presented themselves in the investigation of nature. At one period it was considered impious to attempt to explain the manifestations of the gods. There was an outcry in Athens, a popular demonstration, when the thunderbolts of Zeus were referred to common fire produced by the collision of clouds. The feeling was of the same nature as that conveyed by Campbell's stanza:—

> When Science from Creation's face
> Enchantment's veil withdraws,
> What lovely visions yield their place
> To cold material laws!

only the feeling existed in an intensified form, for here the first of the gods was derided—the Olympian Zeus, Lord of the Air, he who rides upon the storm, and hurls the thunderbolt. For a length of time, therefore, any investigation of nature was impossible for religious reasons. Men were to worship nature, to be filled with awe and wonder—δεισιδαιμονία—in presence of great natural phenomena, but not to inquire too closely into their causes. Twenty centuries later the Doctors of Salamanca who interrogated Columbus, the Inquisitors of the Sacred College who examined Galileo, upheld the same old doctrines, albeit the old gods had passed away. But the investigation of nature was impossible among the Greeks; their capabilities were very limited, they had no instruments for observations or experiments of any kind, neither had they the faculty of observation; their minds were untutored in that particular direction. Then they had to contend against their own particular habit of thought, the extreme tendency to concretion, to hasty generalisation from purely mental premisses; or if an observation had been made, a broad general law was deduced from it without further observation. So also the Chaldæans and Parsis had to contend against the mysticism, the astrology, and magic, which originated among them; and the ancient Hindu was so given to extreme abstraction, and to the evolution of all manner of strange metaphysical dogmas, that we could scarcely look for much science from an Eastern source. Egyptian learning was monopolised by the priests, and they so wove together the real and the unreal, and were so secret withal in their actions, that although much of the Greek learning came direct from Egypt, we cannot trace it to its direct source, or point to one Egyptian writer on philosophy. The Greeks, too, received much from the Phœnicians; but here also we find no record. We will presently inquire more fully into the exact amount of science possessed by the ancients.

We have chosen for our historical survey one of the oldest of the natural sciences, for obvious reasons, the chief being that it will enable us to observe more minutely the early thoughts of ancient peoples in regard to certain phenomena of nature. The science of chemistry does not owe its existence to any one people, or to any sudden process of development. The basis of the edifice is sunk deep in Eastern soil; the time when the foundation stone was laid is too remote to be even suggested; the walls were slowly and laboriously raised during the Middle Ages, and were completed by Lavoisier, Black, and Priestley; the men of our day are working at the roof. We neither hold with M. Goguet that Moses possessed considerable knowledge of chemistry, because he dissolved the golden calf, nor with M. Wurtz, when he says "La chimie est une science Française. Elle fut instituée par Lavoisier d'immortelle mémoire." Chemistry was not a science until long after the time of Moses; it was a science long before the time of Lavoisier. We wonder what Dr. Hermann Boerhaave of Leyden (whose large quarto "Elementa Chemiæ" was published in 1732, nine years before the birth of Lavoisier), would say to the proposition of M. Wurtz. Short of this, it would be difficult to overrate the services which Lavoisier rendered to chemistry. But the science has grown up by a gradual process of evolution; upon its surface we find the impress of many and diverse phases of thought and of action; the science of to-day is the summation of many intellectual efforts produced by the constant struggle of the human mind for truth. How often that truth has been hidden by a mass of sophistries; how often it has been absorbed by some false philosophy to appear again untarnished in due time; how often the attempt has been made to crush it under foot; and how it has ever risen to the surface at last, all who read the history of faiths, nations, ideas, must know. It will be our object to show this is the study of the particular science which now engages our attention.

The word χημεία first occurs in the Lexicon of Suidas, a Greek writer of the eleventh century; he defines it as "the preparation of gold and silver." In the "Lexicon Græco-Latinum" of Robertus Constantinus, published in 1592, the same definition is given, and Suidas is quoted as the authority. According to Olaus Borrichius, however, there were Greek writers on alchemy before this date; there is said to be a Greek MS. of the fifth century on alchemy in the King's Library in Paris, and others of a somewhat later date in the libraries of Munich, Milan, Venice, Hamburg, and Madrid; but we are inclined to doubt whether any of these were written before the ninth or tenth century. They are probably the work of monks living at Alexandria and Constantinople; indeed, one of them is entitled, "Cosma the Monk, his Interpretation of the Art of making Gold." The titles of some of the others will prove to us that we can place but little faith on any date which may be assigned to them:—

"Heliodorus on the Art of making Gold" (περί χρυσοποιήσιος).

"John the High Priest, in the Holy City, concerning the Holy Art."

"Isis the Prophetess to her son Orus."

"Moses the Prophet on Chemical Composition" (περί χημευτικῆς συντάξιος).

"Cleopatra on the Art of making Gold."

"Democritus the Abderite, the Natural Philosopher, on the Tincture of Gold and Silver, and on Precious Stones and Purple."

Equally worthless, we believe, are the Greek derivations of the word chemistry. Many (among others M. Hoefer) derive the word from χέω, to fuse or melt, because the majority of old chemical operations were effected by fire—witness calcination, ignition, distillation, sublimation, desiccation, reverberation. The earliest chemical arts, such as the smelting of metals and the production of glass, were also operations of fire. Indeed, the science has been called *Pyrotechnia* (πῦρ τέχνη, the art of fire), because, says Lemery, in his "Cours de Chimie," "we in effect produce all chemical operations by means of fire." Others derive chemistry from χῦμα—that which is poured out, a liquid, in allusion to the various liquids used in chemical operations; but this derivation is not worth a moment's notice. We must rather look to an Egyptian source. Plutarch tells us that Egypt was called *Chemia*, on account of the black colour of the soil, and that the same term was applied to the black of the eye, which symbolises that which is obscure and hidden. This word is related to the Coptic *khems* or *chems*, which also signifies obscure, occult, and is connected with the Arabic *chema*, to hide. It is probable that we have here the true derivation of the word chemistry. The first treatise on the science, the date of which is known with any certainty, was written by the Arabian Yeber or Geber, and at that time (the eighth century), Arabic learning had considerable influence on European culture. The science was called the *occult*, or *hidden*, because it related principally to the secret art of the transmutation of metals, as the definition of Suidas, given above, and the earlier works on the science prove. The term *black art* has been applied both to alchemy and to the magical arts so often associated with it, and clearly agrees with the above derivation. The *al* in alchemy is the Arabic particle *the*, so that alchemy signifies "the hidden science" *par excellence*; we notice the same prefix in *al*koran, *al*cohol (the burning liquid), *al*kali (the acrid substance), *al*gebra, *al*embic (the cup-shaped vessel), and in the names of many stars, as Aldebaran, Algenib, Alpheratz,—all words of Arabic origin.

Whatever difficulties there may be in determining the precise derivation of the word chemistry, there can be none in defining the science as distinctly and definitely the science which treats of the *changes* which matter undergoes; while physics proper treats of the action of various forces—heat, light, electricity, magnetism—upon matter, in all cases unaccompanied by any change of composition. If we heat a piece of iron to redness, or cause it to convey an electric current, or place it in contact with a magnet, it has been submitted to various actions, but when they are removed it returns to its original condition. On the contrary, if we fuse it with sulphur a chemical change takes place, a new substance is formed, and the iron does not return to its original condition. This idea of change is the fundamental chemical conception. The first man who made glass, or extracted a metal from its ore, effected a chemical change; the idea became most sovereign and dominant in alchemy, the attempt to change base metals into gold; it reigned throughout the period of phlogistic chemistry, for was not phlogiston a subtle entity which effected changes in matter according as it was assimilated by matter or rejected from it? It is equally the character of the chemistry of Lavoisier and Cavendish, of Davy and Dalton, of Bertholet and Cannizzaro. The "philosopher's stone" (of which much more anon) was a substance supposed to *change* all things into gold; the "elixir vitæ" was a substance which was to *change* old men into youths; the "universal solvent" was to *change* everything to a liquid form. Let us look at some of the definitions of chemistry. Boerhaave says, "Chemistry is an art which teaches the manner of performing certain physical operations, whereby bodies cognizable to the senses, or capable of being rendered cognizable, and of being contained in vessels, are so changed by means of proper instruments, as to produce certain determined effects, and at the same time discover the causes thereof, for the service of various arts." Sir Humphrey Davy writes as follows:—"Most of the substances belonging to our globe are constantly undergoing alterations in sensible quantities, and one variety of matter becomes, as it were, transmuted into another. Such changes, whether natural or artificial, whether slowly or rapidly performed, are called chemical; thus the gradual and almost imperceptible decay of the leaves and branches of a fallen tree exposed to the atmosphere, and the rapid combustion of wood in our fires, are both chemical operations. The object of chemical philosophy is to ascertain the causes of all phenomena of this kind, and to discover the laws by which they are governed." Quite recently Dr. Miller defined chemistry as "the science which teaches us the composition of bodies," and such knowledge we can only obtain by pulling matter to pieces (analysis), or by building it up (synthesis). Dr. Hofmann of Berlin has defined the vast body of so-called organic chemistry as "the history of the migrations of carbon," and is not migration change of place?

Chemistry, then, is the science which treats of the various kinds of matter, whether simple or compound, of which the world is composed, their properties, and the laws which govern their combination with, and separation from, each other. We shall first discuss any ideas of the ancients which bear upon changed matter in any form or condition: thus their early cosmogonies; the knowledge they possessed of metals and compound bodies; and their various technical operations, such as glass-making and smelting, alike demand our attention.

If we compare all the earliest ideas as to the formation of the world, we find them resolve themselves into the belief that the ether and chaos, mind and matter, were the original principles of things. The ether, a subtle vivifying principle, "passing as a mighty breath over the chaos; the chaos a boundless watery expanse without form" It was thus according to Sanchoniathon in the belief of the Phœnicians, and the twenty-five principles of the Hindu philosophy of San'chya are finally reduced to these—matter and spirit, nature and soul. The Egyptian deity was called Nûm as the spirit moving over the face of the waters, Pthah as the principle of production. The Hindu deity Brahme typified the productive force of nature. Among more western nations Gaia, the personification of earth, was held to be the first that sprung from Chaos, and the wife of Ouranos. Okeanos was their son, and according to Homer was the source of all the gods. The worship of the elements, and of the sun and moon, was among the very earliest forms of worship; thus we have in India, Agni the god of fire, Indra the god of the firmament; the sun was sometimes worshipped as a symbol of the deity, sometimes as a deity; fire was worshipped by the ancient Persians as a symbol of the deity; in the Homeric religion we find the Olympian Zeus, lord of the air, who possesses absolute and Universal power. We must notice, too, Aidoneus, the brother of Zeus, and lord of the Underworld, said by some of the Greek philosophers to designate earth, and undoubtedly an old nature power. Again, "Hephaistos," says Mr. Gladstone, "bears in Homer the double stamp of a nature power representing the element of fire, and of an anthropomorphic deity who is the god of art at a period when the only fine art known was in works of metal produced by the aid of fire." He is also one of the seven star-deities of Chaldæa, the signs and names of which were given at an early date to the seven metals.

G. F. RODWELL

THE AMERICAN EXPLORING EXPEDITIONS[*]

THE various Government exploring expeditions, the departure of which to the fields of operation for the season we have already announced, are busily engaged in carrying on the important work entrusted to them; and it will be safe to expect as the result a larger addition to our stock of detailed information respecting the western regions of America than has ever been brought together during a single year. The most important of these parties are the northwest boundary survey, the geological explorations of Mr. Clarence King along the fortieth parallel, and the surveys of Lieutenant Wheeler in Nevada and Arizona, under the War Department; that of Prof. Hayden, in two divisions, under the Interior Department; and that of Major Powell in Colorado, under the Smithsonian Institution.

Perhaps the most thoroughly equipped and elaborate exploration is that of Lieutenant Wheeler, which is now fairly in the field, and engaged in carrying on its work. This has for its object a thorough investigation of the region west of the hundredth meridian, for the purpose of determining its geographical positions, thoroughly working out its topography, and investigating its geology, natural history, and climatology.

As the basis of this work, it is proposed by Lieutenant Wheeler to divide the region referred to into eighty-five rectangles of equal size, and to mark their corners with great precision, then, taking each one in detail, to determine its astronomical, physical, and natural history features. This, of course, will require considerable time for its completion; and it is hoped that Congress will grant the necessary authority, so that the work may be accomplished as speedily as possible. As each rectangle is elaborated, it will, of course, join on to those previously investigated; and an index map is to be carried along simultaneously for the more ready understanding of the details. Eight rectangles have been completed by Lieutenant Wheeler in his previous expeditions, and it is expected that thirteen will be finished by the end of the season.

To carry out this programme certain points are to be determined astronomically with great precision, and these as nearly as possible along a continuous parallel. Those already selected are, according to the *New York Herald*, a point near Beaunois, near north-western Kansas; the crossing of the Union Pacific and the western boundary of Nebraska; Cheyenne; the eastern limit of the survey of the fortieth parallel by Clarence King; Sherman, the highest point on the Union Pacific; Fort Steele; Laramie City; the crossing of the Union Pacific and the western boundary of Wyoming; the crossing of the Central Pacific and the 120th meridian; and a point on the western boundary of Nevada.

Telegraphic determination of the longitude will be used very freely, and for this purpose Brigham Young has kindly permitted the employment of his well-equipped observatory in Great Salt Lake City. It is proposed to establish a principal station at or near Sherman, the position of which will be determined with the utmost accuracy, and to use this as a point of reference for the other stations referred to. The work of the present season will be carried on almost simultaneously in Utah, Arizona, and Nevada, several divisions of the main party having already been organised and set to work. The southern and south-western portions of the Salt Lake basin are to be explored; also the mining regions on the Virgin and in Eastern Nevada. It is proposed to establish astronomical points, by means of which to determine with greater accuracy the location of the mineral veins. The Wasatch Mountains will constitute the eastern limit of operations during the year.

The expedition, as organised, embraces the following among the more important of the *personnel*:—Lieut. George M. Wheeler, United States Engineers in command; Lieuts. R. L. Hoxie and W. L. Marshall, U. S. Engineers; Dr. H. C. Yarrow, surgeon and naturalist; T. V. Brown, hospital steward and meteorologist; G. K. Gilbert and E. E. Howell, geologists; J. H. Clark and E. P. Austin, astronomical observers; Louis Nell and John E. Weyss, chief topographers; H. W. Henshaw, assistant naturalist; M. S. Severance, ethnologist; and William Bell, photographer.

At the latest advices the latitude and longitude of Beaver, in Utah, were being determined by Mr. Clark, Mr. Austin being stationed at the Salt Lake City Observatory. Pioche, in Nevada, will be the next point to be occupied. One branch of the expedition, under Lieut. Hoxie, and accompanied by Dr. Yarrow as naturalist, is exploring the regions west of Great Salt Lake City; while the other, under Lieutenant Wheeler, is surveying the Wasatch and the Sevier River regions east of it. From these main divisions parties are sent out to examine the water-courses and mountain regions of the country traversed. They will all concentrate at Beaver, Utah, about October 1, and proceed together toward the south.

ITALIAN SPECTROSCOPY[*]

PROF. TACCHINI presented the matter for the fourth issue of the *Giornale degli Spettroscopisti*, consisting of two memoirs, one by Prof. Blaserna, on the displacement of the lines of the spectrum according to the heat of the prism; the other, by Prof. Donati, on observations of the spectra of solar spots made at Florence with a new spectroscope.

The new spectroscope of Prof. Donati contains twenty-five prisms. They are so arranged that the eye receives only Fraunhofer's line C, and a small portion of the red to the right and left of that line. With this spectroscope Donati has succeeded in seeing clearly the line C reversed on the nucleus of the spots. It does not appear that any of the Italian observers have yet seen the prominences on the disc, a result announced by Lockyer in 1869.

Prof. Tacchini further directed the attention of the Society to his last spectroscopic observations of the sun. For the last few days the number of the protuberances had been rather small, but the chromosphere had been greatly developed, and the vapours of magnesium mixed with it had occupied regions of vast extent. He exhibited a drawing of the spectroscopic image of the Sun's edge, taken on the morning of the 6th of May, 1872, showing the continuous presence of magnesium over an arc of 168°, extending from the north pole to distances of 50° and 118°. This was the first time that he had observed a magnesium region of such vast extent in the sun. And taking account also of isolated tracts, there results a total of 222°, that is to to say, nearly two-thirds of the entire edge, occupied by magnesium vapours more or less intense. The drawing likewise shows the usual correspondence between the faculæ, the magnesium regions, and the portions of the edge at which flames arise to the height of 14 to 28 seconds.

Lastly, Prof. Tacchini gave an account of some spectroscopic observations made at Geneva by Prof. E. Gautier, and exhibited the drawings of a protuberance observed by Gautier on the 15th of April of this year, which serve to confirm the observations made at Palermo on the solar rains, that is to say, masses of luminous hydrogen suspended in the sun's atmosphere, which gradually separate, and ultimately unite at the edge of the disc, and then present all the appearance of eruption, whereas their formation actually takes place by a directly opposite process.

Prof. Blaserna said that he had heard with much interest of Donati's attempt to observe the reversal of the lines on the solar spots. He had also, in accordance with the admirable conferences of Prof. Tacchini in January last, occupied himself with the problem of observing the protuberances on the full solar disc. He then wrote to Prof. Tacchini a detailed letter, proposing two different methods of arriving, if possible, at the solution of this important problem.

The first of these methods, already applied by Janssen and Lockyer to the protuberances on the solar edge, and now adopted by all spectroscopists, consists in using spectroscopes of continually greater power. Prof. Donati has also pursued this method, and has now arrived at the construction of a spectroscope of twenty-five prisms. Theoretically, it is highly probable that in this manner the protuberances might ultimately be seen in full sunshine. But for this it would be necessary to go much further with the number of prisms, increasing them to 50, 73, or perhaps even to 100. This, however, involves a great practical difficulty, and, moreover, it is doubtful whether so powerful a dispersion would not weaken the phenomenon to such an extent as perhaps to render it impossible to see anything at all.

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[*] Communicated by the scientific Editor of *Harper's Weekly*.

[*] Societa di Scienze Naturali ed Economiche di Palermo, May 18, 1872.

The second method, to which he is inclined to give the preference, consists in attaching to the eye-piece of a good telescope a spectroscope which shall form a real spectrum, well defined and sufficiently extended. A diaphragm is provided with a fine moveable slit, adjusted so as to permit the passage only of the Fraunhofer line C and the line D_3. This slit acts like the slit of a second spectroscope of high dispersive power.

The advantage of this construction consists in intercepting all the solar rays excepting those which correspond to the lines which it is desired to study, or those in their immediate vicinity. The extraneous solar light is thereby arrested, and by dispersing this isolated beam by means of a second powerful spectroscope, Prof. Blaserna believes that we must ultimately succeed in seeing the protuberances on the full solar disc.

The importance of such a fact for spectroscopy induced him to associate himself with Professors Cacciatore and Tacchini, for the purpose of putting it to the test, but the means at their disposal were too slender, and neither did nor could yield any result. For this reason he believes that it will be useful to explain the method, in the hope that some other spectrocopist, and perhaps Donati himself, may follow it out with better means and greater success.

SCIENTIFIC SERIALS

Journal of Anatomy and Physiology, vol. vi., part 2, May. A large portion of this number of the Journal is occupied by a series of papers on Myology, by Prof. Humphry; among them by far the most important is one in which the writer indicates a general plan on which the muscles of vertebrate animals are arranged. Prof. Humphry's scheme is simply this:—The locomotory system of a vertebrate animal consists fundamentally of a successional series of alternating skeletal and muscular planes, having generally a transverse direction between the axial line and the circumference. The skeletal planes, "sclerotomes," are represented in the high vertebrate classes by the vertebral processes, ribs, limb and hyoid girdles, tendons of the dorsal muscles, Poupart's ligament, tendinous inscriptions on the rectus abdominis, &c. The muscular planes, "myotomes," are made up of muscular fibres, the general arrangement of which is in an antero-posterior direction. The muscles of the trunk may be grouped under two heads, the dorsal muscles and the ventral muscles, the latter being disposed in three layers The muscles of the limbs are derivatives from the middle stratum of the ventral muscle with a funnel-shaped investment derived from the external stratum. Prof. Humphry's other papers are on the arrangement of the muscles of the Lepidosiren, the Ceratodus, the smooth dog-fish, and the glass-snake.—Prof. Turner furnishes a description of this sternum of the sperm whale. Hitherto in the specimens of the cetacean that have been examined the sternum was incompletely ossified, so that the present communication fills up a gap in our knowledge —Dr. Hollis, in a paper entitled "Tissue Metabolism, or the artificial induction of Structural Changes in Living Animals," describes some experiments made with mechanical and chemical irritants on the now nervous, now vascular tissues of Actinia. The results point to nothing beyond what has been before observed, a swelling and softening of the tissues, with a proliferation of the nuclear elements. Dr. Hollis also furnishes a short paper "On the Homology of a Mandibular Palp in certain Insects," and a note "On the Growth of the Masticatory Organs of Isopod Crustaceans."—Dr. Garrod, in a paper "On Sphygmography," points out the objections to the ordinary "knife-edge" sphygmograph, and describes a new instrument by Bregnet, in which these defects are remedied by a rack-work plan of construction. He further points out most clearly and forcibly the direction in which this apparatus is most useful as a means of observation, both to the physiologist and physician —Dr. Braxton Hicks brings forward some most valuable evidence against the idea of a placental sinus system into which the fœtal silli protrude, and almost proves that normally no blood exists among the silli.—Prof. Traquair describes the caudal fin of the tailless trout of Islay.—Mr. Stirling notes Trichiniasis in a rat caught in the neighbourhood of a dissecting-room. Several anatomical anomalies occurring in the human subject are recorded in this journal. Mr. Bradley provides some notes on myological peculiarities.—Mr. Champneys describes a communication between the external Ilian and Portal veins.—Dr. Watson mentions a case of the termination of the thoracic duct at the junction of the *Right* subclavian and internal jugular veins; and Mr. Galton reports from Vienna the case of a man possessing two supernumerary teeth behind the upper median incisors. The number concludes with the usual review of books and the reports on the progress of anatomy by Prof. Turner, and on physiology by Drs. Rutherford, Brunton, and Ferrier.

Journal of the Chemical Society, May.—This number opens with the proceeding at the anniversary meeting of the Chemical Society, and also the address of the president on that occasion. Dr. Frankland in his address reviewed the present condition of chemical research in this country, as exemplified by the number of original papers received and read before the society, pointing out that during the past year only 22 papers have been received from the members, the number of whom has reached 656, 32 of these being foreign members; whilst, on the other hand, the German Chemical Society, which numbers 528 native members, has received during the same period the results of no less than 238 original researches. Dr. Frankland mentioned one fact which he believed to be one of the principal causes of this comparative lethargy on the part of English chemists. It is that our Universities and examining bodies do not recognise original research, but are content to accept book knowledge to a great extent; and that, on the other hand, in Germany a candidate for a scientific degree has to submit a memoir or dissertation on some original investigation before he is admitted to examination. The only original communication in this number of the journal is by Dr. Debus, on "The action of sodium amalgam on alcoholic solution of ethylic oxalate." In the year 1864 Friedlander, experimenting on this subject, obtained a substance which he named glycolinic acid, to which he assigned the formula $C_2H_4O_4$, that is isomeric with glyoxylic acid. Dr. Debus has now carefully repeated Friedlander's experiments, but has not succeeded in obtaining this body, but instead of this the sodium salt of glycolic acid. Several attempts were made under varying conditions, but all failed to produce the first-named body, sodium glycollate being obtained. As one of the by-products of the reaction in question, Dr. Debus has isolated tartaric acid. It is probably formed by the action of a molecule of hydrogen on one of oxalic ether, which would yield ethylic glyoxalate and alcohol; and it will then be seen that one molecule of hydrogen, combining directly with two molecules of ethylic glyoxalate, would yield ethylic tartrate. The abstracts of foreign papers contain many of great value, several of which have already been noticed in these pages.

Verhandlungen der k. k. geologischen Reichsanstalt, No. 9, 1872. There is not much of special interest for English geologists in this number of the Proceedings. Amongst the papers are the following:—"On the movements which the sedimentary formations of France have undergone," by M. Delesse; in which the author's studies lead him to the conclusion that the sedimentary strata that are buried in the earth's crust, are always in a more or less soft condition; and "A Contribution to Richthofen's theory of the Loess," by D. Stur. The literary notices and reviews which complete the number are unusually full.

Prof. E. D. Cope contributes to the *American Naturalist* for July an exceedingly interesting account of the Wyandotte Cave and its Fauna, to which we shall probably take an opportunity of again referring. Another important article in the same number is by Dr. H. Hagen on Mimicry in the Colours of Insects. Dr. Hagen distinguishes three different kinds of colours as present in insects—viz, colours produced by interference of light, colours of the epidermis, and colours of the hypodermis. The colours produced by the interference of light are only optical phenomena. The epidermal colours belong to the pigment deposited in the cells of the chitinised external skin or epidermis, and are mostly metallic blue, green, bronze, golden, silver, black, brown, and rarely red; they are persistent and never change, either during life or after death.

SOCIETIES AND ACADEMIES

Philadelphia

American Philosophical Society, December 15, 1871.—A sum of money was appropriated for the planting and preservation of a grove of oaks in Fairmount Park, to be called the Michaux Grove, in accordance with the will of the botanist Michaux.—Prof. E. D. Cope read a paper "On the Pythonomorpha of the Cretaceous Strata of Kansas." This embraced a synopsis of the species of the order known from all parts of the world, by which it appeared that America was its home, only four species having been described from Europe. He said that the *Danubiosaurus* of Bunzel had no relationship to the group. The American species were forty-two, distributed as follows: viz., New Jersey Greensand, 15; Rotten Limestone of Alabama, 7; Chalk of Kansas, 17; other localities, 3. The Kansas species were referred to *Clidastes* 3 sp., *Edestosaurus* 4 sp., *Holcodus* 4 sp., *Liodon* 6 sp. Of these *Edestosaurus tortor* and *E. stenops; Holcodus coryphaeus* and *H. tectulus;* and *Liodon curtirostris, L. latispinus, L. glamiferus, and L. crassartus* were described as new.

January 5.—Hon. Eli K. Price read a paper "On some Phases of Modern Philosophy," in which he combated the views of the heterogenists and of the evolutionists. In the latter part of the subject he opposed the views of Darwin, asserting that the variations seen among domesticated animals had no parallel among those in a state of nature, and the fact of their ready hybridisation is an indication of their specific unity. He quoted Prof. Wyville Thomson to the effect that no transition from species to species had ever been observed in palæontological history; and asserted that the variations observed among animals on which the developmentalists relied in evidence of their theory were few and abnormal, and utterly insufficient for the use made of them; that the origin of man from apes was not supported by evidence; lastly, that the theories of evolution are highly injurious to faith and morals, and thus to Christian civilisation.

Janaury 19.—Mr. Benj. Smith Lyman read a paper on "The Oil-bearing Region of the Punjaub," accompanied by a topographical map. He pointed out the tertiary age of the oil-bearing strata.—Prof. Cope read a paper on a new Dinosaurian from the cretaceous strata of Kansas, which was named *Cynocercus incisus.* The vertebral articular faces were deeply excavated above and below, so as to give them a transverse character.—Prof. H. Hartshorne read a paper on "Organic Physics." It explained that the expression "organic physics" is as well justified as "organic chemistry" and "animal mechanics," for vital force is clearly correlated with other physical forces, as heat, light, &c., but the correlation is not identity. Advocates of the continuity theory have endeavoured to make it appear to be identity, but they will not succeed; because the effects of heat, light, electricity, magnetism, and gravitation are known, and they always tend (in the absence of life) to an opposite kind of change to that which occurs under life force; namely, they form of C, H, N, S, P, &c., compounds of few equivalents and stable equilibrium; while under life force the same elements are made to produce compounds of many atoms or equivalents, and of unstable equilibrium. The first are mainly crystalloids, the second always colloids. The directness of this opposition is especially demonstrated by the result of death (arrest of life force), which is attended by the resolution of the complex, unstable, colloidal, organic substances into more simple, stable crystalloids and gases. Eliminating all the functions of living beings otherwise explicable, we must restrict the term "vital action," or "action of life force" to the conversion of inorganic into organic material, with type-formation or organic construction as its result. It is supposable, at least, though not proven, that the assumption of particular forms under given circumstances is (analogous to crystallisation) the property of the bioplasm; *i.e.*, given the matter, the form results as its property or attribute. But chemists have never succeeded in making organisable matter by synthesis; nor is it likely that they ever will. All complex organic substances made in the laboratory (as urea, by Wohler; fatty acids, &c., by Berthelot; and even, if made, crystallisable neurin) are post-organic (a term first used by the author), *i.e.*, results of the downward or retrograde metamorphosis; produced, not by life force, as such, but by the composition or balance between life force and the other forces. They are not germinal or formative, but formed and effete materials (Beale's terms). The question of the possibility of abiogenesis is not yet finally decided. Crosse gave it momentum with his galvanised acarus; Pouchet and Pasteur have long debated it; Owen, Bennett, Clark, and a few others have of late years reasserted it; Bastian (NATURE 1870) makes an elaborate experimental defence of it. We note concerning it as follows:—(*a*) The manipulation (to avoid introducing minute visible forms) requires an almost or quite impracticable delicacy throughout. (*b*) When heat is used, we have always the alternative, to conclude that certain minute organisms, germs or spores, can resist a higher temperature than was supposed, or to conclude that, taking for granted that the heat employed must have killed all germs, new life afterwards sprang up, without parentage. All experience makes the former much more probable. George Pouchet's experiments with rotifers tend this way. Jeffries Wyman found that, although four hours' boiling would not, five hours would put an end to all manifestations of life. Franklin's experiments (and Calvert's) gave similar results against abiogenesis. Supposing (although Huxley does not) that Bastian could not have mistaken "Brownian" molecular movements for evidence of life, we yet observe that if life sprang up in Bastian's apparatus, it was such life as can exist without air or oxygen; altogether unlike, therefore, ordinary world-life. The assertion of Pasteur is justified, that the *onus probandi* lies with abiogenesists, since there is no experience of any living form more than $\frac{1}{1000}$ of an inch in diameter springing into life out of inorganic matter; it is therefore vastly improbable (needing most cogent evidence to prove), that any form less than $\frac{1}{1000}$ of an inch in size can be made to spring into life from inorganic matter. While abiogenesis is unproved, we hold to the conclusion that vital force is not the mere outcome or resultant of any or all of the other cosmic forces. How does it differ? Of the organic cell, or "physiological unit," the most constant determinable acts or changes are incretion and excretion; atomic or molecular motion, definite in results, is an essential of life. Must not the motion itself be peculiar? More definitely, we find that while in the condensation of matter in the (nebular theoretical) formation of the sun and planets there was integration of matter with dissipation of force, such as heat (H. Spencer), life action involves integration of matter with accumulation of force (stored up physical force in the plant, of Barker; "bottled sunshine," of some one else). This is a striking contrast. Sexual union is closely analogous to chemical union; instead of combustion, it makes construction by detaining products. Again, we notice the analogy between the spiral phyllotaxis of plants (opposite leaves a double spiral), (whorls two or more, and bilateral symmetry of vertebrates and articulates and some molluscs, and radial symmetry of radiates and cœlenterates corresponding) and the spiral helix of the electro-magnet. As the opposite chemical and polar elements of the battery are to the current of the helix, so (may be) the polarities of the sperm cell and germ cell to the spiral phyllotaxis of plants and symmetrical (usually double) organotaxis (a new term) of animals. A close (but reversed) analogy exists between heat force and vital force. A spark of fire may "light," and so burn successively, an indefinite amount of combustible matter. A spark of life may animate an indefinite amount, successively, of organisable matter. The former, combustion, reduces complex substances which are unstable to more stable compounds. The latter, life, elevates simple substances to more complex states, but with constant transmutation of their forms. Such analogies are as yet crude, and do not solve the mystery of life. But the facts on which they rest justify and encourage the physical investigation of vital actions, including their study under physics—organic physics. Such a view of life is in no manner antagonistic to theism or to "teleology," any more than is the now familiar reduction of digestion, circulation, absorption, &c., to the category of chemical or physical phenomena. All such analytical inquiries are moreover, legitimate so long as they are accurate, whether they point to biogenesis or abiogenesis, to the origin of types by interrupted appearances or by evolution.—A discussion on E. K. Price's paper, read January 5th, took place, in which Prof. Hartshorne, Prof. Lesley, Mr. Price, and Prof. Cope took part. Prof. Hartshorne supported the opposition to abiogenesis expressed in the paper, on the ground of insufficiency of evidence in its favour, but believed in the evolution of species. Prof. Lesley objected to the insufficiency of Mr. Price's reasoning against the labours of experts in biological science; and stated that the more attention he paid to the subject the better satisfied he became that man was descended from apes. Prof. Cope stated that Mr. Price's paper was in error as to the facts. That (1) variability of specific type was even more common in nature than under domestication, examples from many so-called "protean" genera being cited. (2) That some wild species did produce fertile hybrids. (3) That transitions between species, both at the present time and in past geological periods, were common, but were concealed by a universal *petitio principii* involved in the practice of naturalists. This consisted in uniting distinct forms or species under the head of one species as soon as the intervening connections were formed. (4) That the known cases of transition were numerous, not few; and that common induction required that we should believe of the unknown that which we see in the known, when other circumstances are identical.

Feb. 2.—Prof Geo. B. Wood communicated further results of his experiments with salts of potassa on vegetation, and especially on grain and fruits. He stated that in a field of grain devoted to the experiment, in which the soil had been previously exhausted by bad culture, one half was enriched by farm-yard manure, the other with the same with wood ashes added. The effects of the latter were especially marked, and much greater than with the former. The most striking results were attained by the use of the ashes of the poke, *Phytolacca decandra*.—Prof. Cope read a paper on the "Families of Fossil Fishes of the Cretaceous Strata of Kansas." The greater part of these were shown to be *Physostomous Actinopteri*, of three families, viz, the *Saurodontidæ*, the *Pachyrhizodontidæ*, and the *Stratodontidæ*. Of the first, four genera and ten species were described, some of them (*Portheus* sp.) among the most formidable of marine fishes. The peculiarities of the succession of teeth in *Portheus* and *Saurocephalus* respectively were pointed out. Of *Pachyrizodontidæ*, one genus and four species were described; and of *Stratodontidæ*, three genera and seven species. *Stratodus* was a form provided with multitudes of minute shovel-headed teeth.

PARIS

Academy of Sciences, Sept. 9.—M. Faye, President.—The first paper was by M. P. Duchartre, on the bulb of *Lilium Thomsonianum*, &c. The author finds that this Indian plant seldom flowers in Europe, and traces this to the facility with which it propagates itself by means of off-shoots from the bulb. If it is prevented from doing this it flowers well.—A letter from P. Secchi followed on "Observations on the Variation of the Solar Diameter; Observations of the Protuberances and of the Chromosphere; Observations on the Shooting Stars and of the Aurora Borealis observed at Rome on the 10th of August." Father Secchi finds variations of the solar diameter equal to 3. 4, and even 5 seconds of arc (error of observation less than 0″·5 arc). There were minimum epochs in July, the beginning of September, the middle of November, and the beginning of March and April, when the mean diameter was 32′ 1″ 5‴; and maxima in the middle of August, the middle of September, and during the whole of October and December, and the beginning of February when the mean diameter was 32′ 4″ 5‴. The maxima of diameter correspond to the minima of spots and protuberances. The next memoir was by M. Max Marie "On the Elementary Theory of double integrals and their periods" (continuation). A note from M. A. Potier "On the causes of Elliptical Polarisation by reflexion on transparent bodies."—A note from M. Th. Gaffield "On the results produced by insulation on various kinds of glass," was then presented by M. Chevreul.—"On the lines of Summit and of Thalweg" an answer to the observations of M. Boussinesq by M. C. Jordan.—A note was then read on the induction currents developed in the machine of M. Gramme, by M. J. M. Gaugain.—"On Lithurate of Magnesium, a new species of urinary concretion from the ox," was an extract from a note from M. G. Roster—The empirical formula for the body in question is $C_{36} H_{36} N_2 Mg O_{17}$, it is soluble in boiling water, from which it crystallises on cooling.—A note on the Nutoscope, by M. Ch. V. Zenger, was presented by M. Yvon Villarceau. This was a description of an instrument for illustrating the nature of nutation.—Next followed a note from M. Tarry on the Constitution of the stream of August meteorites.—M. Dumas then communicated some observations on the *Phylloxera vastatrix*.

Sept. 16—M. Faye, President.—The President read a note relative to a communication from M. Hirn on the conditions of equilibrium in, and the probable nature of the Saturnian rings.—General Morin then read a note on Major General Mayevski's "Treatise on Projectiles." M. Morin states that M. Mayevski, in his eleventh chapter, devoted to the consideration of the penetration of solid bodies and armour plates by projectiles, arrives at the same conclusions as were obtained by the Metz Commission, and by Capt. Noble, R.A., in England.—"Observations on the nature of the various parts of flowers," by M. A. Trécul, followed.—A letter from P. Secchi on the appearance of a meteor in the neighbourhood of Rome, and on stellar spectra, was then read. The latter portion of the letter was an explanation of the Rev. Father's views on stellar types, which he explained were not the same as those of Mr. Rutherfurd, as had been supposed by Messrs. Lockyer and Schellen.—M. le Dr. Netter then read a paper on the treatment of cholera by the administration of enormous quantities of aqueous drinks in successive doses.—Then followed the concluding portion of M. Marie's paper on the "Theory of double integrals and their periods."—Notes were received from M. Pigeon, on cholera; M. Charles, on aerial navigation; M. Bouvard, on the Postulatum of Euclid; M. Hervier, on *Phylloxera*; M. Quattari, requesting the Academy to examine his aerial telegraphic apparatus; and M. Le Comte L. Hugo presented the Academy with an engraving entitled, "The sphere is an equidomoid, or a demonstration of the pre-eminence of polygonal figures," which was submitted to the examination of M. Ossian Bonnet.—M. Yvon Villarceau presented a note by M. Prosper Henry, describing the discovery of a new planetoid 125 at the Paris Observatory. Observations on the above by MM. Ludinard, Tisserand, Paul Henry, and Prosper Henry followed.—An extract from a Report by Dr. Oudemans on the total eclipse of 12th December, 1871, observed in the Dutch East Indies, was also read.—A paper, by M. Ch. V. Zenger, "On the rapidity of transmission of light in simple bodies, and on their crystalline form," followed.—"On the changes of phase produced by metallic reflexion," note by M. A. Potier, was next read; and then an extract from a paper by M. Plateau on the measurement of physical sensations, and on the law which connects the intensity of these sensations to the intensity of the exciting cause, was followed by a posthumous note of M. H. Magnan's, *à propos* of two notes by M. Cayron on the cretaceous formation of La Calape and Corbières.—M. Louis Faucon sent some observations on *Phylloxera*, made by himself and M. Gaston Bazille; and another note on the same subject and on vine disease was received from M. F. E. Guérin-Méneville, who believes that every observation tends to prove that the *Phylloxera vastatrix* is only a secondary agent in producing the vine disease now so destructive.—M. Yvon Villarceau then presented a note from M. Fron on the atmospheric movements which accompanied the auroræ of September 2 and 6, 1872—M. Georges sent a note relative to the employment of calcic disulphite to the cure of vines tainted with oidium, which was sent to the Phylloxera Commission.

BOOKS RECEIVED.

ENGLISH.—Cardiff Naturalists' Society Report and Transactions. Vol. III, 1870-71, part 1.—Cholera and Efforts towards Framing an Equilibrium Theory of Health and Disease (Thacker and Co, Calcutta)

FOREIGN.—Tableau de l'Astronomie: Ed. Mailly (T. Hayez, Brussels)—De l'Astronomie dans l'Académie Royale de Belgique: E. Mailly (T. Hayez).—(Through Williams and Norgate.)—Lehrbruch der Zoologie: Dr Otto W. Thomé.—Der Mensch und die Seele: E Reich.—Etudes sur les Appendiculaires du detroit de Messine: H. Fol.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, OCTOBER 10, 1872

HOUZEAU ON THE FACULTIES OF MAN AND ANIMALS

Etudes sur les Facultés Mentales des Animaux comparées à celles de l'Homme. Par J. C. Houzeau, Membre de l'Académie de Belgique. 2 vols. pp. 1,008. (Paris: Hachette, 1872. London: Williams and Norgate.)

IF this work had appeared a few years ago it would have created for its author a considerable reputation. Even now, had it been written in Europe after a careful study of all the best authorities on the subject, it might have been made a very valuable and important treatise. But its author tells us—and the fact is clearly reflected in its pages—that it has been mainly written during a residence in the less cultivated parts of America, without the means of consulting the more recent works on the various subjects of which it treats. It is true that M. Houzeau is a close and acute observer of the habits of animals, and he has furnished us with many curious and original facts; but his own observations and experiments are so overlaid by vast masses of less trustworthy and often irrelevant matter, and are so widely scattered owing to his elaborate classification and minute sub-division of the subject, that they lose much of their force and impressiveness.

For a work written under the circumstances here stated, it is far too large and too pretentious. It aims at an exhaustive treatment of the whole series of the actions and passions of animals and of man, as illustrating their comparative mental nature. It treats in detail of each sense, each habit, each instinct, each custom, passion, and idea; and it discusses so fully the phenomena of language and society that the title should have been reversed, it being really a study of the mental faculties of Man as compared with those of Animals.

The subject of inquiry, as stated by the author, is, "whether the mental faculties of man, of which our arts, sciences, and social state are the product, have not their germs in the lower animals; whether the several parts of our intellectual and moral nature do not insensibly and successively appear in the series of the animal kingdom." He assures us that he approached this inquiry with no preconceived ideas; but we may be permitted to doubt this when we find him bringing in such forced resemblances, as the rolling of pachyderms in mud with the practice of tattooing (vol. i. p. 343), that of ruminants rubbing off their hair to the shaving of men (i. p. 348), and the existence of neuter insects with the custom of castration (i. 351). These and many other similar cases show a determination to find some point of comparison of animals with man, which diminishes the effect of the numerous real and very curious resemblances he has undoubtedly brought forward. In his chapter on the question, whether the power of animals to find their way for great distances depends on a special sense, our author comes to a conclusion which, although we believe it to be a sound one, is opposed to the facts which he adduces. He believes, for instance, that pigeons traverse an unknown country in a direct line, and that dogs and other animals find their way, in cases where neither sight nor smell could guide them. But he shows by many minute observations that animals, *in ordinary cases*, find their way by means of the same faculties which man employs to find his way, and hence he concludes that they do so *in all cases.* But this by no means explains the more extraordinary facts he has first adduced. Here we have an example of his deficiency of information. He quotes the case of the pigeons used during the siege of Paris, and evidently believes that all that was done was to take a carrier-pigeon from any part of France to Paris, when, on being let loose, the bird would infallibly return straight to its former abode. He apparently knows nothing of the fact that these birds must all be trained by means of wider and wider flights over the very country they are to traverse; and that without this precaution, a pigeon, taken by a circuitous route from Brussels or from Bordeaux to Paris, would no more find its way back to those places than would a deaf and dumb man under the same conditions.

In the chapter on the "instinct to use clothing," we have another example of our author's want of rigid impartiality. He endeavours to show that some animals use clothing, and that some men do not, and that it is, therefore, no distinctive character of man. His examples of dressed animals are hermit crabs and the larvæ of *Phryganea* and *Tinea*; and although he adduces instances of unclothed men, he has in no way accounted for that sense of shame which he maintains is not innate, and which yet has, even more than the necessity for warmth, led to the practice of clothing among so many peoples.

In the section on the Sentiments and Passions, we have an elaborate account of the wars, massacres, and cruelties, tortures and human sacrifices, among the various races of men, and we find the characteristic remark, that "the touching custom of preserving a loved one's hair is, perhaps, only a transformation of the old practice of scalping." We commend this notion to Mr. Tylor, as the *ne plus ultra* of survival of savage practices in our modern civilisation. We have also a condensed account of the tortures inflicted on their prisoners by the North American Indians, which can hardly be surpassed for terrible descriptive power; but there is no attempt to find parallels to these essentially human attributes among the lower animals.

Although our author has devoted many pages to a discussion of the principles on which evidence is to be admitted, and has laid down some excellent rules on this subject, he appears to pay little regard to them in his own work. He asserts, for instance, without any reservation, that "a large number of species of apes have a laugh altogether analogous to ours;" but the only evidence he gives of the fact is that "Turks compare Europeans to apes, because they laugh like them;" the grin of anger being here confounded with the laugh of appreciative wit. In like manner he accepts as an undoubted fact the existence of people to whom the use of fire was wholly unknown, although the two cases he gives—the Guanches of Teneriffe, and the Marianne Islanders—are highly suspicious, and have both been shown by Mr. Tylor (in his "Early History of Mankind," a book quoted by M. Houzeau) to be contradicted by many facts, and to rest on no sufficient authority.

It is in Section V., on "Ideas," that we find some of the most curious observations and suggestive remarks to be met with in the whole work. Animals we know, go mad, and they also vary in their mental capacities, but we do not remember any case of idiocy having been recorded among the lower animals. M. Houzeau, however, tells us he had an idiotic dog, which could not take care of itself, and which behaved in an altogether strange and silly manner. But the most curious thing was that its mother observed its mental incapacity, and acted accordingly. From the time when she ceased suckling it, she took great pains to provide its food, bringing it dead birds or pieces of meat, "which she had never done, even for a single day, with any of her other puppies." From this observation, and the well-known fact that animals of the same species differ greatly in their capacity to receive instruction, our author is convinced that it is only through want of observation that we do not meet with mental derangements of various kinds and degrees among our domesticated animals. The faculties of attention, observation, and imitation, exist in a high degree among most of the higher animals; and when we add to these a very retentive memory, and a certain amount of direct and voluntary instruction which parents give to their young, much may be accounted for which has been imputed to those unknown faculties which are termed instinct. Not only does M. Houzeau maintain that the higher animals constantly act by means of intellectual processes altogether comparable to our own, but he extends this view to insects. A German naturalist, Gleditsch, relates that he one day spitted a toad on a stick, which he fixed upright in the ground. A number of burying beetles (*Necrophorus vespillo*) came around it; but as they could do nothing with the toad while in the air, they mined under the base of the stick till it fell, and then buried toad and stick together. The circumstances were quite abnormal for the *Necrophori*, and they acted exactly as an intelligent and reasoning creature would do. Again, when Pierre Huber placed some humble bees (*Bombus terrestris*) under a glass with a piece of comb so irregular that it would not stand firm on its base, they at once set intelligently to work to make it secure, some holding it up while others built walls and buttresses to make all solid. So, again, when the wasp observed by Erasmus Darwin, which could not carry away a large dead fly because the wind caught the wings, cut them off, and was then able to carry away its prey, it acted exactly as an intelligent and reasoning human being would act. As our author well remarks, whenever such facts are brought forward, the usual cry is—"What an admirable instinct!" but instead of having recourse to so miraculous a faculty, able to deal with phenomena occurring perhaps for the first time in the experience of the race, would it not be more simple to suppose these creatures to possess some small portion of our faculties of observation, of memory, and reflection? The following remarks on concluding the whole subject of instinct are well worthy of attention.

"It seems difficult to regard as the effects of a blind instinct such actions as spreading out damp grain in the open air to dry, and taking care of the eggs and the young of captive Aphides. It is difficult to conceive a being performing acts so varied and complex, and so bound one to the other in a connected series of labours, without any perception of the bond of cause and effect which unites them. Animals perform automatically only simple actions depending on their immediate wants. But when the end requires a large number of preliminary and intermediate operations, of a varied character and dependent one upon the other, can we still suppose the entire line of action to be followed out in ignorance and obscurity?" And, after stating the fact of the burying beetles, who, after laying their eggs in the bodies of small dead animals, bury them in order that they may not be devoured by birds and beasts of prey, he continues :—

"If we pretend to see only instinct in this action of the insect, why should we have recourse to a different faculty when man buries his dead? Has not the act of burying for immediate end, in the one case as in the other, the securing the body from the attacks of carnivorous animals? Is there not at the same time, in both cases, a more remote end which forms the true motive of the act? The faculty of invention is doubtless more developed in man than in any species of animal; it is in him more powerful, more elevated, and often directed by nobler motives. But these differences of quantity and of nobility ought not to blind us to the existence of the faculty in various degrees of development among many animals" (ii. p. 236). Bearing also on this question, we have a curious discussion as to the power of animals to appreciate numbers. It is considered to be established that the magpie can count four, which probably refers to Leroy's experiment with crows (NATURE, iii. p. 183). The mule is supposed to be able to count as far as five at least, and this is considered to be established by the following observation. There is a short branch line of omnibuses in New Orleans, where each mule makes the journey five times successively before being changed. The veterinary surgeon of these animals called the author's attention to the fact that, whereas at the end of each of the first four journeys they are silent, as they approach the end of the fifth they neigh. But this does not seem satisfactory. The end of the fifth journey may well be determined by the estimation of mere distance, or of time, or by the sense of fatigue, or, what is still more likely, by some preparations for the change of mules which may be heard or smelt by those arriving. And this is rendered the more probable by an experiment tried by our author himself, showing that dogs cannot count even as far as two. For three successive weeks he repeated the same walk with his dogs on each alternate day; yet, although the dogs were always eager to go out when their master's preparations were seen, on the last trial, being the tenth repetition, none of them showed any knowledge that the day for an excursion had arrived. But neither is this quite satisfactory, as there is too long an interval between each trial, and it rather involves the recollection of a period of time than of a mere number. It can hardly, therefore, be held as proved that the lower animals have any sense of pure number.

Passing on to the consideration of moral and religious ideas, our author adduces the usual proofs that animals have a sense of right and wrong, but which really show nothing more than that they can be made to acquire certain habits through the fear of punishment or the expectation of reward. We next find the broad statement that the idea of duty is not universal among men, but no evidence is offered, except that no one act is held to be a duty universally, or the contrary. But this is to misapprehend the real question, which is rather, whether there is any race of men among whom nothing is considered a duty. Is there any race with whom there are not certain acts which the majority do, or refrain from doing, independently of any fear of punishment, but because they believe them to be right or wrong? And is there, on the other hand, any race of animals whose actions are influenced in the same way? We think the answer to these questions would show a positive distinction between man and the lower animals, which distinction would hardly be lessened by maintaining that the idea of duty so defined is in savages only the fear of punishment by gods or demons, since it will not be maintained that the lower animals are ever influenced by such motives.

Passing over a curious chapter on the utilisation of the lower animals, chiefly by educating and making slaves of the anthropoid apes, we come to the subject of language, which is discussed in a manner which exhibits the author's defects and merits in a remarkable degree. He thinks it necessary to approach the subject by a discussion on the fables of speaking stones and plants, of the sounds emitted by nudibranch molluscs and fish, the hissing of serpents, and the croaking of toads. By interpreting the notes of certain birds into words, such as "Whip-poor-will," and a number of others, he arrives at the conclusion that some savage languages have fewer letter-sounds than have those of certain animals. The physiology of voice and the construction of speaking automatons is next sketched, before we come to the really valuable part of the chapter, in which the variety of sounds and calls of several species of animals are detailed, and it is thus shown that they possess a language of no contemptible extent. He also maintains that animals understand, or rather learn to understand, the language of very distinct species. His dogs, for instance, perfectly understood his poultry. Cocks and hens have one danger signal for the approach of a bird of prey, another for that of a terrestrial animal or for man. The latter would rouse the sleeping dogs, who would instantly rush out and bark, while they took no notice whatever of the former. This proves that fowls have a language capable of expressing slightly different but closely connected ideas, and also that dogs soon learn the languages of other animals.

The subject of Hereditary Transmission is very imperfectly treated. M. Houzeau is evidently unacquainted with Mr. Galton's researches, or he would not have arrived at the conclusion that "a more or less complete transmission of the physical type with independence more or less absolute of the intellectual and moral type,—such is the law of observation, the law of nature."

In his last lecture on "Sociability" the doctrine is boldly advocated that of all animals Ants approach nearest to man in their social condition. They represent semi-civilised societies; while the highest Apes only represent the lowest savage state. The varied modes of association among the lower animals and in the human race are detailed with great fulness, but with little influence on the general argument. On the question of the affiliation of races, we find some good remarks on the comparative value of the useful arts, the food plants and domesticated animals, as compared with customs and superstitions. The evidence afforded by the latter is, he maintains, absolutely valueless unless supported by the former. In the concluding paragraphs of the work are some expressions and arguments which seem to show that the author is not an evolutionist, and has no clear ideas as to whether new species of animals are now coming into existence or not, or as to whether man has or has not originated from a lower animal form. This leads to an ambiguity and inconclusiveness in the whole work which contrasts strongly with the clear and definite views of such men as Darwin and Spencer, whose works lead us on by many and varied lines of research till they converge towards a grand and impressive conclusion. The present work cannot for a moment be compared with such as these; but it has special merits of its own, and it contains a mass of curious facts, acute observations, and sound reasoning, which fully entitle its author to take high rank among philosophical naturalists.

ALFRED R. WALLACE

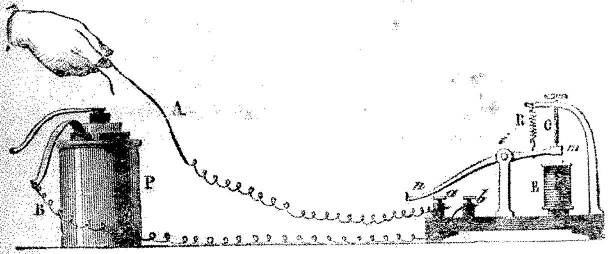
GANOT'S NATURAL PHILOSOPHY

Popular Natural Philosophy. By Ganot. Translated by E. Atkinson, Ph.D. (Longmans and Co.)

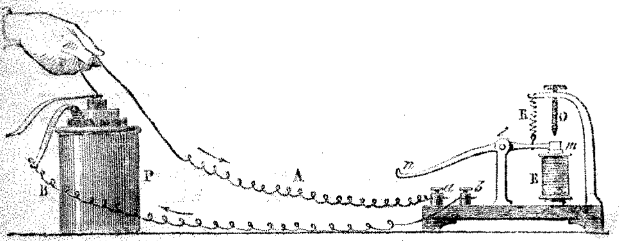
THIS is a good elementary book, giving the first principles of the subjects with which it deals in a clear and concise manner, with very few unnecessary words. The work is not an abridgment of Ganot's "Elements of Physics," but is founded on Ganot's "Cours Elémentaire de Physique," of which it is not a mere translation; but additions and alterations have been made by Dr. Atkinson, with the view of rendering it more fit to serve the purpose for which it is designed, namely, to act as a "text-book of physics for the middle and upper classes of boys' and girls' schools, and as a familiar account of physical phenomena and laws for the general reader." The book is very well adapted for these purposes. It is entirely free of mathematical formulæ, which, though but sparsely used in Ganot's "Physics," are still an insuperable barrier to the use of some portions of that work by the non-mathematical reader. The subjects treated of are the same as those in Ganot's "Physics," namely—the properties of matter, hydrostatics, pneumatics, acoustics, heat, light, magnetism, and electricity. The treatment of these subjects is, however, not only more elementary but somewhat less comprehensive than in the larger book. The engravings of the instruments and of the experiments detailed are good and suggestive, and calculated to be of assistance not only to the learner but to the teacher. There is, however, a good deal of what is superfluous in a considerable number of the illustrations, and a few of the illustrations themselves are unnecessary. It is, perhaps, over-refinement of criticism to object to the superfluity of embellishment in Fig. 140, in illustration of a speaking tube. (By the way, how exceedingly small the fire is!) Fig. 139 seems quite unnecessary in explaining the experimental determination of the velocity of sound by the Bureau of Longitude of Paris.

Although, however, there may be a good deal of what is superfluous in the illustrations, there is nothing which is misleading; but, on the contrary, they are in all cases calculated to leave a correct impression of the point in question on the mind of the reader. Fig. 349, however, of the aurora borealis hardly deserves such a favourable criticism.

As usual with almost all treatises on Natural Philosophy, we have to find fault with the character of the very few remarks which are devoted to terrestrial magnetism; but we know of no elementary book which deals with that very interesting subject sufficiently, and of hardly any which deal with it correctly.



The explanation of the principle of the telegraph is very lucid, and the diagrams connected therewith are exceedingly well adapted for enabling the learner to grasp the principle. The accompanying pair of diagrams in illustration of the principle of Morse's Telegraph leave hardly any verbal explanation necessary.



Diagrams of a self-explanatory character, and which, to a certain extent, stand in place of words, form a very useful feature in an educational text-book.

JAS. STUART

OUR BOOK SHELF

Jahrbuch der kaiserlich-königlichen geologischen Reichsanstalt. No. 1, 1872. Band xxii. (Wien.)

THIS part of the Year-book contains three papers devoted to the mining industries of Austria. In one of these—"On the Future of Mining in Austria," by Constantin Freih. v. Beust, we have concise and interesting sketches of the several mineral-bearing regions of Bohemia and Moravia, as also of the various rock formations of the Alpine districts which are metalliferous. The author comes to the conclusion that mining in Austria is capable of vast development, there being goodly stores of silver, lead, zinc, iron pyrites, and even perhaps of gold, which only require energy and enterprise to win them. The same writer contributes a second paper, "On the Direction of the principal Veins in the non-Hungarian Lands of the Austro-Hungarian Monarchy." Franz Ritter v. Hauer also gives some account of the ironstones worked by the Styrian Iron Company near Eisenerz. Dr. Emil Tietze has a long and able memoir on the geology and palæontology of the southern regions of the Banat mountains (Hungary). The descriptions of cretaceous and liassic fossils, many of which are of species new to science, and the illustrative plates that accompany the memoir, are well worthy the attention of palæontologists. In the *Mineralogische Mittheilungen*, edited by Prof. Tschermak, we have, amongst a number of other papers, one by Prof. Inostranzeff of Petersburg, giving the results of his examination of certain limestones and dolomites as bearing on questions of metamorphism. Prof. A. Exner, of Vienna, also contributes a "Chemical Examination of the Meteorites of Gopelpur." Other papers by M. Websky, A. Brezina, and F. Babanek, on mineralogical subjects, will serve to sustain the reputation acquired by our German friends in a department of science which has far too few votaries in this country.

The Metric System of Weights and Measures: an Address delivered before the Convocation of the University of the State of New York, at Albany, August 1, 1871. By Frederick A. P. Barnard, S.T.D., LL.D., President of Columbia College, New York City, &c. (New York, 1872.)

PRINCIPAL BARNARD was appointed, in 1871, by the Trustees of the University of the State of New York to attend a meeting of the Convocation of that University, who were adverse to the introduction of the Metric System, and to enlighten them as to its real nature, and the immense advantages that would flow from its adoption. He seems to have performed his duty with great ability, and we hope with equal success. This volume contains a revised edition of that address, with considerable additions in the form of notes and appendices. Principal Barnard gives a very lucid account of the origin and nature of the metric system, narrating the recent progress of meteorological reform, and answering with what appears to us unassailable arguments the objections commonly urged to its universal introduction into all civilised communities. One appendix contains a long, interesting and useful dissertation on the Unification of Moneys, with some well-arranged information on what has already been prepared and done. In another appendix he describes and discusses the various experiments which have been made to fix on a standard for measures of capacity. His third appendix is on the legislation of Great Britain and of British India in regard to the metric system; and his last appendix contains some very interesting, and what many will deem astonishing, statistics on the extent to which the system has been already adopted. From this we learn that France, Spain, Holland, Belgium (and their colonies), Portugal, Italy, the North German Confederation, Greece, Roumania, British India, and nearly the whole of the countries of Central and South America, have adopted the metric system in full; they represent a population of 336,419,595. Wurtemburg, Bavaria, Baden, Hesse, Switzerland, Denmark, Austria, and Turkey, representing a population of 84,039,209, have adopted metric values; while in Great Britain and the United States, containing a population of 70,373,091, the system is still permissive. In Sweden and Norway (population in 1867 5,897,159) the decimal division has been adopted without the metric values. Thus the peoples already decidedly enlisted on the side of the system include a total population of about 420,000,000. This looks hopeful, and there seems no doubt that this rational system of weights and measures will ere long be universally adopted. One very remarkable fact the author mentions in confirmation of this. At the close of last century, the simple measure of length called the foot had not less than *sixty* different values—probably many more—actually in use in different parts of Europe; in 1867, there could be found only *eight* of this discordant class surviving. We would recommend Dr. Barnard's book to all who wish to possess a clear and intelligible account of the system and its many advantages within a moderate compass.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Oceanic Circulation

As some of your readers may be misled by Mr. Croll's reiterated and uncontradicted assertions, that he has *demonstrated* the fallacy of the doctrine of Oceanic Circulation advocated by me, I think it right that they should be made aware that these assertions receive no support from the physicists and engineers, who must be much better judges than either Mr. Croll or I can be, as to the value of the *data* on which his computations are based His arithmetic may be perfectly correct; but if his fundamental assumptions are wrong, or inapplicable to the case, his demonstration utterly fails.

The report, now in the press, which I have presented to the Royal Society, as to the Deep-sea Researches, on which I was occupied during the autumn of last year, contains my reply to Mr. Croll's argument. And I profited by the unexpected delay in its publication to bring the principal points of that reply before the Mathematical and Physical Section of the British Association at its recent meeting, with the view of eliciting the opinion of the authorities there assembled, as to the soundness of my argument.

I do not think that I claim too much in saying that this opinion was given by Sir William Thomson, and other distinguished physicists, most explicitly in my favour.

Having also had an opportunity of bringing the question under the consideration of Mr. Hawksley, whose experience as a hydraulic engineer is probably second to that of no one living, I found him entirely of my mind; and Sir John Rennie has repeatedly expressed himself as altogether concurring with me.

So far as I know, therefore, Mr. Croll, in his reiterated assertion that water will *not* find its own level—for that, in plain English, is the position he takes—stands "alone in his glory."

The *facts* which I embodied in a paper presented at the same meeting to the Geological Section, in regard to the contrast of temperature between inland seas and the ocean with which they communicate, seem to me inexplicable on any other view than that of a *deep underflow* of Polar water towards the Equatorial area; and this necessarily involves, as its complement, an *upper flow* from the Equator towards the Poles.

When Mr. Croll shall have given some other *rationale* of these facts, he may fairly claim consideration for it. At present I venture to submit that all the *facts* at present known are in my favour; and that Mr. Croll's asserted refutation is purely *theoretical*.

October 4 WILLIAM B. CARPENTER

Consciousness and Volition

IN an interesting review of Lankester's "Comparative Longevity in Man and the Lower Animals," which recently appeared in the *Times*, is the following sentence:—"Once commenced, its continuance (that of the act of walking) is quite involuntary, and may even be unnoticed by the consciousness." I am anxious to ascertain, from those readers of NATURE who have paid special attention to psychology, whether this is, in their opinion, a correct statement of the condition of the mind during such an habitual act as that of walking. There are undoubtedly certain actions of the body, those denominated reflex, which are performed without any exercise of the will whatever; but these are all either momentary, or, if continued, are nearly uniform and unchanging. To me it seems impossible that any action which is constantly varying can be wholly involuntary. Take, for instance, the motion of the fingers in writing: we are absolutely unconscious of the exercise of the mental faculties by which each successive change in the position of the pen is regulated; but yet is it not certain that each up-stroke and down-stroke is the consequence of a separate effort of the will? There is here an evident connection between the state of mind at the time and the action of the body; and by what other means is it possible to suppose that the mental act which conceives the word we are writing can convey its instruction to the fingers? Certainly not by any process of instinct. A better instance perhaps is in the motion of the muscles of the face and throat in speaking. These muscles are entirely under the control of the will; and every separate motion of them must surely be effected by a distinct voluntary effort, of which however we are entirely unconscious. The same seems to me the case in walking. When we sway the body out of the perpendicular in turning a corner, I am at a loss to understand how this can be performed involuntarily. The explanation seems to me to be that consciousness cannot, so to speak, work so fast as volition, and therefore cannot take cognizance of a large number of rapidly successive acts of the will. The question is not so much one of a nice metaphysical distinction as simply of a correct use of terms, although I am afraid I am opposing the views of such high authorities as Huxley and Carpenter. We have been made familiar with the term Unconscious Cerebration. Is there not also an enormous field of Unconscious Volition?

London, Sept. 28 ALFRED W. BENNETT

Phosphorescence in Fish

I HAVE noticed the phosphorescence in fish on two occasions. Once on a calm night, wind light and sea smooth, in the S.E. trades, lat. 18° S., a shoal of porpoises was playing about under the bows of the ship, and darting under her keel for a space of nearly half an hour. Each looked like a piece of burnished silver on blue velvet. They presented the most beautiful appearance. We were not within "soundings." Every wavelet was covered with phosphorescence during the whole of that night, before and after the porpoises were seen. *They* manifestly could not have been the cause of this. As I leaned over the side and watched them, it seemed to me that their phosphorescence resulted from the condition of the water. Off the coast of South America, in about lat. 22° S., long. 30° W., weather much the same, a small shark accompanied the ship for some time, and presented the same appearance. The sea was brilliantly phosphorescent. The fish could be seen deep down. In both cases I saturated paper with the sea water. When dry the microscope failed to detect any organic matter, nothing but crystals.

11, Church Row, Hampstead ARTHUR NICOLS

On a Measuring Apparatus for Direct-Vision Spectroscopes

THERE are few who possess Browning's "miniature," or other small direct-vision spectroscopes, but must have felt the want of some means of measuring the positions of spectral lines; and, indeed, little useful spectrum work is possible without it. I think, therefore, a description of a simple arrangement I have used for some time for this purpose may prove useful.

In the "miniature" spectroscope the outer face of the prism is inclined to the axis of the instrument at an angle of about 40°. Opposite this a hole of about 2 mm. diameter is drilled in the sliding tube, care being taken to avoid injury to the prism. It is obvious that, through this aperture, lateral objects will be visible superimposed on the spectrum. If now a scale be set up opposite to the hole, it will be clearly seen, and the lines may be easily measured by it. The most convenient scale is one of transparent lines on a dark ground (photographed on glass from a scale drawn in black upon white paper), and illuminated by a lamp behind. A paper scale, preferably drawn in white upon black paper, may also be employed. In this case a common retort-stand clamp serves conveniently to hold the spectroscope, while the scale is laid horizontally on the table below. A sheet of paper, may be substituted for the scale, and the instrument used as a camera-lucida. Observation of faint spectra will be much assisted by shading extraneous light with black velvet, and covering the scale on paper with a black sliding screen, with an aperture through which only one or two divisions of the scale can be seen at once. A double scale, of which one gradation corresponds with the upper and the other with the lower edge of the spectrum, is also advantageous.

With a miniature spectroscope, and scale of millimetres at 25 cm. distance, the separation of Na and Li is about 13 divisions, and can easily be read to 0·2 mm.

The arrangement would, I think, be very applicable to the microspectroscope. The small hole is no inconvenience in ordinary use, and can easily be covered by the finger or by a small piece of black paper inserted in the tube.

In conclusion, I would urge the necessity of noting a sufficient number of reference lines at each observation, and the desirability of reducing measurements to the wave-length scale, for which purpose Dr. Watts' "Index of Spectra" will be found invaluable.

North Shields HENRY R. PROCTER

Cat's Toes

At the village of Cookham-Dean, near Maidenhead, there is a race of cats having more than their normal complement of toes. They generally have six toes on the fore feet, and the usual number on the hind feet; but I saw two individuals which had six toes on each foot, and others which had seven toes on the fore feet, and either five or six on the hind feet. The stock, as far as I can learn from the not over-bright natives, appears to have originated about seven or eight years ago in the person of a "Tom" having six toes on each of its feet. I should think there are now a score or more living in the village.

Harpenden, Sept. 19 R. Lydekker

NEW INSTRUMENT FOR THE PRODUCTION OF OZONE

In the *American Journal of Science and Arts* for July 1872, Prof. A. W. Wright, of Yale College, describes a simple apparatus for the production of ozone with electricity of high tension, and intended for use with the Holtz electrical machine. "The apparatus consists of a straight glass tube about 20 centimetres long and having an internal diameter of 2·5 centimetres, the two ends being stopped with corks covered on the inner side with a thin coating of cement to protect them from the action of the ozone. Through the axis of each cork is inserted a glass tube of about 5 millimetres calibre, and 7 centimetres in length, having a branch tube inserted perpendicularly at the middle, and long enough to permit a rubber tube to be slipped upon it. The outer ends of the tubes themselves are closely stopped with corks, through which are passed straight thick copper wires carrying suitable terminals at their inner ends, and bent into a ring at the others. They are fitted so as to make tight joints, but to allow of motion in order to vary the distance between their inner ends. One of these wires carries a small ball, the other terminates in a disc with rounded edge, set perpendicularly to the axis of the tube, and so large as to leave an annular space of some two or three millimetres breadth around it. The gas is admitted through one of the branch tubes, and escapes from the other after having passed through the whole length of the tube.

"In using the apparatus the wires must be connected with the poles of the machine in such a manner that the disc becomes the negative terminal, as this arrangement gives the greatest degree of expansion and diffusiveness to the current. On turning the machine, and adjusting the ball and disc to a proper distance, a nebulous aigrette surrounds the latter, quite filling the interval between it and the wall of the tube, while the part of the tube between the disc and ball is crowded with innumerable hazy streams converging upon the positive pole, or simply causing the latter to be covered with a faint glow. A current of air or oxygen sent into the tube must pass through this, and ozone is very rapidly produced, and in great quantity. The condensers are of course not used with the machine, when this apparatus is employed.

"The great quantity of the ozone, as well as the ease and rapidity with which it is produced, render the apparatus especially serviceable for use in the lecture-room."

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

If a good start in life is as serviceable for a society as for an individual, the French Association for the Advancement of Science must be considered as highly fortunate. There has already appeared in Nature a short account of its first meeting at Bordeaux, and of the papers read there; but the impressions of one of its invited guests may not be altogether without value or interest.

Confessedly the French was framed on the model of the British Association, and doubtless there was wisdom in that; but our friends across the Channel showed their wisdom also in making no servile copy, but endeavouring to modify our plans, so as to suit their national character or special requirements. The reception-room, the card of admission with a map of the town on the back, the various sections in the morning, the discourses in the evening, the municipal hospitality, all reminded us strongly of our own meetings; yet there were some differences that could not fail to strike an English visitor.

In the first place, it was not so popular an assembly. This arose partly from its constitution. There are two kinds of membership; there are the foundation members, who have qualified by taking one or more shares of 500 francs each, and subscribers who pay 20 francs for the meeting or a life composition. By enrolling these members a large society was created with a large capital before ever the first place of meeting was named. And very quickly was this accomplished; for it was only about Midsummer of last year that M. Friedel talked with M. Wurtz as to the best means of extending knowledge through the departments of France, and it was only last Midwinter that the project was nearly shipwrecked by the sudden and lamentable death of M. Combes, in whose rooms the first meeting had been held, and who had been named the provisional president; yet by the aid of large-hearted friends, such as M. D'Eichthal and M. Menier ("Chocolat-Menier"), the promoters of the movement were able to announce in April a sufficient capital to start with, and before the meeting at Bordeaux the Association numbered 700 members, and possessed 140,000fr.

No provision had been made for ladies' tickets, so when the meeting opened there was a sombre uniformity of black coats. But the English visitors brought ladies with them; there was a learned lady, who was believed to be writing for the press, and another, Madame Hureau de Villeneuve, followed her husband's paper on the Steam-engine by reading one of her own on the Flight of Birds. Encouraged by these, several other ladies made their appearance, and brightened the later meetings.

The accommodation afforded by Bordeaux was singularly good. The brilliant concert-room of the great theatre (which is historically interesting from the National Assembly having been convened in it during the German war) was given as the reception-room; and all the meetings were held in the Ecole Professionnelle, a large building just erected in a very substantial manner by the Philomathic Society, with funds bequeathed for the purpose. This new edifice contains a large lecture-room, which served well for the general meetings, and no end of good class-rooms, which accommodated the eleven sections into which the Association was divided. It is intended for the instruction of the working classes of the neighbourhood in the natural sciences, modern languages, drawing, &c., and so the sittings of the large scientific body were a good inauguration of its future work.

The great subdivision of the sections naturally gave rise to but small audiences in each. As far as I could judge, the chemists and the anthropologists were in greatest force; few naturalists or geologists of eminence were present. The sectional proceedings had more the character of a sitting of one of our learned societies than of a morning gathering at the British Association; but besides a couple of hours thus devoted to more abstruse points of science, there was an afternoon sitting at which subjects of more general interest were brought forward. This came intermediate in character, as in time, between the morning sections and the evening discourses; and it is a fair matter for consideration whether it might not be advantageously copied by us.

The papers seemed on the whole superior to those brought forward at our Association, at least there were fewer communications of trivial importance, or old subjects warmed up afresh. Our neighbours, however, find discussion a difficult thing; it is apt to degenerate at once into a conversation between two heated opponents. There was evident also a want of order, punctuality, and respect for authority; previous arrangements were altered, or the decisions of the chair set aside, in the coolest manner. A want of proper organisation arose from the fact that M. Claude Bernard, the president, never made his appearance on account of ill-health; but M. Cornu, the general secretary, was a host in himself; and as M. de Quatrefages has accepted the presidentship of the next meeting at Lyons, and Prof. Wurtz, whose energy and good nature were unfailing at Bordeaux, is to occupy the post of honour the year following, we may hope that the young Association will quickly get over the diseases of infancy.

Festivities were not wanting. Chief among these was a grand reception by the Mayor at the Hôtel de Ville, but I may specially mention a *déjeuner* given by the French chemists to their brethren from England and Belgium, Holland and Spain. Some of us also will never forget the private hospitality we met with.

But the excursions were the great feature of the meeting, and in them the copy certainly surpassed the original. They played a most important part in the proceedings; Saturday and Tuesday were wholly devoted to them; and they took place on Sunday, on Monday afternoon, and through the three days after the close of the sittings.

There was the expedition to Arcachon, where the *savans* not only strolled about the pretty watering-place, but studied natural history at an aquarium which, unlike that at Brighton, was a very unpretending building, but well stocked with interesting marine animals, and paid an especial visit to the oyster-beds that have been formed on sandbanks in the middle of the land-locked sea in front of the town. Here we traced the growth of the favourite mollusc from the spat on tiles, till it was large and plump, and we had explained to us the difficulties of its cultivation, and the ravages committed by a murex called *Carmaillot* (I am spelling at random) and by the hermit crabs. There was an antiquarian expedition to Perigueux and Les Eyzies, where, on each side of the valley, the limestone cliffs are fissured with caverns, in which men lived, and worked in flint and bone, at that remote period when reindeer and mammoths roamed over the soil of France. There was an expedition to see the new "docks" and huge engineering works of the Garonne, which M. Joly carefully explained; and there was a larger excursion by boat and rail down the Gironde to the open sea, where geologists had an opportunity of inspecting the cliffs of chalk and flint, and then the Tertiary strata, beside discussing the subsidence of the Gascon shore, and the shifting of the sandhills, and seeing how they are now prevented from swallowing up villages and churches as they did of old.

But the great excursion was the final one, which extended over three days, and was unique in my experience. Twenty members of the Association were officially deputed to report mainly on the industrial establishments of the Landes and Lower Pyrenees; and any other members were welcome to join the party. Thus was secured a good nucleus of really scientific men, while the expedition had a serious purpose, and it was evidently to the advantage of the establishments visited that we should be well received. The Landes, as is generally known, is a large tract of country which, until lately, was a marsh of sand scarcely capable even of affording pasture; but now it is reclaimed, and the centre of thriving industries. Forests of pine (*Pinus maritima*) have been planted for hundreds of miles, and the trees are regularly scored for turpentine; maize and other crops are grown; and the undergrowth of heather supplies food for myriads of bees. At Labouheyre we inspected the means employed for separating the rosin from the turpentine, and the machinery for impregnating the pine wood with sulphate of copper, so as to fit it for railway sleepers and telegraph posts; and though the thermometer was at 32° C. (say 90° F.) in the shade, and anything you like to imagine in the sun, we also went carefully over some blast furnaces that are used principally for reducing by charcoal the Spanish iron ores which, being free from sulphur and phosphorus, yield an excellent metal.

Here the party was sumptuously entertained by M. Alexandre Leon; and from this stage special trains or special carriages were placed freely at our disposal by the Compagnie du Midi. The next place visited was a primary school at Morcenx, for the gratuitous instruction of the children of their *employés*, which had been carried on by M. Surell the former, and M. Simon the present director of the Railway Company. We found the boys at military drill; we took part in the distribution of prizes, the Association itself giving a reward to the best boy and girl; and as scientific men, we were particularly interested in the good provision for "object lessons," the chemical and galvanic apparatus, and the care with which the children were taught the rudiments of physical and physiological science. Rejoicing at this proof that the reclamation of the Landes was not confined to the soil, we pursued our way to Dax, and spent Friday night at the Thermal Baths, where we enjoyed the hospitality of Drs. Larauza and Delmas, the physicians of the establishment. The springs of nearly boiling water that gush from many parts of the contorted strata under the town were duly examined, and so were the deposits of rock-salt that were accidentally discovered a year or two ago, and which promise to prove an important source of wealth.

From these hot springs we travelled southwards across the Spanish frontier to Irun, and then a good walk through beautiful scenery took us among the granite mountains to the mines of Bidassoa. Here we saw how large faults in the primæval rock are filled with crystallised carbonate of iron, and how the rich ore is won.

Returning into France, where, through the kindness of M. D'Eichtal, a dinner was awaiting us, the expedition found its way back to Bayonne; and, doubtless, on the morrow some of the party visited the pre-historic camp and the ancient abodes of the Troglodytes, according to the programme; but the chemists generally preferred a quiet day at Biarritz.

J. H. GLADSTONE

THE SPIRIT OF SCIENTIFIC CONTROVERSY

As if in atonement for a prolonged neglect, the study of the organisation of fossil plants is now receiving wide-spread attention. The task first undertaken by Henry Withan has now been shared by many observers. The result is that we already possess a much more complete acquaintance with the ancient vegetation of the globe than we did even a few years ago. But whilst this is undoubtedly true, it is equally so that wide differences of opinion on important points still exist amongst those who have taken a leading part in this investigation. Thus, M. Brongniart and Dr. Dawson believe that the Sigillariæ were Gymnospermous Exogens; whilst Mr. Carruthers and myself are convinced that they were Lepidodendroid Cryptogams. In common with Prof. Schimper and Mr Carruthers, I regard the whole of the Calamites as Cryptogamic plants, having Equisetaceous affinities; whilst M. Brongniart, M. Grand-Eury, and, perhaps partially, Dr. Dawson, deem some of them to be Equisetaceans, and others Gymnospermous Exogens. Mr. Carruthers and Mr. Binney regard the fruits known by the name of *Volkmannia Binneyi* to be the cones of Calamites. On the other hand, whilst I do not deny that such may possibly be their nature, I contend that we have neither proof nor even probable evidence sustaining this idea. Dr. Dawson says that Asterophyllites and Annularia are very distinct plants. Mr. Carruthers affirms that they are not. M. Grand-Eury and myself contend that Asterophyllites is wholly distinct, both in type and organisation, from Calamites. Mr. Carruthers believes that Asterophyllites and Annularia are alike the foliage of Calamites. It would be easy to multiply illustrations proving the existence of these opposite conclusions on important points amongst those observers who have enjoyed the best opportunities of forming reasonable opinions on such subjects. It is sufficiently obvious that some of us must be in error on these questions; possibly each of us is so in a greater or less degree; but when we regard the scientific status of the observers to whom I have referred, leaving myself out of the question, I ask, are they men whom we can accuse lightly of carelessness or ignorance? Must we not rather infer that each man has observed special facts leading him to his own conclusions, and that what we want is a careful comparison of such facts with those which have led our fellow-labourers in an opposite direction to ourselves? Whatever may be the explanation of these discrepant opinions, surely our mutual duties are clear. If any of us thinks that his fellow-labourer has made mistakes (and who has not) let him say so openly, and not suggest the idea by indulging in deprecatory insinuations. Let his opposing *argumentum* be *ad rem*, and not *ad hominem*. Further, let it be *ad rem* and not *ad alteram rem*. Let it not rest upon mere analogies, which may or may not be sound. Let us not reject a conclusion before we know all the facts from which it is drawn, merely because we think we have reason to deem it an impossible one. We have all lived to see many such conclusions take their places as undoubted truths.

One source of danger on these points, in the case of fossil botany, arises from the circumstance that though the ancient types of vegetation bear definite relations to the living ones, very remarkable differences present themselves in the combinations of the vegetative and reproductive organs in the two cases. Who, for instance, could have anticipated, from his knowledge of living plants, such an union of the usual vegetative organs of a cycad with an altogether anomalous reproductive system as we see in *Williamsonia gigas*. Many such examples will occur to those familiar with the subject. Hence, whilst a knowledge of living plants is absolutely indispensable to the student of fossil botany,—he cannot have too much of it—we must not allow our knowledge of recent combinations of vegetative and reproductive structures unduly to bias our judgment as to what may occur amongst fossil plants. Whilst we fully recognise the persistence of types, we must equally recognise the wonderful modifications which they have undergone in primæval times.

The conclusions to which these views lead me are very simple ones. The complex problems of palæophytology require harmonious and trustful co-operation amongst observers if truth is to be discovered. Let us supply this in the spirit of cordial fellow-labourers, and not as rivals in pursuit of a fleeting reputation which cannot be shared with others. We shall never raise ourselves by pulling others down. I will not quote the special expressions that are present to my mind whilst penning these lines; but it would be easy to do so, and to show that no possible benefits can accrue to science from their use. We can easily correct our mutual errors, we cannot so easily sooth wounded feelings, or restore shaken confidences. We aim at being the high priests of nature; let us try to banish all disturbing personal and selfish influences from the temple.

These words of warning may appear superfluous, because they embody mere truisms, equally applicable to every branch of human inquiry, or impertinent, seeing that in the fervour of earnest work, I may have erred in the same way as my neighbours. The fact is, that, like others of my earlier fellow-workers, I am rapidly approaching the autumn of life, and peace and harmony now appear more precious to me than they did in bygone years, when youthful ambition was alike active and inconsiderate. Happily ours are not pursuits which require us to cry *væ victis!* Just in proportion as we meet our opponents in a loving and harmonious spirit, without abandoning, in any degree, our earnest contention for what we believe to be truth, shall we, in our declining years, review our past labours with satisfaction and not with sorrow.—*Sic esto.*

W. C. WILLIAMSON

DISTRIBUTION OF HEAT IN THE SPECTRUM

PROF. J. W. DRAPER has communicated to *Silliman's Journal of Science and Art* for September a very important article under the above heading. After detailing a series of experiments on the distribution of the heat of the whole visible spectrum, of the more refrangible and of the less refrangible region, by rock-salt, flint-glass, bisulphide of carbon, and quartz, he thus sums up the results:—

"The important fact clearly brought into view by these experiments is, that if the visible spectrum be divided into two equal portions, the ray having a wave-length of 5768 being considered as the optical centre of such a spectrum, these portions will present heating powers so nearly equal that we may impute the differences to errors of experimentation. Assuming this as true, it necessarily follows that in the spectrum any two series of undulations will have the same heating power, no matter what their wave-lengths may be.

"But this conclusion leads unavoidably to a most important modification of the views now universally held as regards the constitution of the spectrum. When a ray falls on an extinguishing surface heat is produced, but that heat did not pre-exist in the ray. It arose from the stoppage of ether waves, and is a pure instance of the conversion of motion into heat—an illustration of the modern doctrines of the conservation and transmutation of force.

"From this point of view the conception that there exist in an incident ray various principles disappears altogether. We have to consider an incident ray as consisting solely of etherial vibrations, which, when they are checked by an extinguishing substance, lose their *vis viva*. The effect that ensues depends on the quality of the substance. The vibrations imparted to it may be manifested by the production of heat, as in the case of lamp-black, or by chemical changes, as in the case of many of the salts of silver. In the parallel instance of acoustics clear views have long ago been attained, and are firmly held. No one supposes that sound is one of the ingredients of the atmosphere, and it would not be more incorrect to assert that it is something emitted by the sounding body than it is to affirm that light or heat, or actinism, are emitted by the sun.

"The progress of actino-chemistry would be greatly accelerated if there could be steadfastly maintained a clear conception of the distinction between the mechanism of a ray and the effects to which that ray may give rise. The evolution of heat, the sensation of light, the production of chemical changes, are merely effects—manifestations of the motions imparted to ponderable atoms—and these in their turn can give rise to converse results, as when we gradually raise the temperature of a substance the oscillating movements of its molecules are imparted to the ether, and waves of less and less length are successively engendered."

SCOTTISH BOULDERS

THE first Report of the Committee appointed to collect statistics as to boulders, has been recently issued by the Royal Society of Edinburgh, and contains much that is interesting both to the geologist and archæologist. The first object of the committee, and that to which their labours have as yet been solely directed, has been to ascertain the districts in Scotland where any remarkable boulders were situated. Their second object will be to select those which might be deemed worthy of preservation, with the view of requesting landed proprietors and tenants of farms not to destroy them. The committee sent out a printed list of queries, applicable to boulders apparently above 20 tons in weight, one of the queries being directed to ascertain the occurrence of "kaims" or "eskars," *i.e.* long banks of sand or gravel. The following are some of the most important results educed by the inquiry.—

"1. From a tabular list we learn that Aberdeenshire possesses the largest number of boulders, and also the boulders of greatest magnitude. Ross and Cromarty stand next, then Perth, Argyll, Inverness, Kirkcudbright, and Forfar.

"2. In regard to size, the largest boulder reported is one of granite, in the Parish of Pitlochry, called 'Clach Mhor' (big stone), being about eight yards square, and estimated about 800 tons. There are two boulders between 500 and 600 tons weight, one in Ross, the other in The Lewis. There are three boulders, between 200 and 500 tons, seven between 100 and 200 tons, twenty between 50 and 100.'

"3. With regard to the nature of the rocks composing the boulders, the largest reported are of granite, though there is one known to the convener of the committee, still larger, of conglomerate, in Doune parish. The most numerous are composed of compact greenstone; but these are generally of small size. The next most numerous class are of grey granite. There are also many of gneiss, graywacke, and conglomerate.

"4. The boulders reported generally differ in regard to the nature of the rocks composing them from that of the rocks of the district in which they are situated; and, in many of the reports, reference is made to the district from which the boulder is supposed to have come. Thus, in those parts of Perthshire, Forfarshire, and Kincardineshire where the Old Red sandstone formation prevails, and over which multitudes of granite, gneiss, and conglomerate boulders are lying, most of the reporters have no hesitation in pointing out that the parent rock is in the Grampian range, lying to the north or west. So also in Wigtonshire, where the graywacke formation prevails, and on which many boulders of grey granite are lying, the general opinion is that they came from the granite hills of Kirkcudbrightshire.

"5. The boulders mentioned in the reports are of various shapes. Some approach a cube, well rounded of course on the corners and sides. That is the shape mostly possessed by granite boulders. Others again are of an oblong shape, and this is particularly the case with whinstone and graywacke boulders. A point of some importance occurs in regard to oblong-shaped boulders. The direction of their longer axis, in the great majority of cases, is stated to coincide with the direction in which they have come from the parent rock, when the situation of that rock has been ascertained. Thus in Auchterarder parish, there is a boulder 10 ft. long by 6 broad, the longer axis of which points north-west. In Auchtergaven parish there is a granite boulder 10 ft. long by 8 ft. broad, the longer axis of which points due north. In Memuir parish, Forfarshire, there are two large granite boulders, the one 14 ft. by 9 ft., and the other 13 ft. by 9 ft., the longer axis of which points north-west. In each of these cases the reporters seem satisfied of the situation of the parent rock, and in each case the longer axis of the boulder points towards it. It appears, also, that where there are natural striations or ruts on the boulders, these almost always run in a direction parallel with the longer axis; and that when there are striæ crossing these the number of such oblique striæ is comparatively few.

"6. Notice in the reports is taken of the remarkable positions occupied by some boulders. Thus, the Ardentinny report refers to a large boulder called 'Clachan Udalain,' or the nicely balanced stone, so called, as the reporter states, because 'it stands on the very edge of a precipice, and must have been gently deposited there.' On Iona, near the top of the highest hill in the island, which is about 250 feet above the sea, there is a great boulder of granite. There is no granite in the island. The nearest place where that rock occurs is in the Ross of Mull, &c., with an arm of the sea intervening.

"7. With regard to kaims or long embankments of gravel or sand, there are twenty-three parishes reported to the Committee as containing them. They appear to be most numerous in Aberdeenshire, Forfarshire, and in the east of Perthshire. In Kemnay parish there is a kaim said to be 2½ miles long, running east and west. In Airlie parish there is a kaim 2 miles long, also running east and west. In Fettercairn parish, Kincardineshire, and also in Tarbet parish, Ross-shire, there are several kaims parallel to, and not far distant from, one another."

The committee proceed next to notice points of archæological interest connected with boulders, and are surprised at the large number of them possessing names by which they are known in the districts to which they belong. The names may be classified under several heads:—First, there are names having reference to the agency by which the boulders were supposed to have come into the district. Second, there are names indicative of the use to which boulders were put. Third, there are names making the boulders commemorative of certain events. Fourth, some boulders form such prominent landmarks that they have been used to mark the boundaries of estates, parishes and counties, and are still in many parts of Scotland recognised as affording evidence on that subject. On these points the committee give some very curious information which must be highly interesting to archæologists, and indeed to all who take an interest in the history of the race.

Great numbers of boulders have legends attached to them, one of the commonest being that the boulder was thrown to the spot where it lies by some giant, demon, or even by "Auld Nickie Ben" himself, for some wicked purpose of course; and it is very interesting to notice, that almost invariably, the place from which the legend says the huge stone was thrown, is the nearest spot containing the formation to which the boulder belongs. It is well known that, as a rule, boulders differ from the formation on or in which they are found, and in reference to what we have just mentioned, the place from which the giant or devil took his throw is often at a very considerable distance, sometimes on a different island. For example there is a large conglomerate boulder near the top of a hill, in the island of Edag, one of the Orkneys, which goes under the name of the "Giant's Stone." The legend says it was flung by a giant from the island of Stronsay; now there is no conglomerate rock which could have supplied the boulder in Edag, though there is in Stronsay.

The British Association at its last meeting so highly approved of the scheme of the Royal Society of Edinburgh, that it appointed a committee of some of its most influential geologists to carry out a similar scheme for England and Ireland.

The committee are very anxious that the boulders reported on should be examined by experienced geologists, who may be visiting the districts where they are situated, and are willing to lend the reports they have received on condition that the results of the inspection be made known to the committee.

ON THE FERTILISATION OF A FEW COMMON PAPILIONACEOUS FLOWERS

[NOTE TO EDITOR.—The enclosed paper was written in the autumn of 1869, and then submitted to Mr. Darwin. With his usual kindness he encouraged me to proceed with it; and with his usual thoroughness he advised me to make it more complete than it is before giving it to the public. At the same time, he lent me various publications containing articles on the subject of fertilisation, and, amongst others, some by the Italian botanist, Delpino, who has done so much in this field. I found that he had in two or three publications in the years 1867 and 1868, anticipated most of the observations contained in the accompanying paper; and I proposed to myself to attempt a *résumé* of what had been done of late years in the matter of fertilisation of flowers by Delpino, Hildebrand, and others. But this, though a labour of love, is a greater labour than I can manage, and other calls have grown upon me. I therefore send the paper to you as it stands, begging that this note may be prefixed in order that I may not be thought to be appropriating Delpino's observations.—T. H. F., *October* 1872.]

AFTER reading Mr. Darwin's book on Orchids and his papers on Lythrum and Primula, I made some notes on the fertilisation of Phaseolus and some of the Campanulaceæ, which had the good fortune to meet with his approval, and which he had the kindness to send for publication to the Annals and Magazine of Natural History, where they appeared in October 1868. The comparison of Phaseolus with other Papilionaceous flowers led me to think that Mr. Darwin's fertile ideas might receive many illustrations from the structure and functions of this beautiful and interesting tribe; and the following are observations made during the summer of 1869 upon a few of the commonest of them. I am painfully conscious how imperfect want of time, of opportunity, and of knowledge has left them; and how many points there are, even in these few flowers, which require a much more careful inquiry. Indeed, every new flower has its own peculiarity; and almost every new peculiarity suggests the observation of facts in other flowers not previously noticed; so that the task is endless. Again it is difficult to feel sure of a conclusion unless the whole process of fertilisation by insects can be watched, and to a dweller in towns, ignorant of insects and their habits, it is impossible. If, however, these observations should lead to further inquiry and discussion, they may not be useless. The flowers in question are *Pisum sativum*, several species of *Lathyrus*, *Vicia*, and *Phaseolus*, *Robinia pseud-Acacia*, *Wistaria*, *Onobrychis sativa*, *Trifolium repens* and *T. pratense*, *Lotus corniculatus*, *Lupinus*, *Ononis*, *Anthyllis*, *Ulex*, *Genista*, *Sarothamnus*, and *Cytisus*.

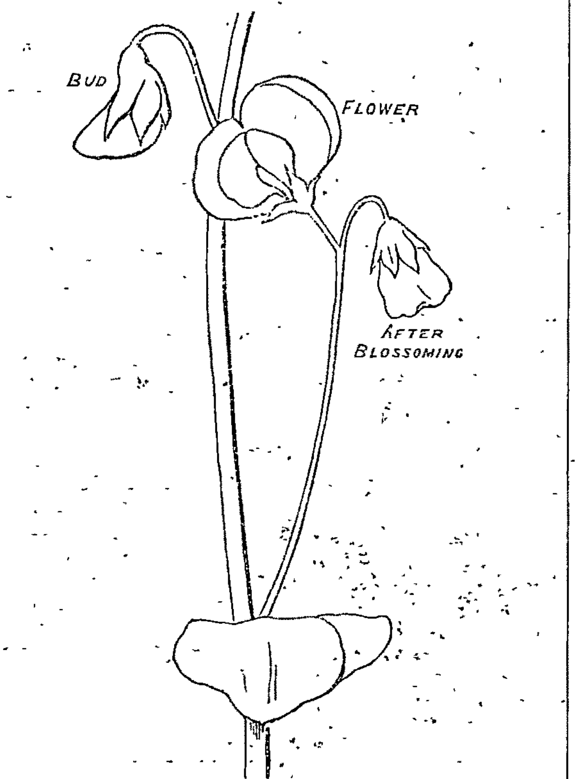


FIG. 1.—*Pisum sativum* (Common Pea) (peduncle and pedicels).

Pisum sativum, or Common Pea.—The blossoms are generally two upon a common peduncle, and each flower has a separate short pedicel (see Fig. 1). The peduncle generally approaches the perpendicular and maintains its position through the stages of bud, blossom, and pod, except that it gets stiffer. The short pedicels, however, change their position twice. In the bud they are bent down so that the base of the calyx is uppermost, and the upper edge of the folded vexillum lowest. In this stage the large calyx covers with a weather-proof awning the tender blossom. As the flower opens the pedicel straightens itself; and when the blossom is fully open it is quite straight, and at an angle of 45° to the peduncle. The effect of this is to raise the flower so that the keel and wings become almost horizontal, whilst the showy limb of the vexillum, bent upwards from the claw, displays a perpendicular face (see Fig. 2).

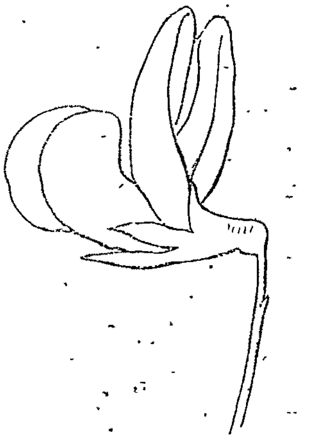


FIG. 2.—*Pisum sativum* (mature flower).

The wings are slightly attached to the keel at the base of their limbs; and the limbs project outwards and a little upwards in front of and above the keel, so as to make an excellent lighting place for insects. The keel is boat-shaped, recurved at the apex, and the lower edges

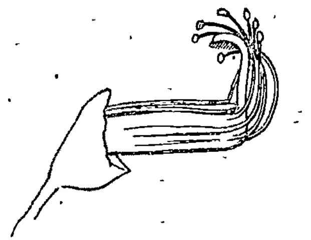


FIG 3.—*Pisum sativum* (lateral view of pistil and staminal tube, with calyx and corolla removed, and tenth stamen separated).

are joined together from the base to the apex. The stamens are diadelphous, the filament of the tenth stamen being separate at the summit and base, and separable in the middle (see Fig. 3). They are of nearly equal length, the pollen is abundant and rather moist, and is shed at the time the blossom expands. The upper parts of the filaments are stiff enough to keep their place, but not so stiff as the style. The lower parts of the filaments form a stiff tube, expanded towards the base, so as to leave a large cavity round the base of the ovary. This cavity is abundantly supplied with nectar. On each side of the tenth stamen at its base, there is a wide aperture, through which apertures, on removing the vexillum, this cavity with its nectar is easily seen (see Fig 4).

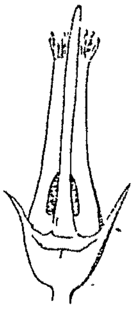


FIG 4.—*Pisum sativum* (front view of staminal tube, with tenth stamen, front showing apertures into nectary on each side the tube).

The style is at right angles to the horizontal ovary, and curves towards the vexillum at the top. The stigma is at the extremity, and faces outwards and upwards towards the vexillum. On the inside for some distance below the stigma it is clothed with stiff hairs, which are set so as to point upwards towards the stigma (see Fig. 5). The style appears to be formed by two folds of the carpellary leaf, which bend outwards from the point where the style joins the ovary, so that the outer side or back of the style which lies towards the suture of the keel, and which has no hairs on it, is formed, not of the outer suture of the carpel, but of the edges of these folds.

At the time the flower opens the stamens have shed, or are shedding, their pollen, which lies in an abundant mass at the apex of the keel around and above the stigma.

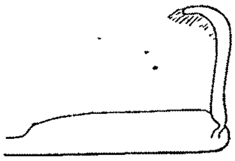


FIG. 5.—*Pisum sativum* (pistil).

The back of the stiff elastic style almost touches the keel; and on pressing down the wings, which, as above noticed, are attached to the keel, the back of the style, which has no hairs, is pressed against the keel, whilst the brush on the front and sides of the style sweeps the moist pollen upwards and pushes it out of the apex of the keel and against any object which is entering the flower, and to which the pollen, being moist, will adhere. On removing the pressure the parts take their place again, whilst on repeating the pressure the same process may be repeated, until the whole of the pollen in the upper part of the keel is brushed out.

As soon as the flower closes and before it withers, the pedicel again droops, the flower becomes pendent, and the calyx again acts as a pent-house to the young pod (see Fig. 1).

Now, undoubtedly, the stigma of one of these flowers is always covered with its own pollen; but if self-fertilisation were the rule, the elaborate structure I have described is meaningless, whilst if the purpose is that insects shall carry the pollen from flower to flower, it becomes a curiously elaborate and complete piece of mechanism having a special object. The change of position of the flower by the bending, straightening, and second bending of the pedicel, so that the tender opening bud and the young fertilised ovary are protected from rain and cold; whilst the open blossom displays itself in the most attractive and convenient form and position for insects; the conspicuous vexillum; the wings, forming an alighting place; the attachment of the wings to the keel, by which any body pressing on the former must press down the latter; the staminal tube inclosing nectar, and affording by means of its partially free stamen with apertures on each side of its base, an open passage to an insect seeking the nectar; the moist and sticky pollen placed just where it will be swept out of the apex of the keel against the entering insect; the stiff elastic style so placed that on a pressure being applied to the keel, it will be pushed upwards out of the keel; the hairs on the style placed on that side of the style only on which there is space for the pollen, and in such a direction as to sweep it out; and the stigma so placed as to meet an entering insect,—all these become correlated parts of one elaborate mechanism; if we suppose that the fertilisation of these flowers is effected by the carriage of pollen from one to the other.

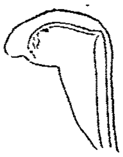


FIG 6.—*Lathyrus* (keel and pistil).

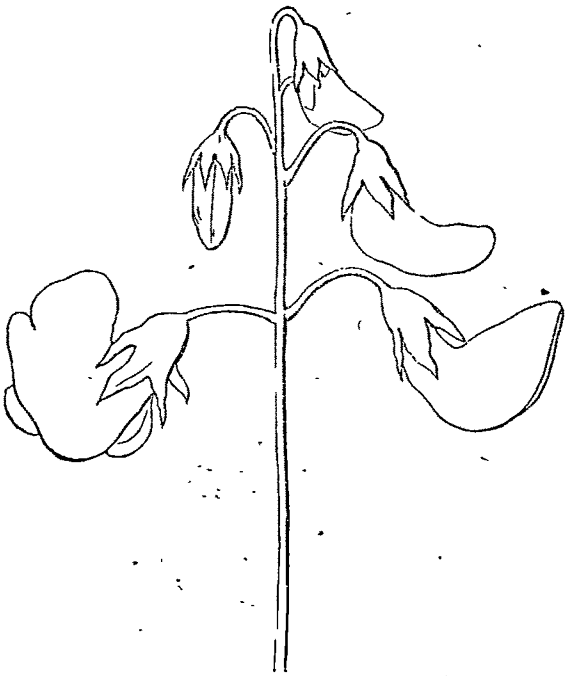


FIG. 7.—*Lathyrus latifolius* (Everlasting Pea).

I have, however, not observed the bees or other insects at work on these flowers, whilst they are to be found in abundance on the neighbouring broad beans and scarlet runners. Do the white pea-blossoms attract night-flying insects?

Lathyrus odoratus.—This is, so far as the above functions are concerned, so like Pisum, that it is scarcely worth while to dwell on the differences. In colour and smell, of course the difference is great, and consequently in the attractions for different insects. The changing position of the pedicels; the brush to the style; the free or partially free stamen, and the nectar inside the case of the staminal tube, and the openings into that tube, are the same (see Fig. 6).

Lathyrus macrorhizus, is, so far as I have observed it, similar.

Lathyrus pisiformis is like the other Lathyri in the above points, except that in the long raceme of flowers, the whole peduncle, and not only the pedicels of the separate flowers, is pendent in the bud. It stiffens and becomes upright as the blossoms open, and the pedicels also stiffen and become horizontal. After flowering the peduncle remains stiff and upright, but the pedicels droop.

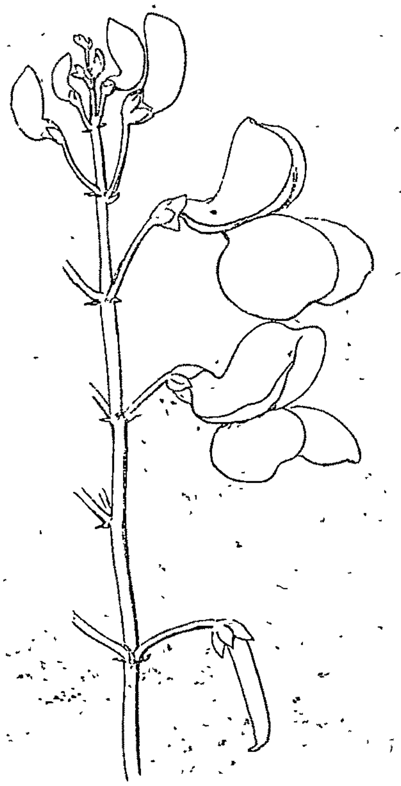


Fig. 8.—*Phaseolus multiflorus* (Scarlet Runner).

Lathyrus pratensis.—The fertilising apparatus is the same as in the above Lathyri.

Lathyrus sylvestris, or *latifolius*, or Everlasting Pea.—Here the many-flowered raceme is itself upright, whilst the pedicels bend, straighten themselves, and again bend, as in Pisum (see Fig. 7). In the fertilising apparatus the mechanism is the same as above described, with the exception that there is a very decided obliquity in the keel and in the style, though not so decided as in the following.

Lathyrus grandiflorus.—In this flower the peculiarities, as distinguished from the above-mentioned Lathyri, are as follows:—The pedicels bend, straighten, and bend themselves again, as above mentioned; but the effect generally, if not universally, is to make the large showy vexillum, and not the keel, horizontal. The vexillum, consequently, and not the keel, would be the natural alighting place for an insect. The wings are at right angles to the vexillum, and the recurved point of the keel projects between them and overhangs the vexillum. An insect alighting on the vexillum, and thrusting itself towards the nectary, must push the wings, and with them the keel, upwards, and make the style and the pollen come out; but they will come out downwards on to the back of the insect, and not on to his thorax or belly.

Another peculiarity is that the keel, and with it the style, is very oblique, and the upper part is flattened, and is twisted so that the back of the style does not press against the keel. Correlatively both sides of the style are well furnished with hairs, and both sides equally operate in sweeping the pollen out of the keel. In this respect *Lathyrus grandiflorus* seems to show a gradation towards Phaseolus.

Phaseolus multiflorus, or Scarlet Runner.—In the position of the blossom whilst flowering, in the nectar-holding cavity of the staminal tube, and in having an entrance to the cavity by the separation of the tenth stamen, this flower resembles Lathyrus and Pisum, but it offers the following peculiarities (see Fig. 8):—

The pedicel of the bud before flowering is perpendicular and stiff, and the bud consequently upright; the pedicel becomes rather more horizontal as the flower opens, and in blossom the wings are horizontal, whilst after flowering the pedicel becomes quite horizontal, and the pod gradually sinks, and ultimately becomes pendent. Correlatively, there is no large pent-house of a calyx, as in Pisum, to protect the young blossom; but the same object seems to be effected by the smooth, strong, thick vexillum, the edges of which are in the bud closed valvately over the tender folded interior petals; whilst in the bud of Pisum the whole of the petals, whilst sheltered by the calyx, are tender and unclosed. The young pod of Phaseolus, also unlike the thin glabrous pod of Pisum, is thick and covered with short hairs.

The keel, which in some Lathyri is very oblique, is in Phaseolus so twisted, and has its edges so joined, as to form an imperfect tube containing the stamens and style; it makes with them nearly two complete spiral turns, and its mouth points obliquely downwards. The stiff, elastic style is clothed with stiff hairs or bristles, forming a circular brush at the point in the tube where it is surrounded by, and in contact with, the moist, sticky pollen of the mature anthers. The stigma is on the lower side of the style, just appearing at the mouth of the tube, is sticky, and is clothed with fine hairs.

The filament of the tenth stamen is entirely separate from the others, and is furnished with a sort of tooth or appendage on the outside, upon pressing which the stamen is drawn back, and free access is given to the nectar-holding cavity. On the other hand, there is no such aperture on each side of the base of the tenth stamen as there is on each side of the base of the semi-adherent filament in Pisum and Lathyrus.

A bee lighting on the wings, or rather on the outer wing, opens for himself a way to the base of the flower. At the same time, the wing being attached to the spiral tube of the keel, he pulls it outwards, the consequence of which is that the stiff style is thrust outwards and at first downwards, so that the sticky stigma first touches the entering insect and sweeps from his proboscis any pollen he may have brought from other flowers. As he passes further, the stigma protrudes further, turns upwards, and the brush of the style, loaded with the sticky pollen of its own anthers, sweeps against, and leaves its load on, his proboscis, with which he departs for other flowers.

The mechanism of this flower is truly wonderful. For further details I could refer to papers by Mr. Darwin in the *Gardener's Chronicle* of October 24, 1857, and November 14, 1858, and to the notes of my own in the *Annals and Magazine of Natural History*, October 1868.

Phaseolus vulgaris is similar to *P. multiflorus*.

T. H. Farrer

(*To be continued.*)

NOTES

Dr. T. Sterry Hunt, F.R.S., for more than twenty-five years an officer of the Geological Survey of Canada, has resigned his position there, and goes to Boston to the Massachusetts Institute of Technology, where he is to fill the chair of Geology, left vacant by the resignation of Prof. William B. Rogers. He was to enter upon his new duties on October 7.

The examination which has been held in common by Magdalen and Merton Colleges, Oxford, for scholarships in Natural Science, terminated on Saturday, when the following elections were made:—To demyships at Magdalen College, value 95*l.*, for five years: Mr. W. J. P. Wood, Clifton College; Mr. C. H. Wade, The Owens College, Manchester. To a postmastership at Merton College, value 80*l.*, for five years: Mr. W. H. Jones, Derby.

The centenary of Linnæus's death will be celebrated at Stockholm on the 10th of January, 1873, when a statue of the great Swedish naturalist will be unveiled.

The Meteorological Congress will meet next year at Vienna, and the meeting will be a very important one, for which the one recently held in Leipzig was only a preparation; the condition of the Continental observatories and of the large British ones will then form a grave subject for discussion.

We learn from *Les Mondes* that the construction of meteorological observatories on the summit of the Puy-de-Dôme is now about to commence. Two observatories, connected by a telegraphic wire, the one in a pavilion of the faculty at Clermont, the other on the summit of the mountain, 1,160 metres above the lower one, will show every moment the difference of meteorological condition between the plain and the upper regions of the atmosphere. The money allotted to this purpose is 100,000 francs, one half being furnished by the town and department, the other half by the State.

The Commissioners of Her Majesty's Works and Public Buildings intend to distribute this autumn, among the working classes and the poor inhabitants of London, the surplus bedding-out plants in Battersea, Hyde, the Regent's, and Victoria Parks, and in the Royal Gardens, Kew. If the clergy, school committees, and others interested, will make application to the Superintendents of the Parks nearest to their respective parishes, or to the Director of the Royal Gardens Kew, in the cases of persons residing in that neighbourhood, they will receive early intimation of the number of plants that can be allotted to each applicant, and of the time and manner of their distribution.

The Annual Fungus Exhibition was held in the Council Room of the Royal Horticultural Society, on Wednesday the 2nd inst. There were several extremely good collections, both of the edible and poisonous kinds, many of the rarer edible species being well represented, while some of the commoner ones were almost entirely absent. Prizes for the best collections were given by Mr. W. W. Saunders, F.R.S.

An exhibition of useful insects and their products, and also of noxious insects, with samples of the injuries caused by them, organised by the Central Society of Agriculture, and under the patronage of the Minister of Agriculture and Commerce, will be held during this month in the Luxembourg Gardens in Paris. It will comprise silkworms and their cocoons of every species, with samples of thrown and raw silk; apparatus for silk culture; with the manufacture and raw product of bees, and apparatus for bee culture; a collection of noxious insects, and apparatus suited for their destruction; other useful insects; collections of mammals, insectivorous birds, and reptiles, &c. The programme of the exhibition may be obtained of the Secretary of the Society of Agriculture, 59, Rue Monge, Paris.

The *Journal of Botany* records the death, after a short illness, of a well-known Continental botanist, Andreas J. Oersted, Professor of Botany in the University of Copenhagen. He had travelled and collected largely in Central America, and had written much, especially on cryptogamic botany.

Dr. Eichler, of Gratz, editor of Martius's "*Flora Brasiliensis*" has accepted the appointment of Professor of Botany at Kiel, Holstein. No change will be involved in the publication of the great Flora.

Aquaria seem to be quite the rage at the present time. A new large aquarium is to be built at Great Yarmouth on the north beach. A space of seven acres has been granted by the Corporation a short distance to the left of the Britannia Pier, and here, in addition to the aquarium, gardens will be laid out, and a museum and gymnasium built.

The fine new Guildhall Library is almost completed, and will be open free to the public very shortly. In addition to the Library proper, the basement will be used for carefully storing the old charters and records belonging to the Corporation, and a handsome room, 80 ft. long by nearly 50 ft. wide, will be devoted to the Museum, and it is hoped will become a receptacle for all the objects of antiquarian interest found in the City of London. Above the Museum is the Library proper, a noble room, 120 ft. long by 50 ft. broad, and nearly 60 ft. in height. The open oak roof will be highly ornamented and enriched, and the windows will be filled with stained glass, the two chief windows being presented by the London and Middlesex Archæological Society, and by the inhabitants of the Ward of Aldersgate.

The first session of the New University College of Wales at Aberystwyth was to open to-day (Thursday). The educational staff will at present consist of the Principal, a Professor of Classics, a Professor of Mathematics and Natural Philosophy, and a Teacher of Modern Languages. Arrangements will be made as soon as practicable for lectures in Geology, Chemistry, and other cognate subjects. The Principal is the Rev. Thomas Charles Edwards, M.A., to whom application for admission must be made. An examination will be held at the beginning of the session, when several exhibitions of 20*l.* each will be awarded.

We have received the prospectus of The Owens College, Manchester, for the session 1872-73. The most important new arrangement is the separation of Geology and Palæontology from Natural History, and the establishment of a separate Chair, which is filled by Mr. Boyd Dawkins, F.R.S. Animal Physiology and Zoology, and Vegetable Physiology and Botany, remain under the charge of Prof. W. C. Williamson, F.R.S. The Manchester Royal School of Medicine is incorporated with The Owens College.

The courses of lectures in connection with the Franklin Institute for the winter session 1872-73, to be held in the Hall of the Franklin Institute, Philadelphia, will comprise Experimental Chemistry, Mineralogy, Metallurgy, Hygiene, Physics, Technical Chemistry, Fire, and Photography.

It is proposed to incorporate the courses of evening lectures on scientific subjects delivered at the Polytechnic Institute into an institution to be called the Polytechnic College. They are largely attended by young men, many of them engaged during the day in shops.

The following scientific lectures will be delivered in connection with the Newcastle-upon-Tyne Literary and Philosophical Society during the session 1872-73. "The Progress of Solar Research," by J. N. Lockyer, F.R.S., Oct. 21 and 23. "New Illustrations of Divine Contrivance in Nature," by Prof. S. Haughton, M.D., Oct. 28 and 30. "Water; its Nature, Circulation, and Functions," by Prof. D. Page, LLD, Nov. 4 and 6. "The Life, Character, and Work of Faraday," by Dr. J. H. Gladstone, F.R S., Nov. 18 and 20. "On Stellar Astronomy," by Prof. R. Grant, Dec. 6 and 13. "Oil Coals, Oil Shales, and Oil Wells," by A. Taylor, Dec. 16, 18, and 20. "On Polarized Light," by W. Spottiswoode, F.R.S., January 29, and 31, 1873. "Early Moral and Political Condition of Mankind," by E. B. Tylor, F.R.S., Feb. 5 and 7.

Lectures will be delivered during the ensuing session in connection with the Oldham Literary and Philosophical Society, on the following subjects:—"Star Depths," illustrated by Oxy-hydrogen Lantern and Photographic Sides, by R. A. Proctor. "Flame," illustrated by experiments, by Prof. J. H. Core. "Balloon Ascents for Scientific Purposes," by James Glaisher, F.R.S. "The Origin of the British Flora." by L. C. Miall, "The Caverns of Devonshire," by W. Pengelly, F.R.S. "Sand, Gravel, and Clay; or, an Arctic Climate in Britain," by J. E. Taylor. "Folk-Lore of Natural History," by Robt. Holland. This will be the second session, the first having been decidedly successful.

The distribution of the prizes and certificates in the Guildford Science Classes took place on September 20, under the presidency of Lord Midleton. The classes have been very successful during the past year, the members under instruction in the various subjects amounting to seventy-seven, against forty-four in the previous year; and the proportion of certificates awarded being larger than the average of all England. Guildford is showing in the institution of these classes an example that might well be followed by other towns whose opportunities are greater. We are sorry, however, to learn that up to the present time the committee has entirely borne the brunt of the elementary expenses, not one sixpence having been contributed by the public of Guildford during their three years' existence. This is not as it should be. Probably in time our country towns will learn that their own interest is involved in cultivating a knowledge of science.

The *Astronomical Register* for October contains the first instalment of an article on astronomical allusions in Homer, Dante, Shakespeare, and Milton, Homer and Dante being treated of in the present number with great care and fulness of knowledge. The subject is an interesting one, and we believe Mr. G. J. Walker's treatment of it will be of service towards a history of the progress of man's knowledge of the heavens.

From the same periodical we learn that the new dome for the Edinburgh Observatory, alluded to in a recent number of Nature, is now erected, and admirably fulfils all the expectations formed of it, such as increased space inside, greater ease of revolution, larger and more easily worked shutter, better ventilation, and freedom from vibration and bumping. This latter curious quality was a very vicious propensity of the old dome, partly from its being mounted on cannon balls, which enabled it to roll in every other direction as well as in that of the line or railway, wherein it was wanted to roll; and partly from the unprecedentedly windy and stormy exposure of the Edinburgh Observatory on the summit of Calton Hill. The new dome, therefore, may be considered a very creditable piece of engineering, and a decided success on the part of Mr. Howard Grubb.

We learn from the *British Medical Journal* that 300 young Russian women have claimed admission as students in medicine and surgery at the newly opened Medical School of St. Petersburg. The number of admissions being fixed, however, at 70, there will be a great many disappointed.

The Volcano of Santorin, when last visited in October 1871, had ceased giving the small eruptions which had been common almost without intermission since the great eruption of 1866, and the summit of the crater, covered with great blocks of lava, presented the same appearance as in 1707. A little steam was still escaping, but this seemed due almost entirely to the vapour of water condensing on the cinders covering the cone. In the north the fumerolles were still active, and all around the stones were covered with sulphur. At the S.E. point the volcanic activity had not completely ceased, but had greatly diminished. All this would show that the eruption had entered on its last stage, and after a period of great central activity in 1866-67; accompanied by a diminution of activity in 1869-70, it is now again assuming a condition of rest and quietude.

The completion of the Australian Land Telegraph is a great scientific feat, for by it London is now within a few hours' communication with Adelaide and all the other centres of population on the Australian continent. Whilst the Suez Canal, the Mont Cenis Tunnel, and the Pacific Railway, are undoubtedly great and stupendous works, the carrying of a line of telegraph across the uninhabited and almost unknown interior of Australia for 1,800 miles is a great result, especially when performed single-handed by the colony of South Australia. Some interesting discoveries have been made during its progress. The river Roper has now been ascertained to be a noble river, and the only one in Australia navigable for large sea-going steamers and ships for 100 miles from the sea, and some apparently very productive gold fields have also been found.

Auroræ Boreales have been very conspicuous lately. On the 25th of August one was seen at Thurso and at Hernosand in the Gulf of Bothnia and feebly also at Lisbon.—On the 26th at Sévres and also at Stockholm.—On Sept. 2, at Sévres, Stockholm, Reval and Windaw.—On Sept. 3, at Sévres, Paris, Hernosand, and Rome.—On the 4th, at Sévres, Paris, London, and Archangel.—On the 5th, at Sévres and Paris.—On the 6th at Hernosand. So that there have been in Europe, during the period from the 2nd to the 6th of Sept., an almost continuous succession of Auroræ visible in England, France, Scandinavia, Russia, and once in Italy.

A White Aurora Borealis was observed at Baumette near Angers, on the 8th of August, by M. A. Cheux, who thus describes it.—About 10 o'clock the sky was lit up in the N.N.W., by a white light; and at successive intervals white rays were shot out mostly towards the North or North-West. At about 10·30, a magnificent white ray shaped like a fan, and 22° in height, occupied the northern part of the sky. This gradually faded, and by 11·30 the sky had resumed its natural colour. This is the fourth Aurora Borealis observed in the last four months, the others having been seen on the 9th of May, the 9th of June, the 10th of July, and this on the 8th of August. The coincidence in these monthly dates is something remarkable.

Prof. O. C. Marsh, of New Haven, has been diligently at work during the past summer in elaborating the rich mass of fossil vertebrates collected by him last year and the year before in various parts of the West. His latest discoveries are two large pachyderms allied to *Palæosyops*, a gigantic fossil tapir called *Hyrachyus princeps*, two carnivora allied to the *Viverridæ*, and, most important of all, two species of bats (*Nyctitherium velox* and *N. priscus*), the first of the order ever detected fossil in America.

SIEBOLD'S NEW RESEARCHES IN PARTHENOGENESIS *

AMIDST the all-absorbing discussion of the problems which have arisen out of the general acceptance among biologists of the law of evolution, the phenomenon of Parthenogenesis which, previously to Mr. Darwin's work on the Origin of Species, excited the interest and called forth the investigations of observers in much the same manner as his theory has done of late years, has met with a reverse of fortune and fallen into a subordinate rank of popularity. The distinguished naturalist, however, who fifteen years ago gave so stunning a blow to current theories of the reproductive process, by demonstrating the occurrence in moths and bees of what he designated as "true parthenogenesis"—that is to say, the development, without impregnation, of an ovum capable of being impregnated—has not let the subject drop. Professor Siebold has made further experimental researches, establishing again, and on a larger basis, his former conclusion, and adding at least one new fact of great general importance for the understanding of the process of sexual reproduction. Although upon its first appearance in 1856, the conclusion arrived at in his "Wahre Parthenogenesis" was admitted by almost all competent naturalists to be thoroughly demonstrated, and beyond the reach of criticism; yet some more and some less eminent biologists have not been wanting to deny the *Lucina sine concubitu*, and have raised such objections as that of a possible error in the condition of the experiments depending on the exclusion of males from the supposed parthenogenetic female; and again, that these so-called females were not demonstrated "not to be hermaphrodites." Indeed so deeply rooted is the conviction that eggs are made to be impregnated by spermatozoa, and that they then, and then only, can proceed to develop, that Siebold felt it necessary to add to his proofs, in order to establish his position that not only do unimpregnated eggs develop into perfect animals, but that such an event is by no means an exceptional occurrence among certain groups, and has a definitely fixed and orderly recurrence amongst them. He naturally was also anxious to extend the class-limits within which a true parthenogenesis can be said to occur, and he desired to inquire into the sex of the parthenogenetically-produced offspring in such cases as could be critically and decisively studied. Hence the renewed researches which have extended over several years, and the results of which are given in the present brochure.

Von Siebold's merit in this and his former work (but more especially in this) is not the enunciation of a new theory, or hypothesis, but the great care, ingenuity, and persistence which he has displayed in investigating cases in which for many years collectors, bee-keepers, and such naturalists of the limited, or "gardener" type, had asserted reproduction by means of unfertilised eggs to take place. It must be remembered that he was himself a strong opponent in 1850 of the supposition which he has now shown to be justified in fact, and that Leuckart in his article "Zeugniss," in Wagner's Handworterbuch, and in other publications, preceded him as an advocate for the existence of true parthenogenesis, endeavouring, by microscopical researches, to give a solid observational basis to Dzierzon's hypothesis. It was not until 1857 that Siebold published his observations on bees, demonstrating what had been previously supposed, viz., that the queen-bee exhausts her store of received sperm in fertilising eggs which give rise to females only, and that then she lays unfertilised eggs, which become drones only, whilst the unfertilised worker-females also lay eggs, which give rise to drones, and again that in certain moths (*Psyche* and *Solenobia*) unfertilised ova develop and produce females only. Leuckart followed (1858) with his "Zur Kenntniss des Generations-wechsels und der Parthenogenesis bei den Insekten." In this work, whilst asserting his claims to the merit of first espousing the cause of true parthenogenesis, Leuckart gives an excellent view of the general signification of the phenomena, and insists on the importance of extended histological observation in the examination of alleged cases of parthenogenesis. In his present work Siebold cannot be charged with in any way neglecting this part of his subject, for he has given most important and minute descriptions of the generative organs of the two principal cases studied (*Polistes* and *Apus*), containing new facts. His method is however eminently experimental, and appears to us a striking contradiction of a very superficial classification of the sciences, which is favoured sometimes by men of science unacquainted with the methods or problems of biology: we mean the division into the exact or mathematical, the experimental, and the classificatory sciences, in which last division the so-called natural history sciences are said to find their place.

The experiments which Siebold made on bees and wasps, though performed by a naturalist, are as nicely controlled, and as clear in the conclusions which they give, as any performed by exact physicists on the times or quantities concerned in this or that physical process. The style in which details of these investigations are communicated is one rare at the present day in biological works, where minute description of structure, or of the apparatus devised for a physiological research, form the staple. Here we are treated to a leisurely narrative of some years of patient work; we share the keen enjoyment of the author as he becomes acquainted with the marvellous intelligence of his wasps and their various proceedings—we feel his satisfaction in overcoming the difficulties of procuring and observing the necessary material, and admire the candour and thoroughness with which he handles the question before him.

Before proceeding to a short notice of the contents of Von Siebold's book, it will be well to give a brief statement of the signification which such inquiries as his have in the present state of knowledge. Harvey's dictum, "Omne vivum ex ovo," expressed a great law, which had to be qualified when the researches of Trembley and others made known, among Polyps, and Worms, and Protozoa, reproduction by fission. To this rapidly succeeded the recognition of a modified fission, in which the animal did not divide into equal parts, nor exhibit the power of reproduction of the whole animal in artificially detached portions of its body; but in which special sprouts or buds were found to be prepared and detached spontaneously, becoming then developed into perfect animals. This process received the name of gemmation, and was stated to occur in polyps and also in the plant-lice. Parallels for these methods of reproduction in animals were readily recognised in plants, in the multiplication by seed, by cuttings or shoots, and by separable buds. A broad line was drawn between "buds" and "eggs," however egg-like the former might appear, in the assumption that eggs were special bodies of a peculiar structure, destined to be "fertilised" by the spermatozoa of the male—after which process only could they develop. These distinctions, some twenty years ago, were the more firmly impressed in the minds of biologists by the then recently acquired knowledge of the process of fertilisation or impregnation. Then came the demonstration by Siebold of the capacity for development of true eggs, even when not impregnated. The sharpness of the limit between buds and eggs was by this at once destroyed; and the closely following researches of Leydig (antecedent to Siebold's work in some cases), Huxley, Lubbock, and Leuckart, on the structure of the supposed buds of Aphis and allied insects, and of lower crustaceans, proved that these bodies were morphologically ova—originating in ovaries, and having the essential structure of fertilisable ova. For them the term "pseudova" was introduced by Prof. Huxley, since they differ in this respect from other ova—that whereas the latter can be, and are in most cases (though with constant exceptions), fertilised, the latter cannot be.* Whilst, then, up to this period such a thing as parthenogenesis appeared to be a strange exception, the question has now shifted; and, since the essential identity in reproductive power of cuttings, buds, pseudova, and eggs is proved, the problem before naturalists is rather, "Why are eggs ever fertilised?" in short, "What is the use of the male sex at all?" We have animals and plants multiplying by fission, breaking up into two or more parts, each of which becomes a new individual; we have them giving rise by growth to masses of cells, which become detached or remain attached, and develop each into a new individual; and finally, we have them elaborating single large cells, which become detached and develop each into a new individual. Why should it be that in certain cases these last require the fusion of another peculiar kind of cell elements before they will develop? Some light seemed to be thrown on this matter at first, by the observation that the unfertilised ova of the bee always produce drones, and that only the fertilised produce females; but this indication of a possibly clearer insight into the matter is entirely upset by the fact, now fully established in the present work, that in some species of insects and crustaceans the unfertilised ova always, or in an enormously large proportion, produce females only; whilst in the Aphides we know that they ultimately produce both males and females. Mr. Darwin has suggested the most satisfactory theory of fertilisation, in assigning to it the object of fusing two life-experiences in the progeny, which thus gains tendencies and acquires impulses from a wider area than does an unfertilised ovum, and is in so far strengthened. Conjugation of two cells, similarly formed but belonging to different individuals (as seen in Confervæ and Gregarinæ) is the simplest arrangement for obtaining this end; the only difference between this and sexual reproduction is that in the latter process one cell seeks, the other is sought; and this differentiation into active and passive, the wooer and the wooed, commencing in the simplest vegetable and animal cells, persists to the highest rank of development. Self-impregnation (if it have a real physiological existence) and parthenogenesis, have, then, to yield, as chief modes of reproduction, to digenesis, or the concurrence of two individuals; and this for one and the same reason. Perhaps the apparently anomalous facts that an animal—or plant, as the case may be—develops elaborate motile zoosperms and copulatory organs, merely to fertilise its own egg; and that other animals and plants develop peculiarly constructed large cells, of a kind apparently especially elaborated in the progress of the general evolution of life, to provide for fertilisation, yet which never are fertilised—are only to be explained as cases of persistent structures with modified function. In the former case, Monogenesis, being sufficient to or necessitated by the conditions of life, yet avails itself of the apparatus inherited from digenetic ancestors; whilst similarly, in the second case (pseudova), Monogenesis, having advantages for the particular case (and not being a common phenomenon in the group), instead of making its appearance through new organs, avails itself of the ovary inherited from digenetic progenitors. Thus the unsignificant form of an ovum (unsignificant, that is, so far as monogeny is concerned) takes the place of the more obviously appropriate bud or fisssion-product. The phenomenon of Alternation of Generations, usually treated of in connection with parthenogenesis, should by experiment on the physical conditions accompanying its variations enable us to ascertain a great deal more than is at present known of what is the signification of the differentiation of male and female sexual elements; and it is from further study of this and of True Parthenogenesis that progress in this part of physiology may be expected.

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* "Beitrage zur Parthenogenesis der Arthropoden." Von C. Th. E. von Siebold, Professor der Zoologie und Vergleichenden Anatomie in Munchen. Leipzig: Engelmann, 1871.)

* Leuckart has more recently proposed, in describing the reproduction of the Cecidomyia larvæ discovered by Wagner, to limit the term "pseudovum" to such ova as those produced by larvæ, or imperfect forms; and not to apply it at all to the eggs of bees, wasps, &c. (which can develop without fertilisation), as was done by Huxley. The falsity implied in the prefix seems to make a rather stronger distinction than is desirable between any of these bodies; for they are all truly ova, though ova of various special properties.

To return to Siebold's researches. The greater part of the book is devoted to an account of the parthenogenesis observable in the wasp Polistes. Leuckart first recorded in his work already mentioned, that the workers of wasps, ants, hornets, and humble bees lay eggs, which in one case he followed to the development of a larva, but of which he was not able to determine the sex. Siebold determined to study a species of Polistes common in and around Munich, which he identifies with much care, and after reference to specimens and authorities from many lands, as *Polistes gallica* var. *diadema* Latreille. He gives a minute description of the characters of the females and males; the two kinds of the former (queens and workers) being only distinguishable by size—the workers in all external characters as well as in their generative organs being merely smaller queens, and fully capable of copulation and impregnation. In the beginning of May, in Munich, the Polistes queens which were born in the previous summer and impregnated then, commence each to build a nest. No queen who built in the former year survives to build a second time, and the young queens never make use of the old nests. The Polistes are very particular in choosing a warm sunshiny spot, sheltered from wind and rain, and as such spots are not too common, a new nest is often begun near the weathered remains of an old one. Walls and trunks of trees, often at such a height as not to be easily reached, are the sites chosen. When the queen has constructed fifteen or twenty cells, she lays eggs in them, and is very hard-worked in guarding her nest and in providing food for the larvæ as they hatch. She feeds them on caterpillars and other soft insects, first removing the alimentary canal (as cooks take out the entrails of a fish), and carefully applying the morsel to the lips of each larva. This process takes some time for one "hand," and hence the first brood is longer in coming to perfection than later broods, in the rearing of which the elder progeny assist. In the middle of June the first lot make their appearance, all small females ready to assist their parent in the advancement of her enterprise. The later broods develop more rapidly and acquire larger size from being better nourished, and towards the end of June (no drones being as yet born) the females which come forth are as large as the old queen; they may, however, be easily distinguished from her by their comparative freshness of colour and wing. Great care is displayed in guarding the nest. At night the queen goes to sleep after having carefully inspected each cell, taking up her position at the hinder side of the nest. If she is disturbed in the night, she takes another survey of her house before again going to sleep. In the daytime if disturbed she makes an immediate attack on the enemy with her sting, and then flies back to her nest. She can sting several times, since the barbs on her weapon are not too long to allow of its being withdrawn. Ants are amongst the most common of the many insect enemies of these wasps, and when one ventures into the nest, the whole colony sting it to death, and immediately throw the body out. Birds are, however, not thus to be got rid of, and destroy immense numbers of the nests, so that Siebold was obliged to protect those he wished to study with nets. The members of one nest are not allowed to remain in another, if by chance a stranger comes in she is hustled out at once by the wasps near the entrance. *Siebold* convinced himself of this by painting the thorax of a number of Polistes belonging to different nests with different colours. Only late in the year, when the wasps seem to be getting careless or tired of their incessant work did he find that one or two had got mixed in certain colonies, to which they did not rightly belong. Although there is this sharp discrimination of individuals, yet it was found that by substituting one nest for another whilst the queen was away, she could often be deceived, so as to make her enter upon the possession of the substituted nest as though it were her own. Siebold found this a very useful plan when he wanted to change the position or locality of a nest so as to bring it into a safer or more accessible spot, or when a nest which he had been observing was by some accident deserted, or when a nest in a favourable position was less forward in the development of its larvæ than one less favourably situated. By making the nests moveable, and substituting the one for the other in the absence of the queen, he was able to save himself much trouble and loss. The nests were made moveable by removing them from their original support and firmly fixing them to boards which were then hung up in the original position. The queens were very anxious after this operation had been performed, investigating with great care the strength of the support and the cord by which the board was hung, and sometimes adding to it themselves additional strength. By degrees such moveable nests could be lowered a little bit each day from an inconveniently high position, or taking the nest in the night under a cover whilst all the wasps were in it, it could be removed from a distant locality to the Professor's garden; in such cases a certain proportion always came to grief by the desertion of the colony; and the queen was then sometimes found at work on the old site constructing a new nest. Although strangers are not admitted in a well-regulated Polistes nest, yet by carelessness or desertion the brood of one colony will sometimes be exposed to the attacks of the workers of another, who then make use of the unfortunate larvæ to feed their own young. It frequently happens that workers who have once indulged in this kind of thing, become what are called "robber wasps," utterly demoralised, and actually undo the whole labour of a colony by dragging out the grubs which they were lately so carefully tending in common with their fellows, to feed the still younger larvæ. When this condition of things has once begun in a colony it soon goes to ruin, and hence it is necessary to destroy any deserted Polistes nests in the neighbourhood of those under observation, lest by entering the former the members of the latter should get the bad habit of pulling grubs out of their cells, and proceed to do the same in their own nests.

The rain is a very constant source of destruction to Polistes colonies, drowning the young by saturating the cells with moisture. Light rain will not, however, do much harm. Whilst Siebold was endeavouring to remove some of the water from a nest which had been drenched in a shower, he was astonished to find the wasps themselves already busy at the work, putting their heads into the cells, sucking up the water, then passing to the edge of the nest and spitting out the fluid. In this way they are able to get rid of the effects of a wetting if it is not very severe. Though the Polistes feed their young exclusively with animal food, they yet appear to collect a sweet fluid which Siebold found in some cells, and which he thinks the workers take for their own enjoyment, since they were seen entering such cells and apparently sucking at the contents—in fact, taking a little refreshment in the intervals of their labour.

The development of the grub is carefully described by the author, and a "pseudo-nymph" stage is recognised intervening between the nymph and the pupa. The perfect insect bites off the lid of its cell, and comes out with perfect wings, deposits first of all a drop of urinary excretion, and makes a trial flight, then returns to take part in the labours of the colony. The cell is often used again for another egg. The first drones make their appearance with the beginning of July, an important fact for Siebold's experiments, for if the nest is to be used at all now is the latest moment; they have to be killed off, and all the remaining larvæ and pupæ destroyed—in order to secure a colony consisting solely of virgins. The drones play a pitiable part in the nest—sneaking about in the empty cells and behind the comb, not till the month of August are their generative organs fully developed, and then they make their first approaches to the females. Their proceedings are minutely described, and it appears that they meet with many rebuffs from the busily-occupied workers of the hive, and that it is outside at a distance from the nest that their addresses are at length accepted by those of the larger females destined to become queens. Not all the large females appear to have this destiny, and none appear to leave the nest until all the brood has been brought through, when (about the beginning of October) the nests become deserted. Only a few flattened old virgin wasps remain, who are killed off by the frosts, whilst the young queens have married and sought out for themselves winter quarters. Siebold distinguished black-eyed and green-eyed drones, and speculates upon the signification of this difference.

Having ascertained these and other matters relating to the Polistes in far greater detail than we have been able here to indicate them, Siebold was prepared to make his experiment. In the nest from which he wanted an answer to these questions, "can unfertilised Polistes females lay eggs which will develop?" and if so, "of what sex will the parthenogenetically produced progeny be?" he proceeded to destroy the queen and all the eggs, larvæ, and pupæ in the cells with the greatest care as late as possible in the season, so as to have as large a colony as possible left, the limit of the time being given by the date of the appearance of the first drone. The queens thus taken were used for careful histological study of the generative organs, and since in all cases Siebold found the *receptaculum seminis* filled with moving spermatozoa, he was able to feel assured that he had really removed the queen in each case. We will merely direct the attention of those interested in histology to the minute description here given of the ovary, which in the main agrees with Leydig's, and of the *receptaculum seminis*, which in opposition to Leydig, on account of its nerve supply, Siebold holds to be contractile. After waiting some days Siebold had the gratification of finding the first eggs laid in the cells of several of the nests from which he had removed queen, eggs, and larvæ, and he felt convinced that they could only have been laid by the small virgin workers who alone tenanted the combs. The whole affairs of the colonies proceeded just as well as when the queens were there, and the virgins watched and worked with the same assiduity as had done their queen-mother. In some cases Siebold actually saw a worker deposit an egg, and such egg-laying workers, when anatomically tested, showed, firstly, in the presence of *corpora lutea* (the precise signification of which the investigator had ascertained by his histological studies of the ovary) that eggs had been extruded, and, secondly, in the complete absence of spermatozoa from the *receptaculum seminis*, that the insect was a virgin. Out of a hundred nests which he had begun to observe in one season, and out of one hundred and fifty in another, only some twenty or so in each case came through all the long series of accidents from weather, insects, birds, &c., to which they were necessarily exposed, and some of those which promised the best results and had cost the most pains came to a bad end in the very last days of the research. In order to determine the sex of the wasps born from the eggs laid by the parthenogenetic females, Siebold in most cases only allowed the development to proceed sufficiently far to enable him to recognise the sex by anatomical investigation. The dried skin, however, of such grubs as were found dead in their cells afforded sufficient evidence of the sex. In all cases the parthenogenetic offspring was without exception male. The queen-wasps as we have mentioned also late in the season lay eggs which produce drones, which are easily distinguished from the drones parthenogenetically produced by their larger size. It occurred to Siebold when he first ascertained that the queens produce drones that such drones might visit his virgin colonies, and thus his whole experiment be invalidated. He was, however, reassured on this point by a nearer acquaintance with the Polistes; for such drones are not born till later than the period at which his small females laid their eggs, the former event taking place at the end of July, the latter at the beginning; and, furthermore, as we have noticed above, it is not till still later (August), when the experimental cells were long since all occupied with eggs, that the power and desire of sexual activity comes to these drones.

E. R. LANKESTER

(*To be continued.*)

ON INSTINCT[*]

WITH regard to instinct we have yet to ascertain the facts. Do the animals exhibit untaught skill and innate knowledge? May not the supposed examples of instinct be after all but the results of rapid learning and imitation? The controversy on this subject has been chiefly concerning the perceptions of distance and direction by the eye and the ear. Against the instinctive character of these perceptions it is argued that, as distance means movement, locomotion, the very essence of the idea is such as cannot be taken in by the eye or ear; that what the varying sensations of sight and hearing correspond to, must be got at by moving over the ground by experience. The results, however, of experiments on chickens were wholly in favour of the instinctive nature of these perceptions. Chickens kept in a state of blindness by various devices, from one to three days, when placed in the light under a set of carefully prepared conditions, gave conclusive evidence against the theory that the perceptions of distance and direction by the eye are the result of associations formed in the experience of each individual life. Often, at the end of two minutes, they followed with their eyes the movements of crawling insects, turning their heads with all the precision of an old fowl. In from two to fifteen minutes they pecked at some object, showing not merely an instinctive perception of distance, but an original ability to measure distance with something like infallible accuracy. If beyond the reach of their necks, they walked or ran up to the object of their pursuit, and may be said to have invariably struck it, never missing by more than a hair's-breadth; this, too, when the specks at which they struck were no bigger than the smallest visible dot of an *i*. To seize between the points of the mandible at the very instant of striking seemed a more difficult operation. Though at times they seized and swallowed an insect at the first attempt, more frequently they struck five or six times, lifting once or twice before they succeeded in swallowing their first food. To take, by way of illustration, the observations on a single case a little in detail:—A chicken at the end of six minutes, after having its eyes unveiled, followed with its head the movements of a fly twelve inches distant; at ten minutes, the fly coming within reach of its neck, was seized, and swallowed at the first stroke; at the end of twenty minutes it had not attempted to walk a step. It was then placed on rough ground within sight and call of a hen, with chickens of its own age. After standing chirping for about a minute, it went straight towards the hen, displaying as keen a perception of the qualities of the outer world as it was ever likely to possess in after life. It never required to knock its head against a stone to discover that there was "no road that way." It leaped over the smaller obstacles that lay in its path, and ran round the larger, reaching the mother in as nearly a straight line as the nature of the ground would permit. Thus it would seem that, prior to experience, the eye—at least the eye of the chicken—perceives the primary qualities of the external world, all arguments of the purely analytical school of psychology to the contrary, notwithstanding.

Not less decisive were experiments on hearing. Chickens hatched and kept in the dark for a day or two, on being placed in the light nine or ten feet from a box in which a brooding hen was concealed, after standing chirping for a minute or two, uniformly set off straight to the box in answer to the call of the hen which they had never seen and never before heard. This they did struggling through grass and over rough ground, when not able to stand steadily on their legs. Again, chickens that from the first had been denied the use of their eyes by having hoods drawn over their heads while yet in the shell, were while thus blind made the subject of experiment. These, when left to themselves, seldom made a forward step, their movements were round and round and backward; but when placed within five or six feet of the hen mother, they, in answer to her call, became much more lively, began to make little forward journeys, and soon followed her by sound alone, though of course blindly. Another experiment consisted in rendering chickens deaf for a time by sealing their ears with several folds of gum paper before they had escaped from the shell. These, on having their ears opened when two or three days old, and being placed within call of the mother concealed in a box or on the other side of a door, after turning round a few times ran straight to the spot whence came the first sound they had ever heard. Clearly, of these chickens it cannot be said that sounds were to them at first but meaningless sensations.

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[*] Paper read before the British Association, Section D (Department of Zoology and Botany), August 19th, by D. A. Spalding.

One or two observations favourable to the opinion that animals have an instinctive knowledge of their enemies may be taken for what they are worth. When twelve days old one of my little *protégés* running about beside me, gave the peculiar chirp whereby they announce the approach of danger. On looking up, a sparrow-hawk was seen hovering at a great height over head. Again, a young hawk was made to fly over a hen with her first brood of chickens, then about a week old. In the twinkling of an eye most of the chickens were hid among grass and bushes. And scarcely had the hawk touched the ground, about twelve yards from where the hen had been sitting, when she fell upon it, and would soon have killed it outright. A young turkey gave even more striking evidence. When ten days old it heard the voice of the hawk for the first time, and just beside it. Like an arrow from the bow it darted off in the opposite direction, and crouched in a corner, remained for ten minutes motionless and dumb with fear. Out of a vast number of experiments with chickens and bees, though the results were not uniform, yet in the great majority of instances the chickens gave evidence of instinctive fear of these sting-bearing insects.

But to return to examples of instinctive skill and knowledge, concerning which I think no doubt can remain, a very useful instinct may be observed in the early attention that chickens pay to their toilet. As soon as they can hold up their heads, when only from four to five hours old, they attempt dressing at their wings, that, too, when they have been denied the use of their eyes. Another incontestable case of instinct may be seen in the art of scraping in search of food. Without any opportunities of imitation, chickens begin to scrape when from two to six days old. Most frequently the circumstances are suggestive; at other times, however, the first attempt, which generally consists of a sort of nervous dance, was made on a smooth table. The unacquired dexterity shown in the capture of insects is very remarkable. A duckling one day old, on being placed in the open air for the first time, almost immediately snapped at, and caught, a fly on the wing. Still more interesting is the instructive art of catching flies peculiar to the turkey. When not a day and a half old I observed a young turkey, which I had adopted while yet in the shell, pointing its beak slowly and deliberately at flies and other small insects without actually pecking at them. In doing this its head could be seen to shake like a hand that is attempted to be held steady by a visible effort This I recorded when I did not understand its meaning. For it was not until afterwards that I observed a turkey, when it sees a fly settled on any object, steals on the unwary insect with slow and measured step, and, when sufficiently near, advances its head very slowly and steadily until within reach of its prey, which is then seized by a sudden dart. In still further confirmation of the opinion that such wonderful examples of dexterity and cunning are instinctive and not acquired, may be adduced the significant fact that the individuals of each species have little capacity to learn anything not found in the habits of their progenitors. A chicken was made, from the first and for several months, the sole companion of a young turkey. Yet it never showed the slightest tendency to adopt the admirable art of catching flies that it saw practised before its eyes every hour of the day.

The only theory in explanation of the phenomena of instinct that has an air of science about it, is the doctrine of Inherited Association. Instinct in the present generation of animals is the product of the accumulated experiences of past generations. Great difficulty, however, is felt by many in conceiving how anything so impalpable as fear at the sight of a bee should be transmitted from parent to offspring. It should be remembered, however, that the permanence of such associations in the history of an individual life depends on the corresponding impress given to the nervous organisation. We cannot, strictly speaking, experience any individual act of consciousness twice over; but as, by pulling the bell-cord to-day we can, in the language of ordinary discourse, produce the same sound we heard yesterday, so, while the established connections among the nerves and nerve-centres hold, we are enabled to live our experiences over again. Now, why should not those modifications of brain-matter, that, enduring from hour to hour and from day to day, render acquisition possible, be, like any other physical peculiarity, transmitted from parent to offspring? That they are so transmitted is all but proved by the facts of instinct, while these, in their turn, receive their only rational explanation in this theory of Inherited Association.

ON THE TREE-FERNS OF THE COAL MEASURES, AND THEIR AFFINITIES WITH EXISTING FORMS *

LINDLEY and Hutton describe two species of tree-ferns from the Coal Measures, both from the Bath Coal-field. I have been able to add eight species hitherto undescribed, chiefly through the assistance of Mr. J. M'Murtrie, of Radstock. These belong to three groups, which are remarkably distinguished by peculiarities in the structure of the stems. Two of the groups belong to living forms, while the third is extinct, being confined to Palæozoic formations. *Caulopteris* and *Tubicaulis* belong to the same type as the living ferns which possess stems, including under this term the humble stems (falsely called rhizomes) of many of our British species, as well as the arborescent ferns of warmer regions; and excluding the rhizomatous forms like *Pteris*, *Polypodium*, and *Hymenophyllum*. In all these stems we have a central medulla, surrounded by a continuous vascular cylinder penetrated regularly by meshes, from the margins of which the vascular bundle or bundles to the fronds are given off, and through which the parenchyma of the medulla is continuous with that of the stipes. In most tree-ferns the medullary axis is larger, and the bases of the stipes decay down to the circumference of the stem, but in *Osmunda* the persistent bases of the stipes permanently clothe the small vascular cylinder which encloses a slender pith. To this latter form belongs the stipe with a dumbell-shaped vascular bundle, separate specimens of which I have obtained from the Coal Measures. These have been described both on the Continent and in this country, under the name of *Zygopteris*, but they belong to Cotta's genus *Tubicaulis*; and they are very closely allied to a group of fern stems which I have already placed together under the name of *Chelepteris*. The stem structure of the common tree-fern is represented by the genus *Caulopteris*, of which I have six species of carboniferous age.

The third and extinct group is represented by Corda's genus *Stemmatopteris*, only now known to be British, and by *Psaronius*, which is, however, not a separate generic form, but only the internal structure of the stems of which Corda's genus is the external aspect. The chief characters of *Psaronius* have been drawn from the structure of the aerial roots which invest the stem, from which, indeed, the generic designation was derived; while the structure of the stem itself has been overlooked. But this is really of the first importance, as will appear from the following description which I have been able to make from a finely preserved specimen of an undescribed species in the British Museum, and from the figures of Cotta and Corda. The circumference of the stem was composed of a continuous envelope of indurated tissue; within this there were perpendicular tracts of vascular tissue never penetrated by any mesh. Between these tracts the leaves were given off in perpendicular series, the large single leaf bundles coming right out from the central parenchyma, where they existed as well-formed bundles, filling up more or less completely the medullary cavity. In one form (*Zippea*) the leaves are opposite, and the great proportion of the circumference of the stem is made up of the persistent and common vascular tissue; in others (species of *Psaronius*) the permanent elements of the stem consists of three, four, six or more perpendicular tracts.

The first two groups are analogous in the arrangement of the parts of their stems to that which exists in the first year's growth of a dicotyledon. In both there is a parenchymatous medulla surrounded by a continuous vascular cylinder, which is perforated in regular manner by meshes for the passage out of the vascular elements of the appendages. The stems of the third group have a structure analogous to that which is found in the stems of monocotyledons, for in both we have the vascular bundles of the appendages existing in the parenchymatous axis, and passing out independently of any closed cylinder. The permanent elements of the circumference of these stems of *Psaronius* are, however, without any analogue in the monocotyledonous stems.

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* Paper read before the British Association at Brighton in Section C, Aug. 19, by W. Carruthers, F.R.S.

There seems, then, good reason for establishing two groups of ferns, with differences characteristic of their stems, comparable to those which distinguish the stems of monoctyledons from those of dicotyledons. But the caution I have always insisted on in dealing only with vegetative organs is specially required here, for I have discovered, I believe, the fruiting fronds of one species of this group of plants. With the Bath specimens of *Stemmatopteris insignis*, Corda, as well as with those found on the Continent, the fronds of *Pecopteris arborescens* are always associated. It is the only fern found with some of the Bath specimens. It is also to be observed that the bases of the stipes correspond with the size of the leaf scars on the stems. These facts are not absolutely sufficient for the correlation of the fronds with the stem, but they are the best evidence for this that we can expect in fossil botany short of actual organic union. Now the fruit of *Pecopteris arborescens* is so near to that of *Cyathea* that I can find no characters whereby they can be separated. Our classification based on the stems must of course yield to that derived from the organs of fructification, and our group of ferns, instead of being made into a new order, as would be the case by some who publish on fossil botany, must be grouped with a tribe of recent *Polypodiaceæ*.

It may seem that this is a forced and arbitrary grouping together of plants that in some important characters so remarkably differ; and so it is undoubtedly to those who, with rash confidence, generalise on the systematic position of plants from stem structure alone. But what can such objectors say to the practice of placing in close proximity plants that are beyond question nearly related to each other in all essential characters, though some have caudices, while others possess rhizomes; yet these two forms of stems are yet more widely separated from each other than the extinct palæozoic group is from the recent forms.

SCIENTIFIC SERIALS

THE double number (Nos. 5 and 6) of the *Annalen der Chemie und Pharmacie* commences with a paper by Carl Grunzweig, on "Butyric acid from different sources." He prepared the perfectly pure normal and isobutyric acids and their salts, and examined their properties very carefully; he then examined butyric acid as obtained from butter, which he finds to be normal butyric acid, and the acid from the oxidation of conine and that from the carob, or St. John's bread, are also the normal acid.—Von Schneider contributes a long paper upon pollen and wax formation; which is followed by a second contribution by Kachler on the compounds of the camphor group. He has accurately examined into the properties of campholic acid and some of its salts, and also into the action of bromine and phosphoric chloride on that body. The action of bromine is to oxidize the acid, forming oxycamphoric anhydride, hydrobromic acid being produced in quantity. He has also examined camphinic acid.—Weselsky follows, with an important paper on the azocompounds of resorcin, in the theoretical parts of which some most elaborate graphic formulæ are brought to life.—Liebermann and Chojnacki have again examined rufiopin, which was first obtained by Anderson from opianic acid. The authors' researches show that it belongs to the anthracene group, as by the action of zinc powder at a high temperature this hydrocarbon can be obtained from it. It therefore belongs to the same series to which the colouring matters alizarin and purpurin belong, being the next higher body in the series to purpurin.—Dittmar and Kekulé contribute a paper on an aromatic glycollic acid. The starting point for the production of this body is toluylic acid, which is acted on by bromine, and the resulting bromo-compound again acted on by barytic hydrate. The oxymethylphenylformic acid obtained forms small plates or needles as crystallised from water.—Amato has endeavoured to obtain dicyanacetic acid by treating dichloracetic ether with potassic cyanide. He has not succeeded in obtaining the acid, but seems to have produced a body which has resulted from the decomposition of dicyanacetic ether with two molecules of water.—This number contains 18 original papers, several of which, however, are translations from foreign journals.

Annalen der Chemie und Pharmacie, No. 7.—This number commences with a lengthy article on chrysanisic acid, by H. Salkowski. This acid was discovered by Cahours, in the year 1849, and has been experimented on by many chemists. Kekulé has proved that it has the constitution of a dinitrobenzoic acid, of the formula $C_6H_2(NO_2)_2(NH_2)CO_2H$. Dr. Salkowski has now made some experiments on this and its derivatives. Chrysanisic acid is obtained by treating anisol with nitric acid of 1·4 sp. gr., which converts it into nitro-anisic acid; this latter body is then treated with red fuming nitric acid, the product of which action is submitted to the action of ammonia, which yields ammonic chrysanisate. This crystallises easily, and the acid can be obtained from it in the pure state without difficulty. The author has prepared a number of the salts of this acid, which are here described in detail. By the action of hydrochloric acid and tin on chrysanisic acid, triamidobenzoic acid is produced; this acid, on heating, is decomposed into triamidobenzol and carbonic anhydride. Triamidobenzoic acid appears to possess both acid and basic properties, as it can form salts with both strong acids and bases. Both classes of salts have been prepared and are described. Thus triamidobenzoic acid forms a compound with two molecules of hydrochloric acid, and also with one of sulphuric acid. Chrysanisic acid, by the action of strong hydrochloric acid, yields trichlorobenzoic acid; and by the action of nitrous acid, dinitroparoxybenzoic acid is obtained. From this body the monethyl and diethyl derivatives can be prepared.—An interesting paper on the influence of potassium and sodium salts on fermentation, by C. Knapp, follows. He finds that both potassium and sodium salts, more especially the chlorides which he has worked with, exert a hastening influence on alcoholic fermentation, the potassium chloride being the better of the two. He also finds that a small percentage of the salt acts more vigorously than a large one.—Richard Maly contributes two papers on the colouring matter of bile, &c.; and Liebermann and Van Dorf follow with an exhaustive paper on the cochineal colouring matters.—The next paper is by Beilstein and Kohlberg, on isomerism in the benzol series. This is the fourteenth contribution from these authors on this wide subject, the present treating on cinnamic acid and metanitrobenzoic acid.

Bulletin de l'Académie Impériale des Sciences de St. Petersburg, t. xvii., No. 1.—This number contains an important paper by Prof. Kamintzin on the employment of inorganic salts as a means for studying the development of lower organisms containing chlorophyll. He studied the action of salt solutions of specified composition and various concentration on forms of algæ, chiefly *Chlorococcus infusionum* and *Protococcus viridis*. The algæ and higher cryptogams bear a higher concentration than the phanerogams. In a 3-per-cent. solution they develop vigorously; in solutions under two per cent. zoospores were produced, these again forming zoosporangia; but where the concentration was higher the development was by division into round motionless bodies. Prof. Kamintzin also studies the development of individual varieties, owing to internal causes, the external conditions remaining the same, thus extending to the lower plant forms the class of observations made by Darwin in the animal kingdom. The plasticity of various forms of algæ under the above treatment is fully shown and illustrated.—O. Grimm describes the integumental structure of one of the Crinoidea, the *Comatula mediterranea*. Certain small canals observed in an internal fibrous layer he considers to be organs of respiration, the water entering by small openings on the external surface.—There are two short papers by Dr. Levschin on the development of osseous tissue and the structure of terminal blood-vessels in the bones of the newly-born. In a paper by Dr. Gruber, the dissection of a hand having two thumbs is described, and compared with three other cases of the same kind.

SOCIETIES AND ACADEMIES

LONDON

Royal Microscopical Society, October 2.—Dr. Hudson read a paper "On *Pedalion mira*," and exhibited specimens under the society's microscopes. This curious little animal was discovered last year by Dr. Hudson near Bristol, and a figure and short description of it is given in the *Monthly Microscopical Journal* for September 1871.—The President read a paper "On the Development of the Skull of the Crow."—Dr. Woodward sent a series of photographs for exhibition, showing the resolution of Nobert's 19th band with a Tolles lens, which was not properly corrected for chromatic errors.

Academy of Natural Sciences of Philadelphia, February 27.—"On an Extinct Whale from California," by Prof. E. D. Cope. George Davidson, of the United States Coast Survey, recently presented the Museum of the Academy of Natural Sciences, the proximal portion of the left ramus of the mandible of a whalebone whale. The specimen was found in digging a well at San Diego, on the coast, in the southern part of the State, at a depth of seventy-four feet below the surface, July 27, 1871. The angle and condyle are broken from the specimen, and the distal extremity was not preserved. It possessed a coronoid process, the apex of which has been lost. The inner face is plane, somewhat convex above, behind the basis of the corodoid process. Anteriorly it becomes more convex, the surface turning inwards to the superior and inferior margins. The exterior face is convex, so that at the posterior foramen its diameter above the middle is greater than that below the middle. The inferior outline, from below the coronoid process to below the last external foramen, is straight, not decurved. It is obtuse most of this distance, but becomes narrowed at the anterior point. The superior margin is obtuse anteriorly, narrowed acute for ten inches anterior to the coronoid process; it is not truncate anteriorly. The internal foramina are large, and form a series below the upper margin, without distinct groove. The external foraminal series terminates much anterior to the interior, that is, the last external is opposite the sixth from behind of the inner row. There is no internal Meckelian groove. The Meckelian cavity of the ramus is large behind the coronoid, but small and in the upper part of the ramus at the last exterior foramen. The dental foramen is large, and above the base of the Meckelian cavity, so that its inner wall descends to the floor of the latter. Below the base of the coronoid the inferior part of the ramus is rounded, but narrower than at the dental foramen. The presence of coronoid process indicates that the present species was a finner, and allied to *Balænoptera*. Though there are no vertebræ or other elements to determine its reference to this genus or its ally *Eschrichtius*, it may be proper to refer it provisionally to the latter genus, since so many of its allies on the Atlantic coast formations have been found to be referable to it. This course is still more appropriate from the fact that the strata of tertiary age near San Diego are reported to be of miocene age, the same in which the eastern *Eschrichtii* have been found. As to its specific characters, these differ entirely from those of the latter. The ramus lacks the decurvature of most of them. In size, it approaches nearest the *E. polyporus*, Cope, and *E. priscus*, Leidy. It is much less convex externally than the latter. The exterior series of pores does not extend so far posteriorly as in *E. polyporus*, and the dental foramen has a superior position besides other differences. Size that of *E. priscus*. This whale, when living, probably attained a length of about forty feet.

PARIS

Academy of Sciences, September 23.—M. Faye, president. —The following gentlemen, members of the International Metric Commission, were present at the meeting, to which they were presented by the president:—Baron de Wrede, Sweden; M. Broch, Norway; M. de Jolly, Bavaria; MM. Stas and Maus, Belgium; General Ricci and M. Govi, Italy; Father Secchi, Pontifical States; M. Hirsch, Switzerland; M. Struve, Russia; Mr. Hilgard, U.S. America. General Fligelly, of the Austro-Hungarian Empire, member of the International Geodesical Committee, was also presented.—M. P. Favre read a note on the origin of the heat developed when the motion of a metallic disc is retarded by the influence of an electro-magnet. The author had stated in a paper read on the 11th September, 1871, that this heat is due to the "work furnished by the operator," and that the magnet expends no energy in producing it, the same effects being produced by permanent magnets which do not expend anything. He has repeated his experiments with apparatus of very great power, and finds all his statements confirmed.— —M. Yvon Villarceau then presented the elements and ephemerides of the planet 103, Hera, by M. Leveau, which was followed by the "Results of a search into the characteristics of the elementary and quartic systems," by M. H. G. Zeuthen, presented by M. Chasles. The President then presented a note of M. A. Lallemand, on the "Polarisation and Fluorescence of the Atmosphere." The author attributes the blue colour of the atmosphere to "hypochromatic fluorescence," which he explains as fluorescence accompanied by change of refrangibility due to the partial absorption of the ultra-violet rays. The next paper was by M. E. Duvillier on "A new method of preparing Chromic Acid." The author mixes into a cream 100 parts of baric chromate, and 100 parts of water, then adds 140 parts of nitric acid of 40° Reaumur, boils for 10 minutes, filters, and allows the baric nitrate to crystallise out, after which the liquid is concentrated to about the bulk of the acid employed, which removes the last traces of baric nitrate, and the chromic acid crystallises after the expulsion of the excess of nitric acid by repeated evaporations.—MM. P. Champion and H. Pellet followed with a paper of great interest, on "The Vibratory Movements produced by Explosive Compounds." The authors, starting from an observation by Mr. Abel, that whilst a small quantity of fulminating mercury exploded in gun-cotton caused its instant violent explosion, the much more violent explosive iodide of nitrogen produced no effect, proceeded to investigate these two bodies with a chromatic scale of sensitive flames, arranged as recommended by Messrs. Tyndall and Schaffgotsch, when they found that the fulminate produced effects corresponding to the notes, la, do, mi, fa, sol. The iodide of nitrogen, however, produced no effect. When the explosives were brought as close as 3·50 metres to the flames, the iodide of nitrogen affected the upper notes, while the same weight of fulminate acted on the whole gamut. The weights used were in each case ·03 grm., and it was not till the iodide of nitrogen had been increased to ·2 grm. that it produced effects equal to the fulminate.—M. Duchartre then presented a note by M. J. Duval-Jouve, "On the diaphragms and fibro-vascular nets of the leaves and stalks of certain Monocotyledons;" which was followed by a continuation of M. Stan. Meunier's paper, "Observations on the Vein Action in Meteorites;" after which M. F. Garrigou read a paper on "The alluvial gravels of the plains of the Garonne at the village of Portet, near Toulouse."—A note of M. Bonvier, presented by M. Bouley, came next, claiming priority of discovery for M. G. B. Pelletan of the "Method of removal of liquids from the closed cavities of the body by means of aspiration," described by M. J. Guérin.—M. Hartsen sent a note relative to an alkaloid extracted from Isopyrum.—M. Dumas presented an analysis of the documents sent to the Phylloxera Commission by two of its delegates, MM. Duclaux and Maxime Cornu; and a note from M. J. Capello, of Lisbon, on the aspect of the sun about the 9th of August closed the meeting.

BOOKS RECEIVED.

ENGLISH.—Elementary Geology: J. C Ward (Trübner and Co).—An Elementary Treatise on Geometrical Optics: W. S Aldis (Deighton and Bell).—Elementary Treatise on Natural Philosophy: Deschanel. Part 4 (Blackie and Sons).

AMERICAN.—Papers relating to the Transit of Venus in 1874, prepared under the direction of the Commission authorised by Congress. Part 1.

FOREIGN.—Eerste Vervolg Catalogus der Bibliotheek en Catalogus der Maleische, Javaansche en Kawi handschriften van het Bataviaasch Genootschap van Kunsten en Wetenschappen (Bruining, Batavia).—Tidschrift von Indische Taal- Land- en Volkenkunde: Stortenbeken en Michielsen, Deel xviii., Zesde Serie, Deel i., Aflevering 3 en 4; Deel xx, Zenende Serie, Deel i., Aflevering 3 —(Through Williams and Norgate)—Der Wirbelsturme, Tornados u Wettersaulen in der Atmosphare: Dr. T. Reye.—Pathologische Histologie der Laftwege u. der Lunge. Dr. A. Thierfelder.

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THURSDAY, OCTOBER 17, 1872

CANON KINGSLEY ON PHYSIOLOGICAL TRAINING

THE recent address of Canon Kingsley, as President of the Birmingham and Midland Institute, has struck a key-note which has been widely responded to. Not that he has said anything new; but truths are none the less true for being world-old. It is something to find a man of Mr. Kingsley's popularity and influence insisting on the need of physical and scientific culture; it is more that the utterance should be made to a crowded audience at one of our great centres of industry; it is still more that our daily and weekly papers should at length discern the importance of that which a select few have long been preaching in vain. We can, however, only refer to some of the more important points on which the lecturer touched, referring our readers to the report *in extenso*, in the *Birmingham Daily Post.*

The following admirable advice was given to the younger students among the assembly:—

"Let me warn you that none of you will profit by any lectures, unless you study at home the text-books recommended by the lecturer. You will be otherwise little wiser than a man who should purpose to learn arithmetic by listening to talk about the proportion of numbers without doing sums himself. You will not teach yourselves even the attitude necessary for your subject—the attitude of mind, by which the facts were discovered, by which they must be understood, by which they must be turned to use. You will not acquire, by mere lecture-hearing, the inductive habit of mind which arranges and judges of facts. Still less, therefore, will you acquire the deductive habit of mind which makes use of facts practically after they have been arranged and judged; and the lecturer will be to you but a sort of singer, a player upon a fiddle, who makes for you pleasant and interesting noises for a while, producing mere impressions which never sink into the intellect, but merely touch the emotions, to run off them at the first distraction, like water off a duck's back. Therefore, remember this for yourselves in this age of periodical literature and literature made easy: we are all too apt to forget that what we did you must do, if you wish to be as good men as we, viz., work for yourselves, as we did; that good lectures, like good reviews, are not meant to see for you, but to teach you to use your own eyes; and those you must use at home in hard study, personal study, continuous study—and study, too, rather of one subject than of many subjects, in order that, by learning how to learn one thing thoroughly, you may learn how to learn anything and everything else in its turn."

After referring to the evils of war in producing the exactly opposite results to those brought about by the process of Natural Selection, by the Destruction of the Fittest, the lecturer thus proceeds:—

"Peace, prosperous, civilised, humane, such as we enjoy now, is fraught with the very same dangers. In the first place tens of thousands—who knows it not?—lead sedentary lives, stooping, asphyxiated, employing as small a fraction of their bodies as of their minds; and that such a life must tell upon their offspring—it may be for generations to come—what medical man does not know full well? And all this in dwellings, workshops, mines, and what not, where the influences, the very atmosphere of which tend to unhealth, and not to health; to drunkenness as a solace under the feeling of unhealth and all unhealth's depressing influences. But now—and this is one of the most fearful problems with which modern civilisation has to deal—we interfere with natural selection from conscientious care of life just as much as war itself does. War kills the most fit to live. We spend vast energies in saving alive those who, looking at them from a merely physical point of view, are most fit to die. Everything which tends to make it more easy to live—every sanitary reform, prevention of pestilence, medical discovery, amelioration of climate, drainage of soil, improvement in dwelling-houses, workhouses, prisons, every reformatory school, every hospital, every cure of drunkenness, every influence, in short, which has (so I am told) increased the average length of life in these islands since the first establishment of life insurance offices, 150 years ago, by nearly one-third—every influence of this kind, I say, saves persons alive who would otherwise have died; and the great majority of these persons, even in surgical cases and cases of zymotic disease, will be those of the least resisting power, the weaklier; thus preserved to produce, in their turn, a weaklier progeny. And what will you do with it? Do I say that we ought not to save them if we can? God forbid! The weakling, the diseased—whether infant or adult—is there on earth a British citizen! no more responsible for its own weakliness than for its own existence. Society—that is, in plain English, you and I and our ancestors—are responsible for both; and we must fulfil the duty, and keep the weakly person in life, and if we can, heal, strengthen, develop to the utmost, and make the best of that which 'Fate and our own deservings' have given us to deal with."

The practical application of this teaching was then pointed out:—

"And so as to the laws of personal health;—enough, and more than enough, is known already to be applied safely and easily by any adult, however unlearned, to the preservation, not only of his own health, but of that of his children; the value of healthy habitations, of personal cleanliness, of pure air, pure water, of various kinds of food, as tending to make bone, fat, or muscle, provided only that the food be unadulterated—and you might stop the adulteration in Birmingham in a month or week if you chose. . . . Have you not here, ready made to your hands, an engine for extending sound knowledge of the laws of health? In a great manufacturing district, which specially needs those laws to be known and obeyed, you have this Institution always teaching physical science. It would not, therefore, go beyond its province in teaching the physical science of health. It teaches, happily, a people specially intelligent, specially accustomed by their businesses to the application of scientific laws. To them, therefore, the application of any fresh physical laws would have nothing strange in it. They have already, I doubt not, that inductive habit of mind which is the groundwork of all rational understanding or action. They would not turn the deaf or contemptuous ear with which the stupid, the savage, the superstitious, receive the revelations of Nature's mysteries. Surely, with such a people to work upon, it were well worth your while to expand your classes of physiology, and give one or more of them a practical turn in the direction of practical health. Your Animal Physiology Class is, I doubt not, a sound and useful one. It cannot well be otherwise, while its text-book is Prof. Huxley's Elementary Lessons; and I am glad to see that your learned lecturer is about to confine himself, for the present at least, to the physiology of the animal most abundant in, and most important to, Birmingham, namely, man. Twenty lectures are announced in your programme dealing with the tissues of the body, their structure and uses, circulation of the blood, respiration, chemical changes in air respired, amount breathed, digestion, nature of food, absorption, secretion, functions of the nervous system. Now, this is as it should be. It is admirable. Teaching of this kind ought, and will in some more civilised society, be held as a necessary element in the school course of every child; just as important as reading, writing, and arithmetic; and is the most necessary and most important branch of technical education, namely, the act of keeping yourselves alive and well. But you can hardly stop short there. After you have taught the conditions of health, should you not teach also somewhat of the causes of disease—of those diseases, especially, which tend to lower wholesale the physical condition of dwellers in towns, exposed to the unhealthy influences of an artificial life? Should you not teach young men and women something of the causes of pestilence, of zymotic disease, and of scrofula, consumption, cerebral derangement, dipsomania, and such like? Should you not show them the value of pure air, pure water, unadulterated food, wholesome dwellings? We want set up in the centre of large towns—it will not come yet, but it will come some day—a statue to the goddess of purity. Is there one of them, man or woman, who would not be the safer himself and the more useful to his neighbours if he had acquired some sound notions about those questions of drainage on which their lives and the lives of their children may any day depend. I say women as well as men; ay, women even more than men. For it is the women who have the ordering of the household, the women who have the bringing up of the children. And if any say, as they have a right to say, 'But these are subjects which can hardly be taught to young women in public lectures,' I reply, 'Of course, unless they are taught by women—women, of course, duly educated and legally qualified.' Let them tell young women what every young woman ought to know, and what her parents will very properly object to her hearing from men, or in the company of men. This is one of the main reasons why I have for twenty years past, and shall as long as I live, advocated the training of ladies for the medical profession. And now, I am seeing the common sense of England, and, indeed, of every civilised nation, coming round to that which seemed to me, when I first conceived of it, a dream too chimerical to be cherished, save in secret; and I trust soon to see a supply of lady-doctors, sufficient to fulfil that old dream of mine, and to establish in every great town of these islands health classes for women.

"Now why should not your Institute, which has taken the initiative in so many useful enterprises, take the initiative in this too? It is already a school of many things. Why should it not be also a school of health—a school of sanitary science? Why should it not send forth year by year more and more young men and women, taught not only to take care of their own health and that of their families, but to exercise moral influence in the same direction over their fellow-citizens—to advocate as one simple, and yet most necessary and important, good deed, the teaching of the laws of health in every school, from the highest to the most elementary? Do that. Send forth healthy pupils yearly, champions in the battle against dirt and drunkenness, disease and death, and you will raise a yet prouder title to the gratitude of your fellow-countrymen than you have earned already by your scientific zeal and your noble liberality. There are those who may answer—or rather there would have been those who would certainly have answered, five and twenty years ago, before the so-called materialism of advanced science had taught us some practical wisdom about education—'And if it were so, what matter? Mind makes the man, not body. We do not want our children to be stupid giants and bravos, but clean, able, highly-educated, however delicately Providence or the laws of Nature may have been pleased to make them. Let them overstrain their brains a little, let them contract their chests, and injure their digestion and their eyes, by sitting at desks and poring over books. Intellect is what we want, and intellect makes money; intellect rules the world. I would rather see my son a genius than an athlete.' And so would I. But what if for want of obeying the laws of Nature you got neither genius nor athlete, but generally an incapable, unhappy personage? Without healthy bodies you will not, in the long run, get healthy intellects. . . . Wherever you have a population generally weakly, stunted, scrofulous, you will find in them a corresponding type of brain which cannot be trusted to do good work. It may be very active, it may be very quick in catching up new and grand ideas—all the more quick on account of its own secret malaise and self-discontent; but will be spasmodic, irritable, hysterical. It will be apt to mistake capacity of talk for capacity of action, excitement for earnestness, virulence for force, and cruelty for justice. It will lose manful independence, individuality, originality, and when men act they will act from the consciousness of personal weakness, leaning against each other, swaying about in mobs and masses."

We sincerely hope that the publicity which has been given by the public press to this address by Canon Kingsley will be the means of awakening the minds of many to the vital importance of that scientific training which he, in common with every enlightened mind, advocates.

MARTIN ON MICROSCOPIC MOUNTING

A Manual of Microscopic Mounting. By J. H. Martin. (J. and A. Churchill.)

SCIENCE in this country is certainly under great obligations to *amateurs*, or *dilettanti* as they are more correctly called. The fact is there are in Great Britain but a very few men of science, that is to say, men professionally devoted to scientific careers, as compared with Germany and even France—we are very poor in this source of power. Germany has a host of universities and high-schools in each one of which there are more men whose lives are definitely told off to the cultivation of science, than there are in our greatest and richest seats of learning. Dilettantism is the fashion of some branches of science in this country, and under it they have thriven in a way of which we may be to some extent proud; but which seems likely enough to impede greatly their more systematic cultivation. Geology has perhaps more than any science benefited by the patronage of *dilettanti*; but it is not difficult to foresee the time when its problems will have become too arduous for any but trained and devoted specialists to make any way with them. The same is to a less extent true for the biological sciences, in which, besides the enthusiastic field-naturalists, the members of the medical profession have been conspicuous as *dilettanti* explorers. At the time when (some five and twenty years ago) the microscope was first brought to a state of efficiency in this country, a perfect army of amateurs entered the fields of animal and vegetable histology, equipped with the beautiful and costly instrument, and brought to light a considerable number of important facts bearing on the structure of tissues and the minuter forms of life. This was not the case on the Continent, the costliness of the instrument, in addition to the other causes which make Englishmen remarkable as scientific *dilettanti*, tending to limit the movement to this country. The taste for amusement with the microscope has by no means diminished of late years, the sales of instruments by English makers being something astonishing in point of number and implied outlay. But it is a fact—for which we can most positively vouch—that the good work done at the present time in England with the microscope has fallen very far behind its original proportion to the number of instruments in use. In fact we have in the history of English research with the microscope, a typical case of the breaking down of dilettantism. Nearly all the discoveries easy to hand, which could be made by half-an-hour's pleasant peeping through a good microscope have been made, and the numerous observers formerly urged on by success have now become reduced to mere collectors of diatoms and mounters of fleas. On the other hand, in Germany, an efficient microscope was produced somewhat later than in this country, on a different system and at a far less cost. It was not taken up by men of leisure and means, nor were the makers tempted to produce the most elaborate and costly mechanical contrivances for no practical end but their own profit. The microscope in Germany fell into the hands of the professed students of science in the universities, and in consequence the art of applying this instrument to the study of structure has steadily advanced in that country, until not only are all the important observations which are made with the microscope made in German laboratories, or by those who have studied in them; but the whole art or "technic" of microscopy has become a German one. When we find that it is necessary, for some purposes, to watch a single cell for twelve hours or more consecutively, and that three or four months' daily labour is not considered too much to devote to advancing one small step in the knowledge of such a matter as the nerve branches which go to the glands in a frog's tongue, we are not surprised that the *dilettanti* are no longer of service in the progress of researches with the microscope. Thorough and single-purposed men are required for such work; in short, "men of science," supported by special institutions.

It is then with very mixed feelings that we contemplate Mr. Martin's book on Microscopic Mounting. It is of about as much use for scientific purposes as would be an alchemist's guide-book in a modern chemical laboratory. Foreigners who do not know of the immense number of microscopes annually sold in this country as toys, will wonder who on earth can make any use of it. Microscopic mounting is a proceeding which bears about the same relation to the genuine microscopic study of an organism, as enclosing a "subject" in its coffin does to human anatomy. The object which is aimed at by the modern student of histology is not to "mount" a pretty specimen, but to apply such staining, clarifying, coagulating reagents, and such methods of disruption, slicing, hardening, or softening, as will enable him to discriminate, describe, and draw structure; or, it may be, so to arrange the somewhat restrictive conditions of microscopical examination as may enable him to observe with the highest powers the tissues of organisms in their *living* state. He does not care a dump for "mounting," and if he has in the course of his work prepared some hundred or two preparations of an organ or organism which he is investigating, the preparations are usually thrown away when the problem under investigation is solved, or lie for years undisturbed in the catacombs of some cabinet. Mr. Martin is of no assistance whatever in all that relates to the use of reagents or adjuncts to observation with the microscope. He is evidently quite ignorant of German; he gives a number of receipts taken from a variety of antiquated English sources; others he takes without acknowledgment from the "Quarterly Journal of Microscopical Science;" see, for example, the paragraph on staining tissues, p. 144, which can only serve to mislead. Some, of course, amongst Mr. Martin's instructions are correct; but we feel convinced that he has not made trial of even the few methods which he has imperfectly described; and we decidedly object to his assuming the office of a guide, when it is but a case of the blind leading the blind.

Still, Mr. Martin's book may be looked at as an interesting specimen of the phase which English *dilettante* microscopy has now reached. Had the author been a little more careful in confining himself to what he really understood as a diligent mounter of microscopic objects, his volume would have had a genuine antiquarian interest, and would have perhaps been useful; indeed, it may even now in some ways be so to those who take a view of the microscope similar to his own.

It would hardly have been worth while expending so many words on a book in itself of so little significance, had not there been two conditions in existence which, if passed by without remark, may have some consequences which had better be avoided. Firstly, the number of persons, medical students and others, now entering on the serious study of histology, and the use of the microscope as an instrument of scientific research, is increasing. They want books to supplement the efforts of teachers in putting them into the way of working and explaining to them—if we may so say—the most useful "dodges." Secondly, there are other books besides Mr. Martin's which profess to give instruction on microscopic manipulation, and are all *worthless*. The students may, in their innocence, be led to purchase such books. We wish them to avoid making this mistake. There is no satisfactory book in English on the subject. Beale's is the best, but too large, and not sufficiently complete excepting as to his own methods. Frey's book, which exists in both a French and a German form, is the best foreign guide to microscopic *Technik*.

OUR BOOK SHELF

Magnetism and Deviation of the Compass. For the use of Students in Navigation and Science Schools. By John Merrifield, LL.D., F.R.A.S. (London: Longmans and Co.)

THE Admiralty Manual of Compass Deviation with its auxiliary Elemenatry Manual would, after some lapse of time, appear to have been turned to good account by teachers of navigation. The small volume under review is one of a plentiful supply which appears to be now issuing from the press on a subject of vast importance to seamen of the present generation; and as such is deserving of notice.

It contains useful information of an elementary kind, although this information is not presented either in the practical form or exact and mathematical arrangement of the two manuals we have noticed as the fountain heads of the science of magnetism as applied to iron ships and their compasses.

It is to be regretted that in a subject where clearness and precision are essentials to teaching, Dr. Merrifield should have written occasionally with looseness and inaccuracy; for example, at p. 52 we find "Vertical iron at the same place will produce the same deviation in whatever direction the ship's head may be, because a vertical line makes always the same angle at the same place with the line of force." There is confusion of cause and effect here—the deviation is not the same in whatever direction the ship's head may be, although the force remains constant or nearly so—and these are important features for the student to realise.

Notwithstanding, however, these shortcomings, Dr. Merrifield's "small manual" may in the main be received as an orthodox production, and will tend to divert the attention of his students from some singular statements and conceptions relating to the deviation of the compass on board ship and its compensation, to be found at pp. 20-23 of his Navigation and Nautical Astronomy published conjointly with H. Evers in 1868.

We hope to see manuals of this smaller class emanating from seamen, who, possessed of the knowledge of the exact requirements in the practice of their calling, can extract from original sources, and simplify the main points in the various divisions of the subject.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Solar Spectroscopic Observations

I HAVE just seen Herschel's letter to you in NATURE of October 3, and am induced to address you, lest his remarks on Indian Climate should make difficulties in respect to the British Association's proposal for a physical observatory for solar observation.

I do not think Captain Herschel has been in England since the Eclipse of 1868, and consequently he cannot speak from experience of the English climate. It is beyond all doubt that from some cause (commonly believed to be haze) there are really very few days when the protuberances can be studied in England. Herschel's letter contains evidence that that cause, whatever it be, is absent in India. We need not go further; we have the two facts (1) that it is impossible in England, with great telescopic power and great dispersion, to see the prominence lines regularly; (2) that it is possible to do so in India, even in the plains at a bad season. The result seems to be that solar investigations should be pursued in India systematically.

I may add that the facility of seeing these things, and the probability that spectroscopic inquiries into the celestial bodies would have been more successful in the earlier stages had there then been any observers in India, are to my mind a strong argument for now establishing in India, and on a permanent footing, an observatory, whose speciality shall be "Researches on the Physics of the Sun and Planets," whether by the spectroscope, or vision, or photography. When the proposal of the British Association is made formally to the Government by the Council, it will, I trust, take this form. There is no such institution now in the British dominions, and when one is established, it should be in the Tropics somewhere, and as systematically devoted to physical researches as Greenwich has been to the moon.

The Director might, probably would, as at Greenwich, in time attach other investigations; but these researches should be his primal object; and, if they were made so, there is no reason why work as standard and as useful should not result.

October 4 J. F. TENNANT

Consciousness and Volition

THE question raised by Mr. Bennett in last week's NATURE is of great importance, and of no small difficulty. During a visit to the late Sir William Hamilton in 1855, this subject came up for discussion. Sir William was then engaged on his edition of the works of Dugald Stewart. I called his special attention to Stewart's doctrine regarding the operation of Will in acts which are usually ascribed to Habit. Stewart asserts that all habitual actions are really voluntary. As he had no acquaintance with the modern doctrine of "latent mental modifications," he would naturally take for granted that there can be neither a volition nor any other act of mind without accompanying consciousness. He accounted for the non-remembrance of that consciousness by the extreme rapidity of the volitional action. (Collected Works, vol. ii. chap. ii.) To this Sir William objected. He went on to show that in many cases an act, or even a long series of acts, originally voluntary, have ceased to be so. The habit or habitude, which is a mental tendency, though not a power, generated by custom, supplies the place of volition. In illustration, he referred particularly to the well-known fact that in India soldiers will march long distances when they are asleep. Now, it seems to me that this decides Mr. Bennett's question. Here we have regulated action, determined, not by volition, but by habit. Sir William, however, failed to meet all my difficulties, because, as I afterwards saw, of his unsatisfactory theory of causation. He so frequently confounded conditions with causes. With him a cause denoted anything without which an effect could not be; hence his doctrine of con-causes, a plurality of causes for each effect. Thus, when I will to move my hand, and the movement follows, Sir William would call the volition *one* cause of the movement, whereas it is merely a condition. It is remarkable that he should fall into this error since he rejected Biran's doctrine regarding the efficiency of volition. In a subsequent conversation we discussed the points of similarity and the points of difference between Habits and Instincts. It would, however, be trespassing on your space to give the details.

I will only add that an "unconscious volition" in the sense intended by Mr. Bennett is not possible. Dr. Carpenter's expresion "unconscious cerebration" I regard as unfortunate, since it appears to rest on the assumption of the essential identity of mental, vital, and physical powers. Mr. Bennett will find many valuable observations on the nature and conditions of habitual acts in Hamilton's lectures on "Metaphysics," Morell's "Outlines of Mental Philosophy" (published in 1862), and Sir Henry Holland's "Mental Physiology."

JOHN MOORE

Stamford House, Sale, near Manchester, Oct. 14

The Solar Spectrum

UPON reading the communication from Capt. Herschel in your number for October 3, upon the solar spectrum, I seem to remember a letter from a correspondent being published some year or more ago in your pages, in which it was stated the writer had seen the bright lines near the sun's limb with one of Browning's direct vision prism spectroscopes, the instrument being placed on the back of a swing looking-glass as a stand. The dispersion of this instrument would be probably rather more than that of one angular prism. I am bound to say that I have been unable myself, up to the present time, to do more than see a bright line near D, superposed on the solar spectrum, with such an instrument as, however, for other purposes, is a most convenient and sufficiently powerful one.

To my own I have added three small wedge-shaped pointers cut out of thin brass, and fixed in the eye-piece, while the slit-plate, and consequently the spectrum itself, is drawn across the field by a micrometer screw having a range of about 600 divisions between its starting point near A and G at the other end of the spectrum. In this way a bright line (say the auroral one) may be brought upon one of the pointers, and its distance from D, in a salted spirit-lamp flame, at once accurately measured and mapped down; and if the pointer is again brought on the line after the observation, it may be verified by the position of the pointer next day upon the solar spectrum itself.

It occurred to me the other evening to try the effect of a V-shaped slit, and it seems to me to have some advantages. The lower ends of bright lines being brought to a fine point, are more easily positioned on a scale or pointer, while the tapering of faint or nebulous lines or bands enables their relative intensity to be more easily compared. My experiment was a rough one, and with a home-made slit; but for working purposes, by a sliding plate a slit might be contrived of a V shape, whose width and length would be altered at pleasure; and it would, I think, for some observations give good results.

Guildford, Oct. 7 J. RAND CAPRON

A Day Aurora

A CONTROVERSY was carried on in your columns about a year ago as to the possibility of an aurora being seen during the day time. A recent communication from Padre Secchi to the French

Academy of Sciences will be found of interest as bearing on this question. Writing from Rome on the 27th of August, he says (*Comptes Rendus*, p. 613):—"On the 15th of this month we had an Aurora Borealis by day, at ten o'clock in the morning up to midday. The magnetometers were greatly disturbed, and in the heavens at half-past ten appeared an arch of light cirrus clouds, stretching from N.N.W. to N.E., and crowned along the whole of its contour by numerous and fantastic rays (*jets filamenteux*). The forms of these rays so perfectly resembled those of the solar protuberances that some of the drawings of them might easily be mistaken for drawings of solar protuberances even by people well accustomed to these observations."

Merton College, Oxford J. P. EARWAKER

Meteor

LAST night, Oct. 9-10, about midnight, G.M.T. a meteor was seen by my wife in the S., considered by her to rival the brightness of Venus, and describing a path which was so carefully sketched by her immediately afterwards as to form a possible basis of comparison; and which therefore may be thought worthy of insertion in NATURE. It seems to have become visible near ζ Ceti, probably rather *nf* that star (which, however, was not noticed by her through a dewed window-pane), and to have passed with a slow motion and a yellowish light, in a path somewhat convex towards the zenith, in the direction of β Ceti, before reaching which it vanished. For about three-fifths of its course it preserved the same aspect, as of a ball of light with sparklings round it, and some appearance of a train; but in its further progress it seemed to waste away to extinction.

T. W. WEBB

Hardwick Vicarage, Herefordshire; according to Ordnance Map, long. W. 3h. 4m. 23s., lat. N. 52° 5′ 20″.

Fossil Oyster

I HAVE recently noticed a fossil oyster, in what Sir C. Lyell calls the Lower Miocene, or Hampstead beds. Can you, or any of your readers inform me if it has been noticed before. I can find no mention of it, in any work within my reach. I have been a subscriber, from your first number; and have observed the kind notice you have extended to other inquirers, and have thus been emboldened to trouble you. INQUIRER

N.B.—I have no pretensions to science, or any scientific acquaintance, being merely a solitary observer.

AN ELECTRICAL BAROGRAPH

I HAVE recently designed a barograph, a brief account of which may be interesting to your readers. The advantages claimed are:—

That the record may be seen as it is going on.

That it is quite as, if not more sensitive than, the photographic barograph, and the scale is larger.

That no time is lost preparing the paper, printed forms answering the purpose.

That the first cost and cost of working are both much less than in the photo-barograph.

A photograph has been taken which shows the instrument in working order, with part of a day's record shown on the cylinder.[*]

The cylinder is ten inches long, and eight inches in diameter, allowing for one inch per hour of paper.

The clock, or governor, is connected by a bar to a movable inclined plane, this is again connected by a bar to the long wire parallelogram which carries the pen, and the clock, by means of an eccentric, causes the inclined plane, and with it of course the pen frame, to move backwards and forwards once every minute. The wire frame is guided by four brass friction wheels, attached to a brass frame having motion up and down only; under it are the coils of an electro-magnet, the armature of which is attached to the brass frame. So long as no electricity passes through the coils the brass frame is thrown up by a small spring high enough to lift the pen off the paper.

The barometer tube is an ordinary glass one 0·58 in diameter, and is fixed firmly to the case. Its cistern is a small glass one, one inch in diameter, and cemented to a brass arm hinged to the left side of the case, and which allows it perfectly free motion up and down, but not sideways. From this cistern projects a very light arm, also hinged, and bent at the end so as to extend over the inclined plane. One wire of the battery is attached to the cistern arm, and the other, after passing round the magnet, to the inclined plane. As soon, then, as these two parts touch, the electro-magnet brings down the brass frame, and with it the pen, on to the papers which at once begins to mark, and continues to do so until the motion of the clock draws the inclined plane from the cistern arm, and so breaks the contact; the pen remains off the paper until, by the motion of the clock, the inclined plane is brought to touch the projecting cistern arm, when the pen at once begins to write. As the barometer, when the pressure increases, must draw the mercury for its increased height from the floating cistern, the cistern becomes lighter, and rises with it, and the smallest motion may be made sensible by altering the inclination of the moving inclined plane. The accuracy of the motion of this plane is secured by making it work on two fine steel points—the same motion, in fact, as that given to the cutter of a dividing engine. The cistern floats in a reservoir of mercury.

The pen is a syphon pen, supplied with *thin* ordinary writing ink. H. C. RUSSELL

Sydney Observatory, Aug. 10

BEAUFORT'S WIND SCALE AND THE BOARD OF TRADE[*]

THE Board of Trade have recently issued instructions to Receivers of Wreck and Officers of Coastguard, with reference to Beaufort's Wind Scale, so that one uniform construction should, as far as possible, be placed upon the wind scale by them. In the Circular the following passage occurs:—

"The Board of Trade are led to think that different constructions are placed by different persons upon the scale known as Beaufort's scale. In illustration, it may be remarked that the higher forces, 11 and 12, are, as the Board learn from the Meteorological Committee, scarcely, if ever, reached in the British Isles. Force 12, which is intended to represent a West India hurricane, the velocity of which is 80 miles per hour and upwards, has been reached only twice in four years on the coasts of the United Kingdom; notwithstanding high winds prevailing at the time of a wreck are frequently described by the ships' officers as storms or hurricanes."

It is here taken for granted that the positions of the anemometers of the Meteorological Committee are such as to record observations of wind fairly comparable with those felt at sea; and also that the anemometers are constructed to record those velocities of the wind which are applicable to the case in hand.

It is not stated how the two instances of velocity of 80 per hour and upwards were ascertained. Since, however, the space traversed or recorded by the anemometers at the observatories of the Meteorological Committee can scarcely be measured for a shorter period of time than 15 minutes, it may be assumed that on two occasions, and only on two occasions during four years, have the anemometers been noted to record a velocity of 20 miles or upwards in 15 minutes—that is, a velocity at the rate of 80 miles an hour or upwards. If the tracings of the Hemispherical Cup Anemometer could be read off for so short a period as five minutes, many instances of 80 miles an hour, and even several velocities of 100 miles an hour and upwards, could be taken from the records of these four years in the Meteorological Office. Indeed, a careful inspection of the lines of wind-velocity published in the Committee's Quarterly Reports renders this supposition extremely probable.

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[*] A photograph and section were obligingly forwarded by the author with his description.

[*] See Circular, No. 558.

During high winds it is well known that the wind does not blow with a uniformly high velocity, but that there occur frequent gusts of comparatively brief duration, many of the heaviest being, indeed, all but instantaneous. Thus the anemometer may indicate a velocity at the rate of no more than 60 or 70 miles an hour, but during the time there may have occurred 20 or 30 sudden gusts quite equal to the Force 12 of Beaufort's scale. Now, it is these repeated heavy gusts which cup-anemometers do not record that sailors have to provide against in the management of their ships. Hence it happens that while at observatories on land, provided only with cup-anemometers, no greater velocities than 60 or 70 miles an hour can be noted, in ships at sea, what the seaman has actually to deal with are velocities of 80 or 100 miles an hour. He accordingly enters these high pressures in his log.

It is evident that the Board of Trade are not in a position to give the assistance to sailors which they are seeking to give, till pressure-anemometers have been established at their observatories.

The Circular contains this very judicious remark:—"The Board desire to impress upon Receivers and Officers employed in reporting casualties, that the direction and force of the wind at the time of a casualty should be ascertained as accurately as possible, and that therefore these particulars should not be inserted without every precaution being taken to insure that they are in accordance with fact." It only remains that the Board of Trade furnish each Receiver and Officer with a simple pressure-anemometer, having a scale, 0 to 12, agreeing as nearly as possible with Beaufort's scale, and so constructed as to show the pressure at the time of observation, and to register maximum pressures, so that the officials may be put in a position to carry out the instructions of the Board.

SCIENCE AT OXFORD AND CAMBRIDGE

THE following courses of lectures are arranged for the ensuing term at the University of Oxford:—

Mr. R. B. Clifton, Professor of Experimental Philosophy, on "Optical Instruments and Physical Optics;" beginning Saturday, the 19th of October. The Physical Laboratory of the University will be open daily for instruction in practical physics from 10 to 4 o'clock on and after Thursday, the 17th of October.

Mr. J. O. Westwood, Hope Professor of Zoology, proposes to form a class for the study of the structure and classification of articulated animals.

Mr. W. Odling, Professor of Chemistry, on "The Succession of Chemical Ideas;" beginning Thursday, October 17. There will also be an explanatory and catechetical lecture on Tuesdays at 11 o'clock, to commence on Tuesday, October 22. The laboratory of the University will be open daily for instruction in practical chemistry from 9 A.M. to 3 P.M. on and after Monday, October 14. In addition to this two courses of instruction will be given in the laboratory—a course on the methods of quantitative analysis, and a course of elementary practical instruction in chemical manipulation, intended for those commencing the study of chemistry.

Mr. G. Rolleston, Linacre Professor of Anatomy and Physiology, on "Human Anatomy and Physiology, with special reference to Ethnology;" beginning Friday, the 18th of October. The work-rooms in the Anatomical Department are open daily from 9 A.M. to 5 P.M. for practical instruction, under the superintendence of Mr. Charles Robertson, the Demonstrator of Anatomy, and Mr. S. J. Sharkey, of Jesus College. A special class will be formed for instruction in Practical Microscopy. Mr. E. Ray Lankester, of Exeter College, will, as Deputy of the Linacre Professor, give a course of lectures on "The General Classification of the Animal Kingdom," beginning on the 19th of October.

Mr. J. Phillips, Professor of Geology, on "The Successive Conditions of Land and Sea, taken in the order of Geological Time;" beginning Monday, October 28.

The following are also announced in connection with Trinity, St. John's, and Sidney Sussex Colleges, Cambridge:—

On "Electricity and Magnetism (for the Natural Sciences Tripos), by Mr. Trotter, Trinity, commencing Wednesday, Oct. 16. On Chemistry, by Mr. Main, St. John's, in St. John's College Laboratory, commencing Thursday, Oct. 17. Attendance on these lectures is recognised by the University for the Certificate required by Medical Students previous to admission for the first examination for the degree of M.B. Instruction in Practical Chemistry will also be given. On Palæontology (the Protozoa and Cœlenterata), by Mr. Bonney, St. John's, commencing Thursday, Oct. 17. On Geology, (for the Natural Sciences Tripos. Preliminary matter and Petrology), by Mr. Bonney, St. John's, commencing Wednesday, Oct 16. A course on Physical Geology will be given in the Lent Term, and on Stratigraphical Geology in the Easter Term. On Botany (for the Natural Sciences Tripos), by Mr. Hicks, Sidney, beginning on Thursday, Oct. 17. The Lectures during this term will be on the Morphology of Phanerogamia. Mr. Hicks will also give examination papers in Botany to candidates for the next Natural Sciences Tripos, beginning Oct. 21. On the Physiology of the Organs of Sense, by Dr. M. Foster, F.R.S.; and a Course of Practical Physiology. The days, hours, and dates of commencement of these two courses will be announced shortly.

AMERICAN PREPARATIONS FOR THE FORTHCOMING TRANSIT OF VENUS

AMID the violent political agitation and the inevitable social commotion of the United States, one would imagine, judging from our own case, that neither the American Government nor the American people had any time or funds to devote to scientific objects of apparently remote utilitarian interest. That this is not the case every regular reader of this periodical must be aware, for seldom does a week pass but we have occasion to notice some scientific expedition fitted out by Government funds, or the meeting of some well-organised and efficient scientific association, or the report of work done at one of the numerous scientific schools with which the country abounds, or the results of an expensive scientific inquiry or scientific experiment; in short, the Americans seem to think it their interest and duty, as it is their inclination, to give substantial encouragement to scientific research and the spread of scientific culture and knowledge. Verily they know how to do these things better in America than in England; but, indeed, of what foreign country can this not be said? This cannot be better seen than in the action taken by the U.S. Government in reference to the forthcoming Transit of Venus.

In March 1871, Congress, instead of appointing one irresponsible official to organise all the preparations necessary for the observation of one of the rarest and most important astronomical phenomena, authorised the appointment of a Commission "to expend such appropriations as might be made by Congress for the observations of the coming Transit of Venus." This Commission is composed of Rear-Admiral B. F. Sands, Superintendent U.S. Naval Observatory; Prof. Joseph Henry, LL.D., President National Academy of Sciences; Prof. Benjamin Peirce, LL.D., Superintendent U.S. Coast Survey; and two Professors of Mathematics of the Naval Observatory, viz., Profs. Simon Newcomb and William Harkness. These gentlemen are all thoroughly qualified, both from their attainments and position, to perform the important and critical duties devolving upon them; and from their varied experience and knowledge, as well as from their differences of mental constitution and vision, they are more likely to do their work exhaustively and with thorough efficiency, than if their task had been committed to the absolute care of a single individual, no matter how well qualified he might have been. "In the multitude of counsellors there is wisdom."

The Commission have set about their work in a thorough and business-like way, and seem determined that America shall have no rival in the perfection of the preparations organised for making the most of the momentous astronomical event. They, however, do not grudge to give the world generally the benefit of whatever important conclusions may result from their inquiries and experiments. At a meeting of the Commission in July last, it was resolved to print such papers relating to the subject as might be of sufficient interest and importance. The first collection of these papers lies before us, and we shall endeavour to lay before our readers the gist of its contents.

The first article is a letter from Rear-Admiral Sands to the Secretary of the Navy, suggesting the advisability of asking Congress to appropriate the necessary funds for fitting out expeditions to observe the transit. Congress, it appears, in 1871 had made a small preliminary appropriation of 2,000 dols., but the Commission having decided that the total cost of carrying out the work in a fitting manner would be 150,000 dols., to be expended in three annual instalments, Rear-Admiral Sands requested the Secretary to procure for them the first instalment of 50,000 dols., which were to be almost entirely spent in the construction of instruments. Judging from the indorsement of the Secretary, it seems certain that the request of the Commission has been granted.

The next letter is from Rear-Admiral Sands to Mr. Lewis, Mr. Rutherfurd requesting his advice respecting the best method of applying photography to the determination of the relative positions of Venus and the sun during the transit. Mr. Rutherfurd replies by giving a detailed description of the method of solar photography employed in his own observatory, describing the form of photographic instrument he considers best adapted for the observation of the transit. He gives directions as to the construction and manipulation of the objective, the tube and focus, and the camera-box, which seem to be in all essential respects similar to those which have hitherto been found most efficient elsewhere. His opinion as to the best form of photographic instrument is, however, worth quoting. Mr. Rutherfurd says (p. 13):—

"If the whole matter of ordering instruments for the photographing of the transit of Venus were in my control, with my present lights, I should have an achromatic objective of 5in. aperture, and 70in. focus, in a cell which would allow of the application in front of it of a lens of flint glass of such curves as would shorten the focal distance (for photographing) to 60in. At the proper point I would place between the two distances an enlarging lens so constructed that the normal image of the sun in the principal focus (then about half an inch) would be enlarged to two inches at the distance of ten inches from the principal focus, viz., at 70in. from the objective. The camera box and tube should be one tube, and the focalising rack and screw should be located at the objective end of the tube, thus simplifying the whole arrangement, and permitting the use of braces from end to end to prevent flexure; and on taking off the photographic corrector, and taking out the enlarging lens, the instrument will be all ready for vision. On consideration I do not think I would counsel a smaller telescope than the one I have named,"

We are glad to see that Mr. Rutherfurd has consented to superintend the preparatory photographic constructions and experiments.

The last and longest, and perhaps most valuable, article in the pamphlet is by Prof. Newcomb, a member of the Commission, "On the Application of Photography to the Observations of the Transits of Venus." He speaks of the two methods which may be adopted for the purpose of observations. Of the first, which consists in fixing the moment at which the planet is in contact with the limb of the sun, he speaks in terms of strong depreciation, as almost entirely untrustworthy. The second method, and the one Prof. Newcomb recommends, consists in determining the relative position of the centre of the planet and the centre of the sun as often as possible during the transit. He then proceeds to examine some plans which have been suggested for the application of photography to this purpose, and to devise the combination among them which he thinks most likely to lead to the desired result. The objects to be attained he sums up as follows:—

1. To form an image of the sun with Venus on its disc of such a kind that from the outlines of the images the points on the photographic plates which correspond to the centre of the two discs, can be fixed with a high degree of precision. 2. The linear distance between these points being determined in millimetres, or other units of length, by means of a micrometer, we must have the means of deducing the angular distance to which this linear distance corresponds; or we must know the value of one millimetre in seconds of arc on each part of the photographic plate, and in each direction. 3. We must have a fixed line of reference on the plate, from which we can deduce the angle of position of the two centres relatively to the circle of right ascension passing through the sun's centre.

Prof. Newcomb then speaks of the necessity for the greatest possible accuracy in the measures; he thinks that, considering the accuracy with which the solar parallax can be found by other methods, we are justified in pronouncing it necessary that the errors at no one station rise to the $\frac{1}{10000}$ of the distance measured. In speaking of the size of image on plate, he assumes that the photographs must be taken by the "wet plate" process. As to size, he justly says that the test consists in the relative sharpness of the images; if it be found that a 2-in. image can be measured with twice the accuracy of a 4-in. one, it will answer an equally good purpose.

In reference to the modes of forming the solar image to be photographed, he thinks the only method that can be adopted is that devised by Prof. Winlock, which has been in successful operation for several years at the Harvard College Observatory, and which has been independently proposed by M. Faye, of the French Academy of Sciences. It consists in placing the telescope in a fixed horizontal position, while the sun's rays are thrown into it by a heliostat placed in front of the object-glass. After enumerating several of the decided advantages which he thinks it possesses, he proceeds to describe the appliances and methods by which the determinations are to be made in this system. What he says as to the heliostat we think very valuable, and shall endeavour to give a clear abstract of it.

If the reflecting surface of the heliostat be warmed by the rays of the sun, or if the two surfaces of the reflecting plates are unequally heated, then (1) the position of the effective optical centre of the angular value of the millimetre on the photographic plate, will be vitiated; (2) the image formed in the focus of the objective will be blurred. In considering effect (1), the problem is:—two rays from points in the heavens, at the angular distance γ, strike the reflector, whose radius of curvature is ρ, so as to meet after reflection near the optical centre of the objective; to find the difference γ' between these directions after leaving the reflector. γ' is the angle which will be measured on the plate, γ the angle we want. Call,

S, the distance between the points on which the rays strike the reflector.

A, the angle which the line joining the points makes with the plane normal to the axis of the telescope.

D, mean distance of the mirror from the objective.

$\delta\gamma = \gamma' - \gamma$, the error produced by the curvature of the mirror in the result of the angular measurement.

Then, $S = D \sin. \gamma \sec. A$.

$$\delta\gamma = \frac{2S}{\rho} = \frac{2D \sec. A}{\rho} \sin. \gamma.$$

Sec. A may be supposed to be unity. Since it is desirable that the error of $\angle \gamma$ should not exceed $\frac{1}{40000}$ of its value, it is desirable that we have $\frac{\rho}{D} > 80{,}000$; and since it is necessary that the error should certainly be within a limit four times as great as this, we must have $\frac{\rho}{D} > 20{,}000$.

It will probably be found that at most of the stations the reflector can be placed within a foot of the objective. If so, the limit outside of which the radius of curvature of the reflecting surface will be unimportant, will be 80,000 feet, and that within which it will be inadmissible will be 20,000 feet.

As to the second effect, that on definition, if the curvature of the reflector cannot be kept within the limit of 80,000 feet radius, or if any small deviations without it cannot be determined with certainty, a serious and fatal objection will arise to the proposed plan. The practicability of attaining this desideratum is the first thing to be determined, and it can only be determined by trial and experiment. The most necessary precaution is that the reflector should be exposed to full sunlight only at the moment of taking the picture. When it is found necessary to use the reflected light for adjustment, the heat rays must, as far as possible, be cut off by a blue or green glass. The necessary time of full exposure of the mirror need not be more than half a second, or a second at most, for each picture.

The most perfect arrangement for moving the reflector would be that of the "siderostat" of Foucault, in which the mirror is moved round two axes in such a manner that the reflected rays remain parallel as the sun passes along its parallel of declination by its diurnal motion, the change due to refraction excepted. The adjustment of the reflector must be made so that the direction of the reflected ray shall vary from that of the telescope as little as possible during the transit. The motion of the mirror must be free from all vibrations, and every instrument must be carefully tested for this condition before being used. To avoid all serious danger of vibration, Prof. Newcomb proposes that no toothed wheels shall be allowed in the moving machinery, but that all motion shall be communicated by fine and well-oiled tangent screws. Whether the mirror should be of plain glass, silvered glass, or speculum metal, is a question to be settled by experiment.

Prof. Newcomb then proceeds to give some valuable suggestions as to the objective, the tube, arrangements at the focal points, the exposing of the plate, determination of the planet's position on the sun's disc, and the angle of position. These are an admirable *résumé* of and criticism on the best results that have been hitherto arrived at on these points. His concluding remarks are worth quoting:—"The determination of the solar parallax from measures of photographs of the sun taken during the transit of Venus is beset with this serious difficulty. That the required element appears only as a minute difference between two comparatively long arcs, much longer, in fact, than are often measured with a micrometer. In order that the solar parallax may thus be determined with a precision exceeding that attained by other methods, it is necessary that the arcs in question be measured with a precision considerably exceeding any ever attained in the astronomical measurement of an arc of similar length. The difficulties of the operations are greatly aggravated by the direction and motion of the body to be photographed, which require the apparatus to be mounted on moving axes, and demand either an instrument of unwieldy proportions, or the use of an enlarging lens. In Prof. Winlock's apparatus the diurnal motion is thrown entirely upon the revolving mirror, so that all the advantages of a fixed horizontal sun are obtained. The apparatus is all firmly mounted on stone piers, thus admitting of exact measurement of all its parts, and avoiding all danger of changing the adjustments by the photographic manipulations. It seems to be that the advantages are all greatly in its favour."

We hope that the Commission will very soon be able to publish an equally, if not more, interesting and valuable collection of papers, containing the results of their own independent inquiries and experiments. We only hope that the preliminary work will be as efficiently done in other countries as there is every promise of its being done in the United States.

THE "HASSLER" EXPEDITION

We are again indebted to the *New York Tribune* for the following account of the final labours and total results of Prof. Agassiz's expedition:—

San Francisco, Cal., Sept. 2.—The steamer *Hassler* reached Acapulco on Sunday evening, Aug. 4, and remained 70 hours. The fishermen of the place were very active, and our own scientific party were not behindhand in diligence, so that these 70 hours yielded the Professor as rich a harvest as he has gathered in almost any port. Acapulco is a lovely Sleepy Hollow; its quiet little bay completely enclosed by beautiful mountains; its environs adorned with a profusion of tall cocoa-nut palms; the promenade from the town to the fort, half a mile distant, shaded by magnificent old lime trees; the town itself clean, old-fashioned, quiet; only three or four vessels in the port. If it had not been for the heat, we should have voted it the loveliest imaginable retreat. Two of the vessels in port were English, and I had one or two pleasant interviews with their captains. As we were parting, I mentioned to one of them that I had long wished to visit England. His answer was pre-eminently English:—"He thought a visit to England would be useful to me; it might remove some prejudices and hard feelings." Now, as I am absolutely certain, and do positively know, that I had not betrayed to him in any way or manner the least shade of prejudice or hard feeling toward the mother country, I must explain his remark by supposing that he was himself conscious of hard feelings toward the United States; and therefore presumed that I felt them toward England. The confidence with which an Englishman applies his English foot-rule to measure the universe is a very marvellous thing; it is as if he thought that the laws and customs of his little island are universal laws of humanity, and he seems incapable of supposing it otherwise.

We left Acapulco August 7. The scenery as we went out of the bay, passing between the islands and the main land, and for several miles after emerging into the Pacific, was exquisitely beautiful. The high hills behind the town reminded me of paintings which I have seen of Hymettus seen from the hill of the Museum. We have hardly seen on the whole voyage anything more picturesque and beautiful. The evening closed with a magnificent sunset. South of the sun were long streamers of golden clouds, and just north of it was a patch of the bluest imaginable sky, broken by three or four projections of brilliant cloud. Our views toward the north were lovely beyond description; the sea nearer to us was the deepest blue, toward the shore becoming purple; then came a long golden beach; behind that deep green hills; behind these a line of purple hills; still farther back blue mountains, and then over all a series of clouds of varying shades. On the evening of the 9th, off Cape Corrientes, we had a heavy shower, a thing that six weeks in the rainy reason had rendered familiar to us. But the next morning we were in a different climate, cool, dry, and pleasant; and gliding on smooth seas, we reached the western edge of the Gulf of California on the evening of the 11th. By noon of the 12th we had a strong head-wind which seemed positively cold after the sweltering heats of Panama and Acapulco. At sunrise on the 13th we anchored in Magdalena Bay, where we remained thirty hours, seeing only the two great islands which form the outer defences of this magnificent harbour. We found here a small colony gathering orchilla, a lichen (*Roccella*) from which cudbear is made. The plant only grows in comparatively rainless regions, and grows very slowly, so that the gathering of a crop leaves the field barren for many years. The bushes on which this lichen grows are of but few species, and most of them of very odd appearance. The animals in the sea were very interesting, and our thirty hours yielded us a rich harvest.

Good weather and favourable winds brought us into the harbour of San Diego by noon on Sunday, August 18. We had not been here long before a telegram ordered the *Hassler* to return to the Mexican coast and sound for a rock reported to have been seen in a certain place. The *Hassler* obeyed, and was gone several days, searching for a rock which probably does not exist, the scientific party meanwhile remaining in San Diego. It was a delightful place for the naturalists and for us all. It was our own country, and we were at home; and among hospitable people who at once made us feel at home. A few Chinese (washers and ironers and fishers) seemed to be the only low people in the place, if we except a few Indians in tents in the adjoining fields. All the rest—I speak of the new town—seemed to be industrious, respectable Americans, Germans, or Spanish. The harbour is a long crescent. The protection is from a long range of hills running southward in a promontory to the west, and two flat islands on the south connected with each other and the continent on the east by a narrow strip of sea beach. On the north side of this crescent are numerous little villages, two of which, Old San Diego and New San Diego, are of considerable importance. In the new town two daily papers are published, and a steamer leaves five times a month for San Francisco. While we were there the town was intensely excited over the arrival of Col. Scott and other railroad magnates, to make arrangements for the commencement of work on the western division of the Texas and Pacific Road. The town has been built in faith that a railway communication with the Atlantic must at no distant day be opened with this the best harbour in the southern part of California. But hope-deferred had begun to make the heart sick. Those who had not means of living had begun to consider the expediency of retreating to some place of greater activity. But the visit of Col. Scott, and the arrangement made by him with the citizens of the town, have put every one at San Diego into high spirits, and they look forward now, I think reasonably, to the rapid growth of their city.

The harbour is excellent. It needs some care to prevent the San Diego River from filling it with sand, to prevent the ocean from breaking the beach that connects the island, and thus obviate the present "scour" in the main entrance; and to prevent wharf and other "improvements" in the distant future from doing the same mischief. The situation of the town is fine, on a gentle slope, with a hard pan foundation for building. The climate is wonderfully equable, it is rather too dry, but windmills are cheap; the direction of the wind is so uniform that the windmill need only be set for west winds; and with a windmill to irrigate one can raise any crop. Many plants, as olives, figs, grapes, &c., only need irrigation for a time and then strike root deep enough to reach perennial moisture. Frosts come only at intervals of many years and are then exceedingly light. We ate tomatoes gathered from bushes that had yielded fruit freely every week in the year for three years past. The melons were of an excellence surpassing anything I have ever tasted. The city is well laid out, and the nucleus of citizens already there is of sterling quality. The Horton House, which is the principal hotel, is admirably kept in the neatest and most comfortable style, with gas, water, and other conveniences, and a good table. One can make oneself at home there as well as in any city of larger size. I met also many persons in private at whose houses I had evidence that some of the best fruits of English and German, French and Spanish civilisation are acclimated here. In zoology our naturalists found a rich field. Fifty-three different species of fish, and sixty or seventy species of other animals, were added to their collections, many of these species being of very rare and valuable kinds, and several probably new. Most of these were found in such abundance that the Professor could take just as many as he chose, as many, that is, as he thought he could make useful at home.

On August 28 we parted with real regret from our new-made but most cordial and hospitable friends at San Diego, and, being again greatly favoured by the weather, we made the Golden Gate on the 31st at sunrise, and dropped our anchor in the harbour of San Francisco at 9 o'clock. The expedition proper here ended, but Prof. Agassiz, with Dr. Steindachner, will remain to gather what they can in this harbour before returning. Their success during the whole voyage in collecting valuable specimens of fish and other animals has been truly wonderful; new and unknown species have apparently been everywhere awaiting their arrival to reveal themselves; rare and valuable fishes have come freely and in numbers to give themselves up, and the more ordinary species have come into their nets in superabundance, so that we have thrown back living into the sea very frequently more than half of what the seine brought up. The whole number of fish brought home from the voyage will probably exceed 30,000, and the other animals of all descriptions will probably swell the number of specimens brought home to over 100,000. It is, however, the quality and kinds that give value to their collection rather than the mere numbers; and the *Hassler* Expedition will have prominent place in the history of zoology, because of the number of new species discovered, as well as for the valuable collection of materials on which original anatomical investigations may hereafter be made. In the history of physics the exhibition will also be remembered, not for the deep-sea dredgings which circumstances beyond the control of the officers of the vessel prevented it from making, but for the valuable geological observations made for the first time in the south temperate zone by an observer thoroughly conversant with the action of glaciers and the glacial sheet north of the equator; the observer who first detected the marks, now apparent to every eye, which demonstrate the existence of glacial sheets before the birth of the present glaciers, even in their most extended form. During nine months the little company have received the courteous attention of the officers of the *Hassler*, and enjoyed the rare privileges which the Superintendent of the Coast Survey and Secretary of the Treasury had granted; nine months of continuous and varied enjoyment. The *Hassler* came round South America to survey the Pacific coast of the United States, but the long voyage has not been idle. It has been employed incidentally in a manner not less valuable than the work to which the vessel is specifically devoted.

ON THE FERTILISATION OF A FEW COMMON PAPILIONACEOUS FLOWERS

II.

VICIA SATIVA.—In the general structure and character of petals, stamens, and pistil, this flower agrees with Lathyrus; but there is a remarkable difference in the shape of the keel, and correlatively in the hairs or brush on the style.

In Lathyrus, as we have seen, the upper part of the elastic style is curved, so that the curvature corresponds with the curvature of the keel; the back or outside of the style presses against the inside of the keel, and is not furnished with hairs, there being no space for pollen on that side, whilst the inside of the style is covered with hairs set upwards, so as to sweep out the pollen which accumulates on this side.

In *Vicia sativa* the keel forms a less regular curve, whilst the style, instead of following the curvature of the keel, is set on at right angles to the ovary, and is straight and perpendicular throughout its whole length. There is, therefore, a large nook or corner outside the style, and between it and the keel, into which the pollen gets. Correlatively the style is not furnished with abundant hairs on the inside, as in Lathyrus; but there is a little tuft of stiff hairs on the outside, a little below the stigma, set on upwards so as exactly to brush out the pollen from the nook of the keel, when the keel is pressed down by an insect (see Fig. 9).

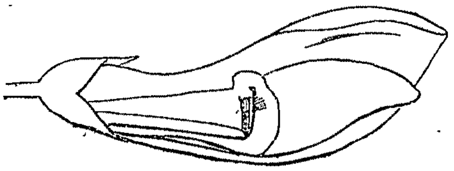

FIG. 9.—*Vicia sativa* (keel and pistil).

Vicia sepium is similar in construction to *V. sativa*. I have not observed whether there is nectar within the staminal tube of *V. sativa* or *V. sepium*, but feel assured that it is to be found there.

Vicia Faba. In the several positions of its buds and pods, Faba (broad bean) differs from Pisum and Lathyrus, and agrees with Phaseolus. The buds are upright; in the flower they are horizontal, and in the pod they are again upright; but the blossom, when open, is, as in all the other cases, horizontal, so as to afford a good lighting place for bees which seek the nectar in the interior of the staminal tube. This tube, the separate stamen, the shape of the keel of the style with its brush, are similar to those of *V. sativa*.

Robinia pseud-Acacia.—This plant bears its flowers in a pendent raceme; consequently, the position of the flower is reversed. The fifth sepal should be uppermost, and the vexillum lowest, with its back to the peduncle; and this is the position of the unopened buds. But as they approach maturity, *i.e.* as the blossom opens, the pedicel of each flower takes a half twist, so as to bring the flower into what we may call the normal position of Papilionaceæ, but with the vexillum uppermost and upright, or nearly so, and the wings and keel horizontal, the open side of the keel being uppermost. The keel is obtuse, and is free from the wings.

The filament of the tenth stamen is joined to the others in the middle, with apertures between it and the others at the base, and there is a cavity at the base of the staminal tube containing nectar. The stigma has a very small brush round it, and there are a few hairs on the inside of the style which seem to sweep out the pollen. The flowers are much frequented by bees.

Wistaria sinensis.—The pendent raceme of this plant displays, as regards the position of the buds and flowers, the same features as that of *Robinia pseud-Acacia*, which it also resembles in the free boat-shaped keel, the semi-separated tenth stamen, and the nectar-holding cavity of the staminal tube. It differs in having no hairs on the style, a difference possibly connected with the character of the pollen. But as the flower does not usually produce seed in this country, it seems unsafe to speculate on such a point.

Onobrychis sativa, or Sainfoin.—In the long raceme of this plant, the pedicels of the flower are nearly perpendicular in the bud, horizontal, as usual, in the flower, and again, after flowering, resume a position approaching the perpendicular. The wings are very small, and are not attached to the keel, and seem to play no part in fertilisation. On the other hand, the keel is large, boat-shaped, prominent, and being joined together to the apex, and having the petals folded over one another when not joined, affords a broad and easy alighting place for insects. The tenth stamen is separate at the base, and the staminal tube so formed that it may contain nectar. Whether it does so or not I have not observed. The filaments are stiff, and the pollen sufficiently dry and dusty to come out in abundance on pressure being applied to the folded top of the keel. The stigma comes out first, and often remains outside the keel, whilst the stamens, on pressure being removed, resume their position.

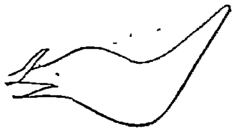

FIG 10.—*Lotus corniculatus* (keel).

Trifolium repens (Dutch Clover).—These flowers, being in an umbel, afford a good foothold for bees, and do not require an alighting place on each flower so much as in the case of larger and separate blossoms. Nevertheless, they are upright in the bud, inclined in the flower, pendent and recurved after blossoming. No flowers are upright in full blossom, and consequently the centre or summit of the umbel becomes bare. The flowers thus tend to the usual position, even though in an umbel.

The claws of the wings and keel are united, and form a half tube, containing within them the staminal tube. The tenth stamen is perfectly free, and the staminal tube, as usual in such cases, contains nectar. Bees are fond of the flowers, and must, in entering the half tubes of the keel and wings, meet the stigma and carry away pollen.

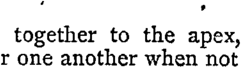

FIG. 11.—*Lotus corniculatus* (dilated filament)

Trifolium pratense.—The position of the flowers in the umbel changes as in *T. repens*, though in a less marked manner.

The long claws of all the petals, including the vexillum, are united so as to form a complete tube, at the bottom of which is much nectar. The limb of the keel is open at the top, but the aperture is small, so that an insect entering cannot fail to touch both stigma and anthers.

The filaments of the nine stamens cohere to one another, and to the tube of the corolla from the point of union of the petals, so that there is no separate staminal tube. The tenth stamen is entirely separate for its whole length. Looking to the course of the apparent veins of the petals and stamens on the tube, it seems as if the vexillum really formed the tube, and as if the nine united filaments of the stamens by themselves would leave a large aperture, and were widely separated from the tenth stamen. If so, it is curious to see the nectar-holding cavity so often formed by the stamens here formed by the vexillum. The entire freedom and wide separation in the tenth stamen, in a flower displaying such a tendency to cohesion, is also curious. Possibly this is necessary in order to preserve a sufficient aperture to give access to the nectary.

Lotus corniculatus.—The flowers of this plant again, though in umbels, when open assume the normal position with the vexillum uppermost.

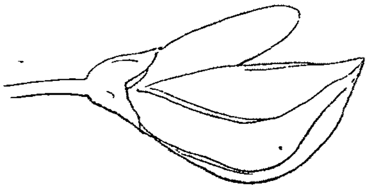

FIG. 12.—Lupin (flower with one wing cut off).

The wings are free from the keel. The keel is long, pointed, and united for some distance above as well as below, with an aperture at the apex (see Fig. 10). The tenth stamen is free, and is separate from the others at the base; the staminal tube is stiff and enlarged at the base into a cavity, which contains nectar. The pollen is moist and abundant. The style is capitate and stiff, but without hairs or brush. How then can the moist pollen be forced out of the narrow mouth of the long pointed keel so as to meet an entering insect? In a very curious way. Five of the stamens, viz. those of the inner whorl, are shorter than the others, and their filaments are dilated at the top. These filaments are stiff, and, I believe, continue to grow after the five anthers of the other whorl have shed their pollen. The dilatation of the filament is wedge or club-shaped, the broad end of the wedge being uppermost (see Fig. 11). Consequently, on any pressure being applied to the keel, the broad ends of these wedges, supported by their stiff filaments, collect the pollen, and push it before them to and out of the mouth of the keel, where it is seen to adhere to the body of the insect which is passing down the keel. It is to be observed that the shape of the dilated filament is such that, whilst pollen might work past it from below upwards, the broad flat upper end of the filament meeting the narrowing tube of the keel can scarcely allow it to pass downwards.

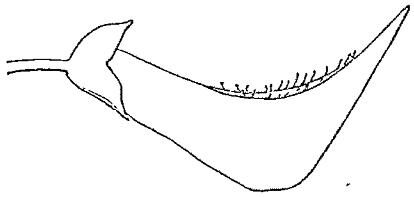

FIG 13 —Lupin (keel).

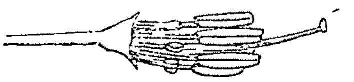

FIG 14 —Lupin (Stamens in bud)

Garden Lupin (common tall blue and white).—In the long raceme of this plant the pedicels are nearly perpendicular in the bud, become horizontal whilst the blossom is open, and rise so as to approach the perpendicular again afterwards. The wings are attached to each other below, are blunt, and are folded over at top so as to afford an excellent resting place. They are not attached to the keel, and move downwards more easily than it does. The keel is very long, very pointed, and the upper edges are slightly connected with an opening at the apex so as to form an approach to a tube. The apex just appears between the wings. The upper edges of the keel are furnished with a few hairs (see Figs. 12 and 13).

The filaments of the stamens are entirely joined together so as to form a close fitting tube round the ovary. There is no cavity within the tube for nectar, no apertures into it at the base, and it is too long and too close fitting for an insect to thrust its proboscis down.

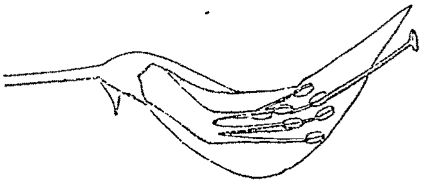

FIG. 15 —Lupin (keel with one side cut off).

There is a cavity at the back and base of the vexillum in which I have not been able to find nectar. But the bees, which constantly visit these flowers, certainly go to this cavity for what they want, and not to the staminal tube. Five of the stamens compose, I believe, what must be the outer whorl, are longer in their filaments than the other five, and have longer anthers (see Fig. 14). These are mature, and before the flower opens have shed their pollen, which remains in a moist mass towards the mouth of the pointed keel. Their filaments then wither and contract. The other whorl of stamens are shorter, and the anthers much smaller, but they are later than the first whorl, and their filaments grow and remain stiff after the filaments of the first whorl have withered. They consequently, on pressure being applied to the keel, thrust the mass of pollen upwards to its mouth. The style is long, and a ring of hairs surround the stigma, of which the upper and inner are the longest, and all of which are set upwards, so that on pressure being applied to the keel the hairs sweep out the mass of moist pollen which the stamens have thrust to the mouth (see Figs. 15 and 16). It is quite pretty to watch the little stream of bright orange pollen emerging from the narrow aperture of the blue keel, and between the bright blue wings.

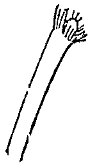

FIG. 16.—Lupin (style and stigma).

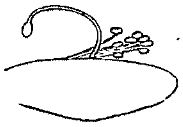

FIG. 17 —*Ulex* (Gorse).

The shorter yellow and blue garden lupins and *Lupinus arboreus* are similarly constructed. In the latter flower the folding over of the wings at the top, and the cavity at the base of vexillum, are strongly marked.

What is the use of the hairs on the edges of the keel?

Ononis arvensis.—The vexillum in the opened flower is perpendicular or a little bent back; and the wings, which are small, are also perpendicular, so that an insect may light either on the vexillum or on the wings, and has to thrust itself between the vexillum and the wings.

The keel is long and pointed as in Lotus and Lupin: and the stamens push out the pollen as in those flowers.

The stamens are quite monadelphous, the staminal tube is close fitting, and there is no nectar and no space for nectar within it. The humble-bee certainly does not put his proboscis down the tube, but between the tube and the vexillum.

Anthyllis Vulneraria.—This flower being in an umbel needs no peculiar position to give insects a foothold. Its peculiarity is that the large calyx, the sepals of which cohere up to their narrow mouth, forms a dilated tube or vessel which contains abundance of nectar. The limbs of the wings are attached to the keel, but the claws of all the petals are long, narrow, threadlike and perfectly free, so as to leave free access to the nectar when the proboscis of an insect has once passed the mouth of the flower.

The aperture between the vexillum and the coherent wing and keel is however very narrow, so that an insect in passing it cannot fail to push the keel outwards and bring out the stiff style and stamens. The filaments are entirely joined together, and form a long close fitting tube in which there is space neither for nectar nor for the proboscis of an insect.

Ulex nanus (Autumn Gorse.)—This flower is upright in the bud, assumes the usual horizontal position when in blossom, and reverts to the upright position in the pod. The bud is protected by a stout, large and hairy calyx, and the pod is stout and hairy.

The wings are perfectly free from the keel, and the rounded lobes of the keel are separate from one another at the extremity and for a considerable part of the lower side, so as to make the flower comparatively open. The stamens and pistil are stiff, and come out on the keel being pressed down; and the pollen, which is dusty, comes out in a cloud.

The staminal tube is perfectly closed and close fitting. There is no cavity in it for nectar, and no aperture at the base. There are traces of nectar on the veins and in the hollows of the vexillum, especially on the midrib and in the hollow towards the base. The bees are fond of it. They settle on the keel and thrust their heads between it and the vexillum, pushing the latter upwards. In struggling to do this their legs are in violent motion on the top of the keel, pressing it down. In doing this they invariably open it, make the anthers project, and dust their own body with pollen ready to meet the stigma of the next flower (see Fig. 17).

The contrast between the free wings, the obtuse and semi-attached lobes of the keel, the stiff filaments, the hairless style, and the dusty pollen of Ulex, and the adherent wings, closed keel, moist pollen, and brush-clad style of Pisum, Vicia, Lathyrus, &c., and the correlation between these differences, having regard to the ultimate object in both cases, viz. the conveyance of pollen by an insect, are very striking.

Ulex Europæus is similar to *Ulex nanus*, and I have seen bees upon it in April. But are there enough of these insects abroad during the winter season, when this flower blossoms, to fertilise it?

Genista Anglica.—The wings are separate from the style, the keel is straight and horizontal, but is reflexed after maturity, probably when it has once been visited by an insect. The anthers have stiff filaments and dusty pollen, all of which is shed when the flower is once opened.

The style is stiff, and coils back on the opening of the keel, whilst the stigma is oblique. When so curved back the stigma would touch an entering insect. I have not ascertained where the nectar is in this flower, but probably not in the closed and close-fitting staminal tube.

Sarothamnus (Broom.)—The keel is perfectly free from the wings, is obtuse and closed when the flower first opens. In this stage the style is bent against the keel in such a way that its stigma (which is at the extremity) is turned away from an entering insect. At a touch the keel opens and falls down by a sort of hinge, and does not recover its position. The stiff stamens shed their dusty pollen, as in Ulex and Genista. The elastic style at the same time coils itself inwards towards the vexillum. In a few moments it has curved back so far as to complete one spiral coil, and bring the stigma round so as to meet an insect subsequently entering. In opening the flower with the finger or a pencil, the stigma does not catch its own pollen, but after recoiling can hardly fail to rub the next body which enters.

The staminal tube is complete, and there is no space for nectar or aperture into it. I have not ascertained where the nectar is to be found, but not, I think, in the thin, close-fitting staminal tube.

Cytisus (*qu. nigricans?*)—common in London greenhouses.—The raceme is terminal; the peduncle is nearly horizontal. The pedicels are set on all round the peduncle, but in blossoming are so bent upwards as to make the vexillum of each flower nearly upright, and the keel and wings nearly horizontal, *i.e.*, so as to bring the flowers into the normal position.

The separation of the petals, the reflexion of the keel, the closed staminal tube, and the dusty pollen, are the same as in Sarothamnus and Genista. There is no nectar, and no place for any in the staminal tube.

The above details seem to point to some generalisations concerning papilionaceous flowers.

1. The position of the flowers in blossom, whatever their other wants and habits, is such as to make them attractive and convenient to insects. In general the showy vexillum is upright, and the keel and wings horizontal. This is effected in various ways: by the raising and straightening of the stalk, as in Pisum and Lathyrus; by the lowering of it, as in Faba, Phaseolus, and Ulex; or by giving the pedicel a half twist, as in Robinia, Wistaria, and Laburnum.

2. The cohesion of the petals (which in this single tribe is so various) is in each flower correlated to the position of the nectary, the structure of the fertilising apparatus, and the nature of the pollen. Thus in Ulex, Genista, and Sarothamnus, the cohesion of the petals is at a minimum, the wings do not adhere to the keel, and the keel itself is comparatively open. Correlatively the filaments are stiff and the pollen dusty, and the insect gets freely dusted with it, without aid from any union of the petals. In Pisum, Lathyrus, Vicia, Phaseolus, and others, the wings not only serve as a landing place for insects, but, being united to the keel, serve to pull it down and force out the pollen. In Trifolium, the coherence of the petals is at a maximum, and produces a complete long tube containing much nectar, and having the organs of fertilisation in the access afforded by its narrow mouth. In Anthyllis the claws of the petals are so thin and so free from each other as to afford no receptacle for nectar, whilst the staminal tube is closed and tight fitting, but the want of a nectary is made up by the cohesion and form of the calyx. The various degrees of cohesion between the petals of the keel—from the comparatively free keel of Genisteæ, through the prolonged acute keels of Lupin and Lotus, and the oblique keel of Lathyrus, to the spiral tube of Phaseolus—and the adaptation of each of these forms to its own stamens and pistil, is no less remarkable.

3. The degree to which the cohesion of the stamens is carried (so remarkable a feature in this tribe) appears to depend on the necessity for access to nectar. In

those flowers in which the stamens are monadelphous, viz., Ulex, Sarothamnus, Genista, Cytisus, Ononis, Lupin, there is no symptom of nectar within the staminal tube, no space for it, and no access to the interior. In some, at any rate, of these, viz., Ulex, Ononis, and Lupin, the bees certainly resort to other parts of the flower. On the other hand, where the tenth stamen is entirely free, or where it is separated at the base, so as to give an insect access to the interior of staminal tube, as in all the other flowers I have described, there is a cavity for nectar within the staminal tube, and there is nectar within this cavity.* As regards the double aperture, viz., one on each side of the base of the separate stamen, which so often occurs, Mr. Darwin suggests that, one aperture being necessary, the law of symmetry will account for there being two.

4. Other points in the structure of the filaments, anthers, and pollen seem also to be more or less related to and to depend upon the same function of fertilisation by insects. In Ulex, Genista, and Sarothamnus, where the flower is open, and in Lupin and Lotus, where the agency of the filaments is required to drive the pollen out of the keel, the filaments are stiff. In Phaseolus, where the style performs this function, they are limp. In Lotus and Lupin, the peculiar form and growth of the second whorl of stamens, and their adaptation to this function, is most remarkable. In Pisum, Lathyrus, Vicia, Phaseolus, and Lupin, where the pollen is moist, there is an apparatus for sweeping it out. In Ulex, Genista, and Sarothamnus, where it is dusty, the flower simply opens and it comes out of itself.

5. The structure of the style and stigma is in every case adapted so as to bring the latter in contact with an entering insect. In some cases, *e.g.*, in its emergence from the spiral keel in Phaseolus, and in the recoil of the style in Sarothamnus, this is effected by a very elaborate process. But the most peculiar function of the style in many of these flowers is that of sweeping out the moist pollen of its own flower from the keel. For this purpose it appears to be furnished with hairs or bristles, placed in different flowers on different parts, but always so placed as to perform the function in question. In Pisum, and, generally, in Lathyrus, the brush is on the inside of the style; in *Lathyrus grandiflorus* on both sides; in Phaseolus all round the style, but more thickly on the side next the entering insect than on the other; in Vicia on the outside of the style; in Lupin at the very extremity; but with all these differences it is always so placed as to find the pollen and sweep it out of the variously constructed keels. In this respect these flowers remind one of the brush-clad styles of the Campanulaceæ.

6. It is scarcely necessary to repeat that the nectar is found in various parts of the flower—within the staminal tube, in the vexillum, and in the calyx. But in all cases the correlation of the parts is such that an insect seeking the nectar must touch the stigma and carry away pollen.

These generalisations, if even partially correct, seem to me to be of considerable interest, not simply as illustrations of the mode in which insects fertilise flowers by carrying pollen from one to the other, but because by connecting the facts of morphological structure with living physiological functions, they give meaning and interest to the former, and possibly indicate the direction in which the true cause of that structure is to be sought.

It is but right to add that there is one genus, *Coronilla*, which, so far as I have been able to observe it, forms an exception to the above generalisation; but I have not been able to procure sufficient flowers to enable me to state any positive conclusion with respect to this genus; and I only mention it in order to call the attention of other observers to it.

T. H. Farrer

NOTES

During the absence of Prof. Tyndall in America, the opportunity is being taken to rebuild the laboratories of the Royal Institution on a considerably enlarged scale.

It will be seen from our University intelligence that Mr. E. Ray Lankester, Scholar of Exeter College, Oxford, has been appointed Deputy to the Linacre Professor of Anatomy and Physiology at the University.

The open Scholarship in Natural Science at St. Mary's Hospital Medical School has been awarded to Mr. Alfred Tilley, and the Exhibition to Mr. W. H. Weddell. Both these gentlemen are students of the London University.

There are now no fewer than five separate organisations at Cambridge for the improvement of female education—all of them thriving. 1. The examination of women, senior and junior girls, and of schools managed by a syndicate, of which the Rev. G. F. Browne, M.A., St. Catherine College, is the secretary. 2. A system of lectures for women, associated with four exhibitions, and a fund for assisting governesses, managed by a mixed committee of ladies and gentlemen, of which H. Sidgwick, M.A., Trinity College, and Mrs. Bateson, St. John's College Lodge, are the treasurers. 3. A series of classes by correspondence arranged by Mrs. Peile. 4. A lending library for students, managed by Miss J. Kennedy. 5. A college for women, called Merton Hall, of which Miss A. J. Clough is the principal. We understand that this last establishment is rapidly filling. The lectures commence this week.

The Vestry of St. George's, Hanover Square, advertised some time ago for a medical officer of health and analyst for the parish, and a considerable number of candidates have, we understand, come forward. It has been suggested in various quarters that the Vestry would do well to appoint two officers instead of one; and on this point minds are divided. While some are in favour of a double appointment (with, we suppose, double pay?) others say that the Vestry are not likely to do this, and that it is undesirable that they should, seeing that their real want is an accomplished scientific sanitarian, who will, if necessary, appoint an assistant to do the routine chemical work, just as he has an inspector to do the routine sanitary work, but who will supervise everything and be responsible for everything. It is further urged that it is absurd to suppose that chemical knowledge is not continually required from a medical officer of health, quite apart from the provisions of the Act for the Adulteration of Food and Drugs, and equally absurd to suppose that a medical man without previous special sanitary experience is at all fitted to become at once medical officer of health to so important a parish as that of St. George's, Hanover Square. We confess we have a leaning to the latter view.

At the last meeting of the Council of the Pharmaceutical Society, it was resolved unanimously that the resolution passed in 1862, prohibiting ladies from attending the lectures, be rescinded, and that ladies be admitted as students to the lecture classes of the Society. At present but one lady has taken advantage of the privilege offered; but as soon as the resolution becomes more widely known it is probable that the liberality of the Society will be recognised by ladies, who will avail themselves of this excellent opportunity of studying practical chemistry and botany. The lectures on chemistry are by Prof. Redwood; those on botany by Prof. Bentley, commencing early in October. The chemical lectures are continued three days a week until the end of July; the botanical lectures, lasting for the same period, being delivered on two days in the week. During the summer months they are delivered in the Botanical Gardens, Regent's Park.

A Russian lady, who desires to be anonymous, but is rumoured to be "still very young, and a native of Siberia," has offered 50,000 roubles for a medical course for ladies, to be given at the Imperial College of Physicians. Classes are to be formed ostensibly for midwifery, but this will not exclude the higher studies of medicine. The course is to be one of four years' duration. The threat from Zurich no longer to admit the "unprepared Russians" proves thereby a wind that blows somebody some good. "I hear," says the correspondent of the *School Board Chronicle*, "that Madame Souslof's practice at St. Petersburg is actually undermining that lady's health."

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* I have not actually looked for and found nectar in *Onobrychis sativa* and *Lathyrus macrorhizus*, but have no doubt that it is there. I have found it in all the rest.

THE nationalities represented at the International Commission on the Metric System recently sitting at Paris, are the following:—Great Britain, Germany, Austria and Hungary, Bavaria, Belgium, Denmark, Spain, France, Greece, Italy, Holland, Portugal, Russia, the Papal See, Sweden, Norway, Switzerland, Turkey, Wurtemburg, United States, Chili, Argentine Republic, Colombia, Equador, Hayti, Nicaragua, Peru, San Salvador, Uruguay, Venezuela. We believe, though it is a fact not generally known that Her Majesty's enlightened Government at first refused to allow England to be represented!

THE foundation stone of the New Watt Institution and School of Arts, Edinburgh, was laid on Wednesday, 9th inst., in Chambers Street, the spacious new street which runs on the north side of the College, and in front of the Industrial Museum.

THE vestry of Lambeth have appointed Mr. James Muter, F.C.S., analyst to the borough.

DR. J. E. EDDISON will deliver a course of eight lectures on "The Physiology of Circulation and Respiration," in connection with the Leeds Philosophical and Literary Society. The lectures will be strictly elementary, and as much practical illustration as possible will be introduced. The following single lectures will also be delivered during the ensuing season:—"Hill and Valley Sculpture," by Prof. Archibald Geikie, F.R.S., November 5 and 7; "The Meteorology of the Sun in connection with that of the Earth," by Prof. Balfour Stewart, F.R.S., December 3; "Radiant Light and Heat," by Prof. Balfour Stewart, December 13; "The Sense of Hearing," by Michael Foster, M.D., F.R.S, January 21, 1873; "The Primitive Social Condition of Man," by E. B. Tylor, F.R.S., February 4; "The Exploration of Moab," by the Rev. Canon Tristram, F.R.S., March 4; "On some new Phenomena associated with Magnetism," by W. F. Barrett, March 18.

THE following is the syllabus of the twenty-third session of the Manchester Scientific Students' Association:—The Physiography of Europe during the Pleistocene Age, by W. Boyd Dawkins, F.R.S., October 14. On the Glandular Hairs of the Fraxinella, Nettle, and Malpighia, by Charles Bailey, October 21. On Comparative Anatomy, by Herbert W. Oakley, October 28. On the History of a Mountain, by John Plant, November 4. On Meteors and Meteorites, by Rev. Joseph Freestone, November 18. On Horology, by Thomas Armstrong, December 2. And the following syllabus of papers is announced to be read at the Microscopical Club:—Glandular Vegetable Hairs, first paper, by Charles Bailey, October 3. On some Improvements in Oxy-hydrogen Illumination as applied to Microscopic Objects, by John Barrow, October 24. The Tetraspores of the Algæ, by John Hardy, November 14. The Polyparies of the British species of Hydrozoa, by Thomas S. Peace, November 28. The Micro-Spectroscope, by John Angell, December 12.

DURING the latter part of September, we learn from the *Times of India*, Bombay was visited by terribly destructive rains, causing not only serious injury to property, but great loss of life. Among the many instances of destruction on September 19 was that which occurred at the Library of the Asiatic Society—the largest collection of books, perhaps in India. The library is located in a series of rooms in the northern wing of the Town Hall, and before the commencement of the monsoon the root had, as usual, been inspected, and, as it was supposed, made thoroughly water-proof. Whether owing to bad repairs, or to the excessive force of the downpour, the water found its way into one of the rooms—the room in which the librarian has his office—and completely saturated and more or less destroyed about three thousand volumes, chiefly works on jurisprudence. The expensive illustrated books, which are kept in the same room, fortunately escaped the general drenching. The injured volumes were spread out in the Town Hall to dry, but it is feared that the larger number of them are totally destroyed.

AURORÆ BOREALES were visible on the 3rd of August at Stettin and Cracow, on the 4th at Emden, on the 8th in North America, on the 9th at Emden and Thurso, on the 15th at various localities in England and Stettin. On the 7th of August a smart shock of earthquake was felt at Innsbruck, which was followed by three more on the 8th of the same month.

ON the 9th and 10th of September a severe hurricane passed over the islands of Guadaloupe, Martinique, Dominica, St. Kitts, Barbadoes, &c. Sixteen vessels, including the steamer *Isleman*, was wrecked at Martinique, and several lives lost. Every vessel in the port of Dominica was struck to pieces, and there also many lives were lost. Several ships were driven on shore at St. Kitts. The gale lasted all day on Tuesday, the 19th instant, the barometer commencing to fall from ten o'clock on the previous morning.

THE following is from the *Athenæum*:—A singular controversy has occurred at Constantinople. The Government have determined that instruction in the Imperial School of Medicine shall be given in Turkish, and have removed all the professors who cannot speak the national language. Of course this has occasioned an outcry on the part of the friends of those French-speaking professors who have spent many years in the country and have not chosen to acquire its language. The Turks say they started their school as a national school, and not as a foreign one; that the pupils receive inadequate instruction from its being conveyed in a foreign language; and that they have not been supplied, as they expected, with manuals in Turkish. The authorities have, therefore, determined to run the risk of the change, and attempt to get for this school, as for others, books and teaching in the vernacular. They maintain that, as medicine has for ages been taught in Arabic, it can be taught in Turkish.

THE *Moniteur Scientifique* informs us that at Proskau, in Upper Prussian Silesia, near the Prussian-Polish frontier, an agricultural college on a large scale has been established by the State, in which everything relating to agriculture, horticulture, arboriculture, and the rearing of cattle, horses, bees, and poultry is practically taught. In addition to several smaller lecture-rooms, there are two large amphitheatres, which will accommodate 200 students each; three separate chemical laboratories; a large distillery; beetroot sugar works; model brewery; museum for mineral and botanical collections; collection of agricultural implements; library containing 6,000 volumes; four farms. 5,000 hectares of forest land, and 4,000 hectares (= 2·47 acres to the hectare) of arable meadow land are attached to this institution, in which instruction is given by a staff of twenty-four professors. Proskau has 1,900 inhabitants, of whom 1,500 are Poles.

THE *Times of India* of August the 16th states that an agricultural society, to be called the "Bombay Presidency Agricultural Society," composed of influential gentlemen, has been organised in Bombay. The object of the society is to diffuse agricultural knowledge amongst the people in the Bombay presidency, by establishing a journal and issuing separate tracts on agriculture in Marathi and Guzerati, and, if possible, by founding schools for this special purpose. The journal and tracts will supply a want that has been felt for some time past, and the new society is one that ought to succeed, and, if properly managed, will be sure to be of immense service in the Bombay Presidency.

We learn from the *Garden* that the directors of the Alexandra Park Company have requested Mr. M'Kenzie to prepare a scheme for establishing a school of horticulture, for which purpose about twenty acres of the grounds attached to the building will be set apart. As we have no school of horticulture in this great gardening country, we hope something more may come of this than of its short-lived and feeble forerunners.

The Portuguese *Jornal de Horticultura Pratica* announces a forthcoming "Flora" of that country, by Señor Barao de Castello de Piava, who was formerly Professor of Botany in the Academia Polythenica. Great things are expected from the new work, in which the subject will be brought up to the level of the knowledge of the present day, including all the discoveries which have been made since the time of Brotero, whose once celebrated "Flora Lusitanica" is now seldom to be met with for sale.

A new *Révue des Sciences Naturelles* has recently been started at Montpellier under the management of MM. Dubreuil and Kecker, to be published every three months.

We learn that the publication of the *American Journal of Conchology* has closed with the completion of its seventh volume. This quarterly, edited by Mr. George W. Tryon, has appeared under the auspices of the Academy of Natural Sciences in Philadelphia, and has included, from time to time, a great many very important conchological monographs, chiefly presented to the Philadelphia Academy, many of them accompanied by coloured plates. Hereafter, such communications will be published in the Journal of the Proceedings of the Academy itself.

The Second part of the *Quarterly German Magazine*, just received, contains translations (still into very indifferent English) of only two articles · Dr. J Rosenthal on Electric Phenomena, and Prof. de Bary on Mildew and Fermentation.

Les Mondes has a long description, with illustrations, of the new "Horloges electriques" of M. C. F. Milde, the principal of which is a commutator for distributing the hours in all directions. It consists of an electro-magnet proportioned to the requirements, whose armature, at the moment of attraction, acts upon the arm of a lever, which governs a sector, whose centre of rotation is upon a pillar.

An apparatus has been recently devised in Germany for obtaining specimens of water at any desired depth of the ocean. A strong, heavy vessel, entirely closed and empty, has a valve through which water may be admitted, but which is only put in motion by means of powerful electro-magnets connected therewith. These magnets are also connected with a wire which accompanies the rope, by means of which the apparatus is lowered from the ship. When the empty vessel, which is in fact a plummet, has reached the required depth, an electric current is sent from the battery on shipboard to the coils below; the magnetism thus generated opens the valves, and the vessel is filled and ready to be drawn up.

According to the correspondence of the *New York Herald*, an ingenious plan has been adopted by Prof. Agassiz's expedition for determining how far the submarine regions are pervious to light. A plate prepared for photographic purposes is inclosed in a case so contrived as to be covered by a revolving lid in the space of forty minutes. The apparatus is sunk to the required depth, and at the expiration of the period stated is drawn up and developed in the ordinary way. It is said that evidence has thus been obtained of the operation of the actinic rays at much greater depths than hitherto supposed possible.

THE BIRTH OF CHEMISTRY

II.

Thales of Miletus—Later beliefs in his doctrine—Anaximenes—Empedokles—Herakleitos—Anaxagoras—Demokritos—The Atomic Theory—Aristotle—The Ethereal Medium—Transmutation of the Elements—The Four-element Theory—Mode of interpreting it—Cause of the absence of Natural Science among the Ancients.

THE elements of the Greek philosophers were, as we shall presently show, rather *principles* than elements in the sense in which we speak of the sixty-five elements now known to chemistry. There was a marked tendency in the earliest period of Greek philosophy to make one element or principle fundamental, and to evolve the other elements and the world from it. Thales, of Miletus, who lived in the sixth century, B.C., and who was called "the first of natural philosophers" by Tertullian, and the "first who inquired after natural causes" by Lactantius, affirmed that water was the first principle of things, perhaps, according to some writers, because Homer had made Okeanos the source of the gods. At least we are reminded of the boundless watery chaos of older cosmogonies. This doctrine of Thales was not without its supporters during the Middle Ages, and, indeed, the convertibility of water into earth and air was not absolutely disproved until about a century ago. One of the ablest supporters of the dogma was Van Helmont (b. 1577, d. 1644), who affirmed that all metals, and even rocks, may be resolved into water; animal substances are produced from it, because fish live upon it; and vegetable substances may be also produced from it. This last assertion he endeavoured to prove by what would appear to be a very conclusive experiment in those days, when neither the composition of the air nor of water was known. He took a willow which weighed five pounds, and planted it in two hundred pounds of earth, which he had previously carefully dried in an oven. The willow was frequently watered, and at the end of five years he pulled it up and found that its weight amounted to one hundred and sixty-nine pounds and three ounces. The earth was again dried, and was found to have lost only two ounces Thus it appeared that 164lb. of wood, bark, roots, leaves, &c., had been produced from water alone. Hence he inferred that all vegetables are produced from water alone; not knowing, as was afterwards proved by Priestley, that a constituent of the atmosphere called carbonic acid had furnished the solid part of the tree, although, indeed, there was much water with it. Boerhaave devotes a page of his big book to a discussion of "whether water be convertible into earth." He concludes that the small earthy deposit observed when rain-water is distilled, arises from the particles of dust which had settled on the water before its introduction into the distilling vessel. Mr. Boyle had previously affirmed that "a very ingenious person, who had tried various experiments on rain-water, put him beyond all doubt about this transmutation, for he solemnly affirmed, on experience, that rain-water, even after distillation in very clean glasses, near two hundred times, afforded him this white earth." Finally, Lavoisier, in 1770, communicated to the *Académie des Sciences* an elaborate paper, "On the nature of water, and the experiments by which it has been attempted to prove the possibility of changing it into earth." In this he conclusively proved that water cannot be changed into earth, although it be distilled backwards and forwards for many successive days. Here then we find the old Thalesian theory at last disproved, but not before it had endured for twenty-four centuries; and this is by no means a solitary example of the permanence of old ideas. We shall become acquainted with yet older theories, which are still admitted, and which seem to be essential to physical philosophy.

On the other hand, Anaximenes regarded air as the primal element, Herakleitos fire, Pherekides earth, and some philosophers grouped two elements together. Anaximenes held that clouds were caused by the condensation of air, rain by the condensation of clouds; he appears to have clearly connected condensation with cold, rarefaction with heat. Archelaus affirmed that air when rarefied becomes fire, when condensed, water. It was very generally believed during the Middle Ages that water when boiled was converted into air. Empedokles introduced the idea of four distinct elements—earth, air, fire, and water, not capable of passing one into the other, but forming all things by their intermixture. These elements are acted upon by two principles, a uniting force of amity, a separating force of discord, corresponding somewhat to our attraction and repulsion. He endeavoured to prove the four-element theory by the following crude experiment: wood is burnt upon a hearth, fire seems to be evolved from it, the smoke is air, moisture is deposited on the hearthstone, while the ashes are earth, hence wood is made up of earth, air, fire, and water. Empedokles was one of the first to materialise the Homeric gods; he applied his four-element theory even to them, declaring that Zeus was the element of fire, Here the element of air, Nestis the element of water, and Aidoneus the element of earth. Herakleitos (about 460 B.C.) made fire the primal element, and assumed that it condensed itself into the material elements, and that air, water, and earth were respectively formed as the fire became more condensed. He asserted, moreover, that all things are in perpetual motion and change, the moving force being fire; "fire is to him," says Schwegler, "even in individual things, the principle of movement, of physical as of spiritual vitality; the soul itself is a fiery vapour." We find in the fire of Herakleitos to some extent the attributes of what we now call a physical force; thus it is precedent to matter, and is the motive power of the universe, it influences and changes matter, it is perpetually undergoing transformation, but ultimately returns to its own form. Prof. Max Muller speaks of Herakleitos as "one of the boldest thinkers of ancient Greece." We can well understand why fire should, at a very early date, be regarded as chief of the elements, and the motive power of the universe; it had long been worshipped as a symbol of the deity by the Chaldæans; a worship which possibly originated with the Scyths; for Zoroaster, who introduced fire worship among the Medo-Persic races, is supposed to have been a Scythian. Again, Agni, the god of light and fire, was placed first in the Hindu Trinity.

Anaxagoras of Klazomene (B.C. 500) asserted that originally all things existed in infinite disorder; before the creation there was a chaos of mingled particles of matter, which were arranged in order by a designing intelligence or mover of matter (*νοῦς*). The primitive constituents of things are not definite elements, like those of Empedokles, but are *homœomeries* (ὁμοιομέρειαι) that is *like parts*, small particles of matter like the masses they produce when they aggregate. Thus a mass of iron is produced by the aggregation of an infinite number of iron-homœomeries brought out of the chaos by the *νοῦς*, which latter possesses vortical motion which enables it to separate like parts and bring them together, somewhat on the principle of gold-washing. If a dish containing substances of different relative weight, such as cork, sand, and lead shot, intimately mixed together, be caused to rotate, like particles will come together, the lead in one place the sand in another, and this experiment will help us to realise to some extent the meaning of Anaxagoras when he assumes that the vortical motion of the *νοῦς* caused homœomeries to aggregate and form the world. Leukippos taught that the world is produced by the falling together of small indivisible particles or *atoms* (from *α* and *τεμνω*), which are the principles of things, and which possess rapid circular motion. Demokritos (460 B.C.) extended the atomic theory of Leukippos; he contended that the principles of things are atoms and a vacuum. The atoms are invisible by reason of their smallness, indivisible by reason of their solidity, impenetrable and unalterable. They have no other qualities, neither heat, nor cold, nor colour. Atoms are infinite in number, the vacuum is infinite in magnitude. Atoms differ from each other in size, shape, and weight. They are actuated by necessity or fate (ἀνάχκη), and possess an oblique motion in the vacuum, which causes atoms of like shape to collide and group themselves together, by which means all things are formed. The vacuum is necessary, otherwise motion of the atoms would be impossible, because there would be no place to receive them. Long before the time of Demokritos an atomic theory had been proposed in India by Kanáda, the founder of the Nyaya system of philosophy, of which this theory forms the distinguishing feature. The theory of Leukippos is attributed by Possidonius to Moschus, a Phœnician. During the Middle Ages many writers made the atomic theory a prominent part of their system. Descartes adopted it in a somewhat modified form, and associated with his particles the vortical motion possessed by the homœomeries of Anaxagoras. Finally, almost in our own day, the atomic theory was introduced into chemistry by Dalton, and its introduction marked an important era in the science. At the present time the doctrine of atoms forms a principal feature in chemistry, and other branches of science find the conception most conducive to the philosophical explanation of phenomena. The definition of an atom given by Demokritos is almost as absolute and precise as that which we find in our most modern treatises. Thus the theory has endured for more than twenty-five centuries, and is likely t endure until there shall be no more science. It offers a strikin example of the oneness of physical thought; the conceptio seems to be essential to Natural Philosophy; the most stupendou phenomena may be referred to atomic motions. S. Augustin has well said, "Deus est magnus in magnis, maximus autem i minimis."

The Hindus not only possessed the idea of the atomic consti tution of matter, but further associated an attractive force wit the atoms. This is well shown in the following extract given b Sir William Jones, from the poem of "Shi'ri'n and Ferhád, or th Divine Spirit, and a human soul disinterestedly pious": —"Ther is a strong propensity, which dances through every atom, an attracts the minutest particle to some peculiar object; search th Universe from its base to its summit, from fire to air, from wate to earth, from all below the moon to all above the celesti spheres, and thou wilt not find a corpuscle destitute of th natural attractibility; the very point of the first thread in th apparently tangled skein; is no other than such a principle attraction, and all principles beside are void of a real basis; fro such a propensity arises every motion perceived in heavenly or i terrestrial bodies; it is a disposition to be attracted, which taug hard steel to rush from its place and rivet itself on the magnet it is the same disposition which impels the light straw to attac itself firmly to the amber; it is this quality which gives eve substance in nature a tendency towards another, and an inclin tion forcibly directed to a determinate point."

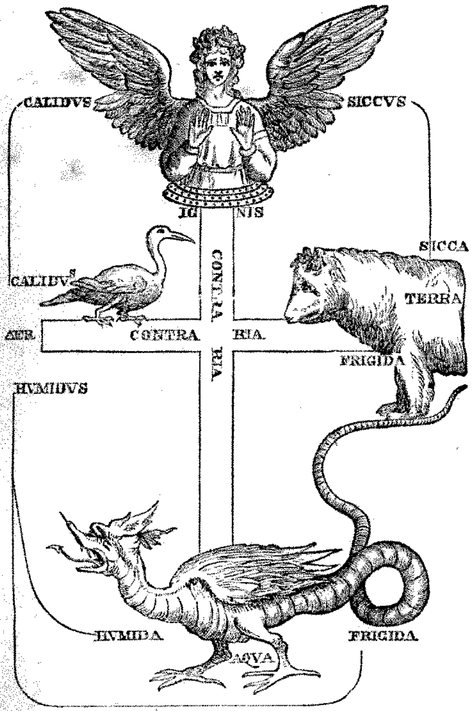
The most prolific writer on Science amongst the ancients wa Aristotle (b. 385 B.C., d. 322). He was the author of vario treatises, on the Heavens, on Generation and Corruption, o Physics, on Respiration, on Audibles, &c., and his views as we on metaphysics and ethics, as on science, were nearly universall accepted during the Middle Ages. Indeed, the scientific writing of Aristotle influenced science for nearly twenty centuries. Few however, of his opinions concern us here. He was the first t introduce into Greek philosophy the *ether*, which he regarded a a fifth element (hence afterwards called *quinta essentia*) more subtl and divine than the other elements. The word quintessence frequently used by the alchemists and early chemists, and is foun in our most recent English dictionaries. The idea of an infinitel rarified and all-penetrating matter had long existed in physic philosophy, notably in the Hindu systems; it was probably recog nised as a fifth element prior to the ninth century B.C. Aristotl is said to have called it αἰθὴρ from ἀεὶ and θέω, because he co ceived it to be always in motion, and to be the moving agency the other elements; but we cannot admit this derivation now, an prefer to trace it to *αἴθω* and *indh*. In the present day we find impossible to explain various phenomena, notably those connecte with radiant heat and the polarisation of light, without assumi the existence of some rare ethereal medium, cubic miles of whic would not weigh a milligramme, and we still call it the ether. Fe physical systems have avoided this supposition; we make less u of it in chemistry than in physics; but it would be difficult account for such actions as the combination of chlorine and h drogen under the influence of light, without it. Aristotle hel that the four elements are mutually convertible, and he assigne two qualities to each, one of which was common to some oth element. Thus he said :—

Fire is hot and dry.
Air is hot and moist.
Water is cold and moist.
Earth is cold and dry.

In each of these one quality is dominant. Thus fire is mo hot than dry, air more moist than hot, water more cold tha moist, and earth more dry than cold. If the dry of fire vanquished by the moist of water, air will result; if the hot air be vanquished by the cold of earth, water will result; if th moist of water be vanquished by the dry of fire, earth will resul This idea of the transmutation of the elements was adopt generally in works on alchemy; the following figure which en bodies it is from a work entitled "Preciosa Margarita Novella, published in Venice in 1546.

Aristotle's method of expressing the transmutation of the el ments does not seem to differ much from that of earlier phil sophers; it would appear that he means to imply that if wat be heated air is produced, while if it be heated more strongly as to evaporate it to dryness, earth is left. His account of th generation of fire from air and earth is based on the most shallo and meagre observation, and shows to what results the mo astute mind may be led if unaided by experiment. The generation of fire, he says, is made evident by the senses, for flame is notably fire, but flame is burning smoke, and smoke is from air and earth.

It is not here that we may tell how the philosophy of Aristotle was introduced into Europe by the Arabians, how from it arose that stupendous mass of false philosophy and perverted Aristotelianism called Scholasticism, and how for centuries the blind acceptance of the Peripatetic dogmas retarded the progress of science. Worse than all, Averroës, who has been called "l'âme d'Aristote," and who scattered Aristotelianism broadcast over Europe, did not know Greek, and the Latin versions of Averroës were "Latin translations from an Hebrew version of an Arabic commentary on an Arabic translation of a Syriac version of a Greek text." We may not, therefore, blame Aristotle for the results which followed from the too general and literal acceptance of his philosophy. Mr. Lewes has well said, "However he may have been impelled to systematise on imperfect bases, and to reason where he should have observed, it is not too much to say that had he reappeared among later generations, he would have been the first to repudiate the servility of his followers, the first to point out the inanity of Scholasticism. His mighty and eminently inquiring intellect would have been the first to welcome and to extend the new discoveries. He would have sided with Galileo and Bacon against the Aristotelians."



Alchemical Representation of the Transmutation of the Elements.

We have spoken above of the endurance of the Thalesian theory, that all things are formed from water, and of the yet older theories of the existence of an ethereal medium, and of atoms; but the theory which affirms that the world is composed of the four elements—earth, air, fire, and water, is yet older, and is, indeed, the oldest physical theory of which we have any knowledge. It certainly existed before the fifteenth century B.C., it was adopted in India, Egypt, and as we have seen, in Greece at a very early date. Then in the case of those philosophers who made water, air, fire, &c., primal elements, this element was first transmuted into the three other elements, and the world was formed from the four. We must be careful, however, to remember that these four elements are not to be understood too literally, they were rather principles or types of qualities than actual elements. Several philosophers divided fire into a purer and grosser part. Seneca tells us that the Egyptians extended the theory by assigning to each element an active and a passive form: thus fire was divided into light which shines, and into fire; air into passive atmosphere and active wind; water into fresh and salt water; and earth into cultivable land on the one hand, and rocks on the other. These elements were extended yet more. In later times *Fire* would come to signify everything appertaining to ignition; thus light, whether accompanied by heat or otherwise, flame, the heat inherent in all bodies, incandescent bodies, stars, fiery meteors, lightning, and all visible manifestations of electricity, would be included under the term. *Air* would include smoke, steam, all vapours, and whatsoever approached to the nature of a gas. When gases were first discovered a hundred years ago, they were called *Airs*; thus we read of *fixed air*, *nitrous air*, *dephlogisticated air*, &c. *Water* would include all liquids, of which, no doubt, blood, milk, wine, and oil, were in early times the most familiar; the words *aqua fortis*, *aqua regia*, *aguardiente*, *eau-de-vie*, &c., are vestiges of the old practice. *Earth* included all rocks, however dissimilar they might be, all kinds of cultivable land, metals, and whatever appertained to solidity. Every solid was regarded as a kind of earth at first. A century ago many substances were called earths. At the present time out of the sixty-five elements known to the chemist, eight are classed as "earths" and three as "alkaline earths." The fact is, the four ancient elements were types of great classes of which the whole world was constituted. In their most general sense, *earth*, *water*, *air*, signified *solidity*, *liquidity*, *gaseity*, while *fire* was the force exercising itself upon matter. We have seen that the elemental fire of Herakleitos is the mover of matter, the principle of movement, that which produces perpetual changes around us. Fire was the ψυχή, the anima, the soul, the vivifying spirit. The mythological side of the belief is seen in the story of Prometheus, who is fabled to have stolen fire from Heaven and therewith vivified mankind. The philosophical side of the belief is seen in the dogmas of Herakleitos. The four-element theory evolved itself from the crude ideas about ether and chaos, mind and matter, before discussed; it is one of those crude physical theories which is enunciated and accepted by races the most diverse in character, country, faith, destiny. There is great oneness in the human mind in the matter of broad principles in crude cosmical ideas. And let us not forget that the four-element theory was universally accepted during the Middle Ages, and was only disproved a century ago, when air was proved to be a mixture of two gases, water a combination of two gases, fire the result of intense chemical action, and earth a mixture of some dozens of elementary bodies, some combined, some single. We do not deny that during the continuance of the four-element theory it may often have been taken in its strictly literal sense; but we do venture to assert that the richer and more cultured intellects regarded it in the light we have above described.

We can quite understand why there was so little natural science among the ancients, when we remember the absence of all experimental method and means, and the obstacle presented by the habit of mind which induced them to apply reasoning in place of experiment in the study of nature, to reason upon an immature or ill-observed fact, and to generalise upon altogether insufficient data. A simple sophistry applied to observation could lead to the most monstrous results. Take, for example, the argument of Diodorus, as given by Sextus Empiricus to prove that nothing is moved:—"If a thing be moved, it is either moved in the place where it is, or in the place where it is not. But not in that wherein it is, because it rests in the place wherein it is; neither in that wherein it is not, for where a thing is not, there it can neither act nor suffer. Therefore nothing is moved." Again, Sokrates and many of his followers taught that it was unwise to leave those affairs which directly concern man, to study those which are beyond his control and external to him. Thus, to inquire into the nature and distance of the stars seems an useless speculation, because even if we could ascertain these things, we could neither alter the course of the stars nor apply them to any benefit of mankind.

We have, however, seen above that many of the Greek philosophers had more or less definite notions concerning matter and force, and that they frequently insist upon the transmutation of matter from one form into another; so far and so far only are we concerned with their dogmas in our inquiry into the Birth of Chemistry. But we must not fail to notice the existence at a very early date of the four-element theory, of an atomic theory, of the idea of an ethereal medium, of the idea of transforming one kind of matter into another by the agency of some motive principle. Neither let us forget to note the similarity of principles in diverse philosophies; thus the homœomeries of Anaxagoras and the atoms of Leukippos are clearly related, so, too, are the *νοῦς* of Anaxagoras, the *ἀνάγκη* of Demokritos, the actuating form of fire of Herakleitos, the moving ether of Aristotle. The links which bind together ancient and modern physical thought are strong and enduring, and since they have lasted during the rise and fall of many nations, and during the most profound changes in the mode and tone of thought, it is not unlikely that they will endure as long as the chain itself.

G. F. RODWELL

THE DIATHERMACY OF FLAME

I HAVE just read Mr. Ericsson's paper on "The Sun's Radiant Heat" in NATURE, October 3, p. 458, and find that he has made some experiments on the diathermacy of flame closely resembling those which I made in 1869, and described in chap. viii. of "The Fuel of the Sun" published January 1870. Although the object of our investigations was identical and the method of proceeding very similar, the results obtained are so contradictory that one of us must be quite wrong, and therefore I think the subject demands discussion.

Referring the reader to the engraving illustrating Mr. Ericsson's paper, I may easily describe my apparatus. Like Mr. Ericsson's, there was a gas-pipe from the side of which projected a row of burners, each provided with a separate stop-cock. My burners, however, differed from his in being perpendicular to the main pipe which was always used in a horizontal position. My blackened bulb thermometer was similarly fixed at one end of a chamber or vessel, the other end of which was open to receive the radiations from the flames. This, however, was much simpler than Mr. Ericsson's. It had not the double chamber with intervening wall, nor was it surrounded by water, but was simply a thin tube of tin plate *polished inside* to prevent absorption of radiant heat. The thermometer was insulated from metallic contact with this tube, and thus could only receive heat from it by radiation, which the polishing reduced to a minimum. The sectional form and opening of the tube was made to correspond nearly with that of the presented side of the gas flames, but was somewhat larger.

At first I used Bunsen-burner flames, then flat flames like those figured by Mr. Ericsson, afterwards simple jets formed by the gas issuing from a small pin-hole, the jets being far enough apart to be quite independent; finally a row of such jets so near to each other that they came in contact, coalesced fully, and formed one sheet of flame, the edge of which was presented to the mouth of the polished tube containing the thermometer.

Guided by results obtained in a previous series of photometric experiments on the transparency of flames to their own special radiations, and by the first experiments I made on diathermacy, I relied on the arrangement last described, viz. the coalescing jets. The reason for this will presently appear.

My mode of proceeding differed in another respect from Mr. Ericsson's. Instead of lighting one jet at one end and then another and another in succession towards the thermometer, I always worked with an odd number of flames, and began with the middle jet, then lighted one on each side, next one on each side of those three, then one on each side of those five, and so on. My flames were thus maintained at a constant mean distance from the thermometer.

By means of a well-constructed experimental gas meter, with micrometic regulator, and a minute alarum clock, the supply of gas was accurately adjusted, so that each additional jet, or pair of jets, should consume an exactly equal differential increase of gas. The results obtained were as follows:—

Number of Jets	Consumption of gas in cubic feet per hour	Highest reading of Thermometer
1	1·0	19°·0 Cent.
3	1·5	23°·0 ,,
5	2·0	27°·0 ,,
7	2·5	31°·0 ,,
9	3·0	35°·5 ,,
11	3·5	39°·0 ,,
13	4·0	43°·5 ,,
15	4·5	48°·0 ,,
17	5·0	53°·0 ,,

Here, then, is a serious discrepancy. I get an increase of 4° by the first addition of two flames, and by eight of such additional pairs obtain an increase of 34°, instead of the 32° due to theoretical diathermacy. These 2° of excess (being due to the latter end of the series) I attributed to the increased temperature of my apparatus.

Mr. Ericsson obtained an increase of only 7°·9 instead of 17°·6, the theoretical requirement.

Without any disposition to underrate the value and importance of Mr. Ericsson's researches, I think that in this matter he has been deceived by overlooking some important sources of fallacy.

1. He tells us nothing about the quantity of gas consumed. His jets all issue from the same main pipe, which he describes as supplied with "gas at ordinary pressure." Now with such a supply the quantity of gas burning from each jet would steadily diminish as he turned on the additional jets. On turning the second jet the first would diminish; when the third was turned the supply to both first and second would be reduced; and so on, to an extent depending upon rates of sectional area of the supply pipe to that of the jet holes. If Mr. Ericsson's drawing is made to scale, the error due to this was of great magnitude.

A second source of error is described in Mr. Ericsson's own words; he says, "It will be observed that the prolongation of the axis of the conical vessel upwards passes through the central portion of the flames at the point of maximum thickness and intensity." Now the point of maximum thickness of a flame is just that part which is hollow, and consists of a central core of unburnt gas with an outer coating of true flame, and the central portion of such a flat flame as Mr. Ericsson represents includes much of the blue portion of the flame, consisting of hydrocarbon not yet in full combustion. Mr. Ericsson, therefore, was not experimenting upon the diathermacy of ten flames, but upon the diathermacy of ten discs consisting of a mixture of flame proper and unburnt hydrocarbon. Now Tyndall has demonstrated the remarkably high resisting or absorbing power of such hydrocarbon in reference to the radiations from a flame produced by hydrocarbon combustion. The flame itself might therefore be perfectly diathermous, and yet, when examined in this manner, exhibit a considerable degree of athermacy.

There is still a third source of error in Mr. Ericsson's mode of proceeding, the magnitude of which I am not yet able to estimate, though some experiments made since publishing my first results lead me to suspect that it is sufficiently important to demand very careful elimination. I allude to the arrangement of a series of separated flat flames, with the broad surfaces presented to the thermometer.

What must we have between each of these separated flat flames? Obviously each flame is coated with a film of vapour, the product of the combustion of those portions of flame lying below it; these vapours, though rapidly rising, must form a layer of sensible thickness equal to an important fraction of the whole thickness of such thin flames. When operating with the whole eleven flames, there were twenty-one such films between the first flame and the thermometer. Now, we know from the experiments of Tyndall, that a large proportion of the rays of heat emitted from a hydrocarbon flame will be absorbed by such intervening strata of aqueous vapour, carbonic acid, and carbonic oxide. It is true that the middle or blue part of the flame, having less combustion going on below it, must have a thinner coating of such vapours than the upper part; and thus in Mr. Ericsson's arrangement this third source of error is diminished in the same proportion as the second is increased. It was these theoretical considerations, confirmed by results of preliminary experiments, that induced me to abandon the flat flames in favour of the simple round jets, and finally to adopt the continuous flame formed by the coalescent jets.

As I stated on the first publication of the results of these experiments on the diathermacy of flame, I do not regard them as sufficiently delicate to be finally and quantitatively conclusive; the means at my disposal rendered them less satisfactory than those I made on the transparency of flame. Still, I think they are not open to any such serious sources of error as those I have here pointed out.

I hope that Mr. Ericsson will not be offended by the candour of my criticism, nor by the egotism which is inevitable in an unaffected defence of one's own philosophical bairns.

My experiments, like those of Mr. Ericsson, were made with the direct object of throwing some light upon the great mystery of solar radiation; and the fact that we have arrived at such opposing conclusions will, I hope, lead to further investigation, and finally to a settlement of the important fundamental physical question, whether the properties of flame, in reference to the absorption and transmission of heat and light, are, as I have ventured to suggest, diametrically opposite to those of gases and vapours—whether flames are specially transparent and diathermous to rays of their own emission, and resist the passage of heterogeneous rays; that a flame is thus not merely heated gas, but another and distinct form of matter, or rather is matter in a different state of activity.

If this be established, we shall be driven back upon "the wisdom of the ancients," and be forced to admit the classification of the four elements, "fire, air, earth, and water," or flame, gas, solid, and liquid; remembering, of course, that they used the term "element" with a different meaning to that of our modern acceptation. They described elementary or necessary conditions, not elementary constituents. It was the philosophy of material existence, not the composition of material substances, which chiefly occupied their attention. From this point of view their classification may, after all, prove to be correct.

I must reserve for another communication some remarks I proposed to make on the application of the above to Mr. Ericsson's researches on the radiation of the chromosphere.

W. Mattieu Williams

SCIENTIFIC SERIALS

The part of the *Transactions of the Linnean Society* just published, forming the 2nd part of vol. xxviii, consists of two elaborate botanical papers; "Memoir on the Spermogones and Pycnides of Crustaceous Lichens" by Dr. Lauder Lindsay; and "On the Hippocrateaceæ of South America" by Mr. Miers. Of the important features of the latter paper we gave a sketch on the occasion of its being read before the Society. It is illustrated by seventeen very beautiful plates executed by the author The first is an extremely elaborate paper, illustrating the great variation in the spermogones and pycnides in the same species of lichen, and even in the same individual. For this purpose as many as twelve or even 20 specimens of the same species, preserved in various herbaria, are in some cases minutely described. This paper is also illustrated by eight coloured plates.

The *American Naturalist* for August does not contain so many original articles as usual. The Rev. Samuel Lockwood describes a new Entozoon from the eel, belonging to Duvaine's type, the Acanthocephala or spiny-heads, but forming a new genus; the name proposed is *Koleops anguilla*. Dr. J. J. Woodward has a paper on the use of monochromatic sunlight, as an aid to high-power definition; and the Rev. H. J. Bruce describes some of the familiar birds of India. Among the shorter articles there are some very interesting notes.

The number for September opens with an interesting article by Mr. S. H. Scudder, the curious history of a butterfly. The American butterfly *Brenthis bellona* occurs in two different forms produced at different times of the year, in both cases the larva hibernates, but with one set when just out of the egg, with the other when half grown, the butterfly appearing in one case in May, in the other in September; and it seems impossible that these two parallel races of the same species can ever mingle. Prof. N. S. Shaler has a paper on the Geology of the Island of Aquidneck and the neighbouring parts of the shores of Naragenset Bay; and Dr. R. H. Ward sends a microscopical contribution entitled "The new Immersion Illuminator." Mr. C. V. Riley, who has paid great attention to the *Phylloxera* and other diseases of the vine, has some valuable remarks on the cause of the deterioration of some of the native grape-vines, which he has contributed to his report as Entomologist to the State of Missouri.

The *Quarterly Journal of Science* for October commences with two meteorological articles, the Origin of the Great Cyclones, by Prof. T. B. Maury, and an anonymous paper on Weather Prophecies. The author of the former article considers it proved that cyclones are formed chiefly, if not exclusively, along the edges of the great atmospheric currents, the surface currents and the upper currents alike, the polar streams which descend into our valleys, and the aerial gulf streams which move invisibly over our heads. Capt. Oliver continues his series of papers on the Amorpholithic Monuments of Brittany, and in continuation of a previous series we have an article on Natural and Artificial Flight—an Aerial Ship. Mr. F. C. Danvers, on Paper in the International Exhibition, gives a slight sketch of the history of the manufacture of paper and of the various specimens to be seen in the Exhibition. The Physiological Position of Tobacco, by Mr. E. A. Axon is a powerful attack on the use of the weed as not only unnecessary and destitute of any beneficial results, but positively injurious.

The first paper in the *American Journal of Science and Arts* for September is by Prof. J. W. Draper, "Researches in Actino-Chemistry," from which we have already reprinted an extract on the distribution of heat in the spectrum. Prof. Shepard concludes his account of the Corundum region of North Carolina and Georgia; and then follows a sketch of Barrande's account of the origin of Palæozoic species. Mr. A. A. Hayes has a long article on the red oxide of zinc of New Jersey. In Prof. O. C. Marsh's continuation of his preliminary description of new Tertiary Mammals are descriptions of a large number of new genera and species.

In the *Geological Magazine* for October (No. 100), the Editor, Mr. Henry Woodward, gives us notes, illustrated with excellent figures, on some British Palæozoic Crustacea belonging to his order Merostomata. These notes include a full description of *Hemiaspis limuloides*, a species originally established by Mr. Woodward in 1865, and also shorter characters of three other species of the same genus, namely, *H. speratus* (Salt ms.), *H. horridus*, sp. n, and *H. Salweyi* (Salt). These Silurian forms are particularly interesting as they constitute a connecting link between the suborders Eurypterida and Xiphosura.—Of the latter group Mr. Woodward here notices some species of the genus *Bellinurus*, and describes a new form under the name of *B. Koenigianus*, also a new *Prestwichia*, *P. Birtwelli*, both from the Coal measures.—Mr. W. T. Aveline publishes a short note on the continuity and breaks between the various divisions of the Silurian strata in the Lake district, and Messrs. Davidson and King some remarks on the genera *Trimerella*, *Dinobolus*, and *Monomerella*. In this paper the authors propose the establishment of a new Brachiopod family, Trimerellidæ, allied to the Lingulidæ.—Dr. H. A. Nicholson describes a new genus of fossil tubicolar Annelides founded upon a division of the fossils hitherto referred by Palæontologists to *Tentaculites*. The so-called genus *Tentaculites*, according to Dr. Nicholson, includes forms belonging to the Pteropodous Mollusca and others which are true tubicolar Annelides, the former being free shells, the latter attached to other bodies. He proposes to retain the name *Tentaculites* for the Pteropods, and to establish a new genus, *Ortonia*, for the Annelides. He describes and figures a new species of the latter from the Cincinnati group of the Lower Silurian of Ohio under the name of *Ortonia conica*.—The concluding article in the number is a further instalment of Prof. Nordenskiold's account of the Swedish Greenland Expedition of 1870.

SOCIETIES AND ACADEMIES

Philadelphia

Academy of Natural Sciences, April 2.—Prof. Leidy made some remarks on specimens of fossils of extinct mammals from the Tertiary of Wyoming. One of these is an upper jaw fragment with two molars; the other a lower jaw fragment with a single molar. The upper molars have crowns composed of four lobes, of which the outer are like the corresponding ones in *Anchitherium*. Of the inner lobes, the front one is much the larger, and is prolonged outwardly in advance of the antero-external lobe. It is homologous with the antero-internal and antero-median lobes as existing in *Anchitherium* in a completely connate condition. The postero-internal lobe is the smallest of the crown. It is conical and conjoins that in front. A barely perceptible trace of a postero-median lobe is seen. A strong basal ridge incloses the crown, except externally, where it is feebly produced. The three upper molars occupied a space of 8 lines. The first molar is $2\frac{1}{2}$ lines fore and aft and $3\frac{1}{4}$ transversely; the second is $2\frac{3}{4}$ lines fore and aft, and the last one $2\frac{1}{4}$ lines. A question arises as to whether these teeth pertain to any of the animals previously indicated from lower jaw specimens with teeth. They are too large for the known species of *Hyopsodus* or *Microsyops*. They nearly accord in size with the lower molars of *Notharctus*, and perhaps belong to this genus. *Limnotherium* appears not to differ from this, as the number of teeth and their constitution are the same. The lower jaw fragment accompanying the upper one may belong to the same animal. The molar it contains, though resembling those of *Notharctus*, differs in several points. I propose to refer the fossils to a species with the name of *Hipposyus formosus*. Prof. Leidy further remarked that he had recently the opportunity of examining the tooth described by Prof. Marsh under the name of *Palæosyops minor*. The tooth evidently belongs to the curious pachyderm with the beaver-like incisors named *Trogosus castoridens* On observing the molar tooth, which is not worn away like those in the jaw specimen upon which the latter was named, it at once called to mind, the tooth which had been described under the name of *Anchippodus riparius*. On comparison, it would appear as if the specimens referred to *Palæosyops minor* and *Trogosus castoridens*, really belong to the same genus and species. The tooth of *Anchippodus ripparius* was obtained from a tertiary formation, Miocene or Eocene, in Monmouth Co., N.J. If the determination is correct, it would go to show that the Bridger Tertiary formation of Wyoming was contemporaneous with the Tertiary deposit of Monmouth Co., N.J. Prof. Cope stated that the largest mammal of the Eocene formations adjoining those of Wyoming, *i.e.* of the Wahsatch group of Hayden, was the *Bathmodon radians*, Cope, of about the size of Rhinoceros. It was an odd-toed ungulate, with peculiar dental characters. The incisors were well developed above and below, as in the tapir, but the dental series was little interrupted. The crowns of the upper molars were all wider than long, and presented mixed characters. On the outer margin one only of the usual crescents of ruminants was present, but a tubercle represented the anterior one. The one which was present was directed very obliquely inwards. Inner crescents were represented by two angles, the posterior forming the inner angular margin of a flat table, the anterior a mere cingulum at its interior base. The arrangement of these parts was stated to be of interest in connection with the relation ships between the types of hoofed animals. The single outer crescent was a ruminant indication, while the inner table resembled the interior part of the crown of *Titanotherium*. It differed, however, in its early union with the outer margin, its edge being thus possibly homologous with the posterior transverse crest in *Rhinoceros*. The premolars had two or three lobes with crescentic section arranged transversely. He regarded the genus as allied to *Chalicotherium*. He stated that the mammalian fauna of Wyoming and Utah more nearly resembled that of the Paris Basin than any yet discovered in our country, and that it had been discovered to contain a still greater number of generalised mammalian forms. One of the most marked of these was the genus just described by Dr. Leidy.

Paris

Academy of Sciences, September 30.—M. Chevreul, president.—The following members of the International Committee on the Metric System were present at the meeting :—MM. Stankart and Bosscha, for the Low Countries ; Mr. Chisholm, for England ; General Ibanez, Spain ; MM. Lang and Herr, Austria. The following papers were read :—"On the demonstration of the formula which represents the elementary action of two currents," by M. J. Bertrand, a long mathematical paper on Ampère's law of electrodynamical attractions ; "On the immediate determination by the principle of correspondence of the number of points of intersection of two curves of any order which meet at a finite distance," by M. Chasles.—Next came a note on the stability of colours on stuffs in general, and on silk in particular, by M. Chevreul. The author refers to a paper he read before the Academy twelve years back, when he drew attention to the unstability of many of the aniline colours then recently introduced. He now again calls attention to these colours, and considers that the use of them cannot fail to have a disastrous effect on French commerce and industry.—A paper by Father Secchi followed, entitled, "Solar Spectroscopic Researches." The author calls attention to the following extract from a letter to Herr Schellen, written by Mr. Young, of Dartmouth College, U.S. A. Mr. Young was stationed on Mount Sherman, 8,300 feet above sea level, and used a telescope of 9·4 inches aperture. He says, "The spectrum of the sun, although not entirely reversed at the border of the disc, became continuous, as Father Secchi has seen in Italy. When the air is calm the height of the region where this occurs is not greater than 1″. The lines rays of the chromosphere were remarkably augmented in number. H_1 and H_2 were seen reversed, as was *h* and the other hydrogen lines. In the spectrum of each spot the lines of hydrogen were reversed in a region slightly more extended than the penumbra ; this has been verified for at least twenty different spots." Father Secchi states that these observations confirm his own made at Rome in 1869.—M. Bertrand then presented the Academy with a posthumous work of M. Duhamel, entitled "An Essay on the application of scientific methods to the moral man," upon which he made some remarks. He was followed by M Max Marie's concluding paper "On the theory of the residues of double integrals." Next came MM. A. Rabuteau and F. Papillon's "Researches on the Physiological Action and Antifermentable properties of Sodic Silicate." The authors have added various quantities of this body to different kinds of fermentible matter and find a quantity of two grammes to completely stop all fermentation of whatever kind. Its action is exactly analogous to that of borax but more energetic. Two grammes of the latter injected into the veins of a dog produced no effect whilst one gramme of the silicate produced violent purging and vomiting and ultimately death after an interval of nine days —On the effect of vegetable parasites in altering bread by M. M. F. Rochard and Ch. Legros was referred to the commission appointed to examine the *Oidium aurantiacum*. M. Bertrand then presented a note "On the movement of the Planets around the Sun according to the Electrodynamic Law of Weber," by M. F. Tisserand. M. Yvon Villarceau presented a note by M. Stephan on the "Elements and Ephemerides of Planet 122. M. Yvon Villarceau remarked that M. Stephan had also calculated the orbit of 121, and he then presented a note by M. R. Luther, on an "Observation of the Planet 95, Arethusa, made at the Observatory of Bilk-Dusseldorff," which was followed by a note of M. Trève, "On Magnetism."—M. Milne-Edwards then presented a note by M. N. Joly, entitled, "Observations on the Metamorphoses of Osseous Fish in general, and particularly on those of a small Chinese fish of the genus Macropoda, recently introduced into France."—This was followed by a paper by M. H. Sicard, "On the Connection which exists between the Nervous and Muscular Systems in the Helices."—And then came a note by M. Lichtenstein, "On a Process for the Destruction of *Phylloxera*," by the burying and subsequent destruction of the young shoots.' Papers on the same subjects were received from MM. A. Rainaud, Peyrat, and Louvet, and were sent to the Phylloxera Commission.

PAMPHLETS RECEIVED.

English.—The Philosophy of Theism : J. Croll —Quarterly German Magazine, No 2.—Proceedings of the Bath Natural History Society and Antiquarian Field Club, Vol. ii. No. 3 —Proceedings of the Liverpool Naturalists' Field Club, 1871-72.—The Geology of the country around Liverpool : G H Morton.—Notes for my Students in Magnetism . W. J. Wilson.—Annual Report of Committee for amending the law with respect to the property of married women.—Journal of Mental Science, October.—Quarterly Journal of Science, No 34 —Heywood's School Atlas of Twelve Maps.—Pyrology, or Fire Analysis . Captain W A. Ross.—Journal of the Statistical Society, September.

American and Colonial —Canadian Naturalist, Vol vi. No. 4.—Popular Science Monthly, October —Preliminary Description of New Tertiary Mammals O C. Marsh —Notice of some new Tertiary and Post-tertiary Birds : O. C. Marsh —Proceedings of the Academy of Natural Sciences, Philadelphia, January—April 1872. —Washington Observations for 1870, Appendix II. Report on the Observations of Encke's Comet during its return in 1871 Hall and Harkness —The Curious History of a Butterfly : S. H Scudder —Proceedings of the Asiatic Society of Bengal, August

Foreign.—Verhandlungen der k k. geologischen Reichsanstalt zu Wien, August 30 —Sulla incinerazione dei Cadaveri. G. Polli —Zeitschrift fur Meteorologie, September.—Sur la mesure des sensations physiques : J. Plateau —La Belgique horticole, July—October.—Om Echinoideerms bygynad : S. Loven.

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THURSDAY, OCTOBER 24, 1872

AGASSIZ AT SAN FRANCISCO

THE completion of its labours by the United States *Hassler* Expedition presents many points of almost dramatic interest. We have the veteran naturalist, a native of the little republic of the old world—having transferred his home to the great republic across the Atlantic, and settled himself to his scientific work at the University in Massachusetts which derives its name from the old seat of learning on the banks of the Cam,—there gathering about him a band of earnest students, the master and his disciples together building up at Cambridge, in the course of a few years, one of the best appointed schools for practical instruction in Natural Science, and one of the finest Museums of Comparative Zoology in the world. The citizens of the Great Republic are constantly discovering within their own vast territories some extraordinary natural production which in old times would have ranked among the great wonders of the world: now a grove of gigantic trees in California; now the marvellous cañons of Colorado; now a wonderful assemblage of hot springs and geysers in Nebraska. But not content with the Government exploration of their own domain, the munificence of a private citizen of Massachusetts fitted out this *Hassler* coasting survey expedition with the necessary appliances, and placed the veteran Agassiz at its head, for the purpose of investigating the natural features of the extremity of the Southern Continent, and the inhabitants of its seas, the latter department being specially placed under the management of the accomplished naturalists Pourtales and Steindachner. Our readers already know how the experienced eye of Agassiz detected in Patagonia the same evidences of extensive glacial action with which he was already so familiar in the northern hemisphere; and the contents of the dredging nets will furnish employment to the staff of American naturalists for many a month to come. After cruising up the Pacific Coast of South America, the voyage of the *Hassler* finally ended in United States territory at San Francisco, where the expedition met with such a reception as has probably never before been accorded to any body of scientific amateurs. The *Alta California* thus welcomes the great naturalist on his return to his adopted country:—

"San Francisco has in its midst a man of science than whom none in America, or out of it, more richly deserves the love and homage and respect of our people. He has come all the way around our Southern Continent, not for gold, as many came, not for silver, as many came, not for diamonds, as many would come or go, but for scientific knowledge, for discoveries in the hitherto unsearched waters of the seas, the unexhausted treasuries of Nature growing, budding, and blossoming along the shores of a continent. When such a man, if there be any other such, chances to visit a distant city, he is quite likely to be made the guest of it, to be *fêted*, and to be made to feel that his merits of head and heart have endeared him to the people, and that the city feels itself honoured particularly, instead of honouring him. All the dukes and princes that ever stepped foot in America, never deserved a tenth part of the attention which is due to Prof. Agassiz. There is in America no man living who, as a scientist, compares with this gentleman in acquirements in his lines of study, and in the triumphs achieved. Many of our citizens have called upon him, and extended such courtesies as private parties may, and perhaps quite as extensively as is agreeable to him. But the question is, what does this city owe to itself in this matter? A public reception by the city would be a very graceful courtesy extended to a very great and most worthy gentleman, and the honour to herself would be one of which every citizen might well be proud. We hope it may be done."

The same paper and the *San Francisco Morning Bulletin* both print full reports of the professor's address on the occasion of his reception in the Pacific Hall by the California Academy of Sciences, heading their article, in genuine American style, by sensation headers of the following description:—"Agassiz. Grand Reception under the Auspices of the California Academy of Sciences. Pacific Hall in a Blaze of Intellectual Light. From Polyp to Mammal. Modified Darwinism—The Prophecy and Advice of a Man of Science."

From the address itself we may be permitted to make a few extracts, for the purpose of illustrating what are the subjects that are now uppermost in the minds of our scientific friends on the other side of the Atlantic. But, first of all, let us cordially congratulate the representatives of Science in that remote State, on the honour which has been reciprocally conferred upon them and upon Agassiz and his fellow workers by the enthusiastic welcome which they have given him, and on the encouragement which his visit has afforded for the further prosecution of their arduous labours. In the words of Prof. Davidson, the Principal of the California Academy of Sciences:—

"Less than one month since, the great master of modern scientific thought and research addressed a few earnest students of science, who have patiently and bravely kept their lamp trimmed and burning. The magic of his words thrilled our hearts and cheered our hopes; but, best of all, that gave renewed confidence to friends who had an abiding faith in our efforts. That you might have a richer feast, we have prevailed upon him to break a long and comparative silence, when he most needed rest and repose. In less than one hour after our last Academy meeting, the seeds sown by him had borne fruit in giving to the Academy a greater number of life members than we had gathered in ten years. Within a fortnight the Agassiz Professorship of Oriental Languages and Literature was established for the Universities of California by the munificent endowment of our fellow citizen, the Hon. Edward Tompkins; and now, after eighteen or nineteen long years of arduous and desperate struggle with poverty in this State of marvellous wealth and boundless property, the California Academy of Sciences is amply rewarded in being the instrument of introducing to the citizens of San Francisco Prof. Agassiz."

In the opening of his address, Prof. Agassiz made the following remarks on the present aims of science:—

"For the last three years I have been prevented from appearing in public, owing to the indifferent condition of my health. I venture this evening to address you. I have been asked to give some account of the voyage of the *Hassler*, which has terminated its cruise in the harbour of San Francisco. I am afraid that the incidents of that scientific expedition are too monotonous to be very entertaining; and willing as I am to accede to the request, I think I will submit to your attention remarks upon the present aims of Science, which may at least have a more solid foundation than our past efforts during that voyage, to increase the bases and material foundation of knowledge. Allow me to say that this examination, as I may call it, has been entirely incidental to the necessities of the Coast Survey. The good ship which brought us here is

> intended to continue the work of the Coast Survey along this coast; but instead of allowing us to make this voyage empty, some scientific gentlemen were invited by the Superintendent of the Coast Survey to take passage in her, and make the most of the opportunity. Liberal citizens of Massachusetts added means to the good will of the Superintendent—so that whatever collections and investigations should be made during the voyage should not be an additional expense to the great international undertaking, or to the Navy.
>
> "I think it desirable that these facts should be known, in order that not too great expectations should be entertained concerning the scientific results of the *Hassler's* voyage; for all that which could be done was done by means supplied by private individuals, and not by the large resources of the Government. Unfortunately, it is almost everywhere still so, that Science has to take the humblest place in the world, as if equal opportunities were yet granted with a reluctant hand. It is only in recent times that the value of research begins to be felt; and I hope to live, old as I am, long enough to see the community, the enlightened community, which has become my second fatherland, appreciate what Science is doing for the general prosperity, and then contribute to the necessities of Science with that generous liberality which characterises the acts of the American people. It is not generally understood—and perhaps we scientific men are at fault in this matter—that Science is at the foundation of all natural progress in the community, in industry, in the arts, in almost everything."

After a few details as to the objects of the study of natural history, he proceeded to explain that "the only difficulty in the way of the study lies in the fact that there are no teachers; that the community lacks teachers in this department; and wherever there are a few educated, they are at once swallowed by the numerous institutions of learning which are organising everywhere. And we cannot educate a sufficient number of them, for the simple reason that there are other walks in life which are more promising in the rewards they secure for their devotees. So Science is always behindhand, and yet it is she who furnishes the primary material for all the progress in modern times."

We need not follow the speaker through his singular misrepresentation of the theory of evolution as taught in this country, viz., that the various forms of life, as we now see them, "may be the work of blind forces, of forces without intelligence, without discriminating power, and without forethought," and that the object of the study of nature is "to determine whether we ourselves are descended from monkeys or whether we are the work of a beneficent Father." We will rather pass on to his peroration, which sets forth some truths at least as applicable to us as to the audience he was addressing:—

> "It will no longer do for the coming generation to say, I will accept this or that doctrine, because knowledge is pressing at your halls: but I will say to you, you may know it because you must know it, and unless you are willing to learn it you may grope in ignorance. That is the condition that stares us in the face for the future, and it becomes on that account the duty of every man to foster knowledge and to prepare the coming generation with all those appliances which lead to an independent opinion on all those matters. And that question is pressed upon you for the first time. You have for the first time in your existence as a State a generation which is about ready to enter the University. You have not had that in past generations, in past years, but your children approach the time when they may prepare for college. It is your duty that you have, then, a college which will set measures of the highest aspiration for the coming generation. And you cannot be willing to depend for their education upon the bounty of other States when in your prosperity, when you have rivalled all civilised communities. When I see luxury here, as in the oldest States of the world; when I see here the appliances for all the efforts of man carried to the highest degree, are you willing that your children should go and beg for information at the doors of other countries? It is for you to educate them and to give them those means which will make them find at home those advantages which otherwise you will have to seek for them by parting with them during those very years when their character is shaping. But there is another consideration than that of your immediate necessities here. No community can be utterly great without culture. Culture is the background of every great community. It is, in fact, the true and only test of real greatness."

Are not we in this country also forcing our children "to go and beg for information at the doors of other countries"? As long as we do not provide at home those educational advantages which so many go abroad to seek, we must submit to have to sit at the feet of our own daughter, and to learn from America both how to honour our really great men, and how to attain that scientific position among the nations to which our wealth and our material resources entitle us. The lesson is hard to learn, but it is one which must be learnt either by us or our children; and the longer we leave the task unlearnt, the harder will it be to learn.

RAILWAYS AND SCIENCE

Life of Richard Trevithick, with an Account of his Inventions. By Francis Trevithick, C.E. 2 vols. (London: E. and F. Spon, 1872.)

Life and Labours of Mr. Brassey, 1805-1870. By Arthur Helps. (London: Bell and Daldy, 1872.)

Railways or no Railways; or, The Battle of the Gauges Renewed. By Robert F. Fairlie. (London: Effingham Wilson, 1872.)

THESE three works are directly concerned with railways, though each of them deals with a different aspect of the many-sided subject. It would be out of place in these columns to review in detail each of the publications; they may, however, suggest a few thoughts not inappropriate to the columns of a scientific periodical.

The first on the list takes us back to the birth of the steam-engine, carrying us on through its chequered and roughly-handled youth to the time when it was fairly set agoing on that wonderful career, by means of which the whole face of the earth has been changed as if by the breath of a god, the relationships of nations and of men altered entirely, and civilisation hurried on at a rate almost bewildering. The benefits conferred on humanity by the discovery of the simple scientific principle on which the steam-engine rests, are incalculable, and not to be realised in anything like their fulness by those who, like most now living, have been born to these benefits, and who can only grumble that the advantages of the legacy left them are not by any means what they might be, were they not marred by the officiousness, the avarice, the prejudices, and the blundering stupidity of those who have constituted themselves its trustees. That this has been a cause of complaint all along is one of the many lessons to be learned from Trevithick's life; and so will it ever be, until the simple but stupendous and widely beneficent principle which the man of science, by reverent asking, has obtained from the liberal hand of Nature, is not only more thoroughly understood and more scientifically applied by those who undertake to put it to the uses of humanity; but until Science holds supreme sway over all the actions and thoughts of men universally, until she holds the same place as a guiding principle for the lives of men, and their relationships to each other and to the surrounding universe, that superstition and wild imagination have held for ages. It will only be when education is founded upon a thoroughly scientific groundwork, when men are trained from their youth upwards to regard human and extra-human phenomena with the clear, bold, intelligent vision of science undimmed by superstition, unwarped by prejudice, and unshortened by selfishness, that the full significance of scientific discoveries will ever be realised. Only then will they have a chance of making unobstructed way in conferring upon mankind the innumerable benefits with which many of them are fraught, and in raising the race higher and higher in the scale of civilisation, till that golden age be realised which poets dream of as in the unknown past, but which assuredly lies in the certain though, it may be, far distant future.

These remarks are suggested by the "Life of Trevithick," which, read in the light of the present day, makes one feel somewhat sad, and certainly sorry for the great and unselfish mechanical genius who scattered his inventions broadcast among his fellows, to the great enrichment of the latter, while he himself led a chequered and almost homeless life, dying, at last, penniless among strangers. Trevithick himself was not a scientific man, and seems to have had only a vague notion of the scientific principles upon which his numerous applications of the expansive power of steam were founded. But he was not like many so-called "practical" men, who work only by rule of thumb, and profess contempt for the scientific principles which they put to practical use. Trevithick appears to have had the greatest respect for science, and invariably submitted a new invention, or application of the one simple principle which governed all his inventions, to his scientific friend Mr. Giddy (afterwards Gilbert, and President of the Royal Society), in order that they might be submitted to the rigid test of scientific theory. Had a similar course been followed at a much earlier period, and had the earlier manipulators of steam been animated by the same spirit as well as the enthusiasm, and penetration, and disinterestedness of Trevithick, the steam-engine, instead of remaining the clumsy and unpliable machine it did for so many years, might about a century and a half ago have reached the perfection and wide applicability it has attained at the present day. And it was only in proportion as mechanicians clearly realised for themselves the full significance of the simple laws of steam, and fearlessly allowed them, under judicious control, to work with a powerful purpose upon properly constructed machinery, that anything like the wide-spread benefit was derived from them that they were calculated to confer; in other words, it has only been in proportion as engineers have grounded the rules of their art upon scientific principles that the steam-engine has attained to its present comparative perfection and innumerable labour-lessening and therefore blessed uses. In this it is that the great merit of Watt and Tevithick lies, both of whom had the penetrative genius to perceive that the mighty power which lies latent in a cup of water was almost entirely frittered away for want of proper guidance and a suitable channel wherein to work; and within a very few years after these men had made their important inventions, the development of the steam-engine had made infinitely more rapid progress than it had done during a previous century,

The history of the application of steam to machinery, the gradual development of the steam-engine, and especially its use for locomotive purposes, must be known to all our readers, and therefore we shall not attempt to repeat the oft-told story. It is one of those "fairy tales of science," which are more wonderful and often more bewitchingly beautiful than any of the thousand myths by which our "rude forefathers" blindly but naturally attempted to explain the many mysteries of the universe; and they have the additional merit of being true and therefore undying and never leading to disappointment and distrust. Nor shall we attempt to adjust the relative claims of Watt and Trevithick to priority of invention, or try to show their respective shares in the discovery from which the world is now reaping so much benefit. It is humiliating to think that Watt and Trevithick lived for a considerable time only a few doors from each other in a small Cornish town, each bent upon accomplishing a beneficent and highly useful purpose, and yet never spoke to each other, but on the contrary regarded each other with considerable bitterness all their lives; and this simply because the one advocated high pressure while the other was pushing the adoption of low pressure engines. Mr. Francis Trevithick, naturally enough no doubt, but with very bad taste and we believe much injustice, speaks of Watt frequently with great bittterness and depreciation as his father's rival, and jealous and ill-speaking opponent. To revive these squabbles serves no good purpose, but merely gives occasion to the world which lies in ignorance to sneer; the merit of Watt was very great, and so was that of Trevithick, and there is no need whatever to exalt the one at the expense of the other; each has a lofty and enduring pedestal of his own. We also think it displays considerable want of reflection and of the philosophic spirit to tirade, as Mr. F. Trevithick does, against the ingratitude of mankind towards those men who have conferred upon them great benefits in the shape of useful inventions, and against the deafness of men in place and power towards their claims for assistance and reward. The fact is that all great inventors, like all men of supreme penetration and foresight, are often too far in advance of their own generation to meet with much sympathy and appreciation from it. Mankind are not to be sweepingly blamed for this on any score. The race is yet a long way off perfection; and if the world ran so close on the heels of its great men as to be able at once to comprehend and appreciate them, these men would not be so great after all. The world, on the whole, acts very honestly, however hardly, to her man of genius, and when she does reach his standpoint, she erects a monument to him if he be dead, or if happily he be still alive, she rewards him with a pension. That Trevithick should have died penniless was pretty much, we think, his own blame, attributable to his own reckless imprudence, and his decided and blameworthy weakness of character in being unable to manage the affairs of his own household. It was no merit in him, and no sign of unselfishness, but simply a defect in his character, as it is in that of all men who act as he did.

That Trevithick is entitled to be called the "father of the locomotive" there seems to us to be no doubt, from the tediously full statements in the biography by his son; not only so, but he conceived and sketched, and even modelled, many of the improvements, perhaps in a crude form, that have been most recently introduced. In 1796 he made a model of a locomotive; in 1801 he ran one on a rough common road in Cornwall; and in 1803 he astonished the Londoners early one morning by driving the then uncouth creature through ten miles of the streets of the metropolis. Trevethick, however, reaped little benefit or credit from this wonder-working invention, for which the world at the time was not ripe. Of the numerous other applications of steam which Trevethick either thought of or embodied, we may mention the principle of the screw-propeller, the steam dredging-machine, the application of the steam-engine to an infinite variety of purposes in mining and tunnelling, the invention of something very like the borers used in the Mont Cenis tunnel, and the application of steam to nearly every important agricultural and manufacturing process; indeed he actually constructed several thrashing-machines, which many, no doubt, consider quite a recent invention.

It is by reading a biography like this, which takes us back to the middle of last century, and brings us down to nearly the middle of the present, that we are able in some small degree to estimate the benefit which science has conferred upon man in discovering the great but simple principle which underlies every application of steam as a moving power. Not only has it in almost every direction brought manufacturing processes to the highest degree of perfection, increased almost infinitely the power of production, but it has been the means of lessening, directly or indirectly, the severity and the amount of manual toil, thus making the mechanic's life sweeter and easier, and leaving him leisure for self-culture; and in general advancing to a distinctly appreciable amount the civilisation of the race. Especially, as we have already said, has the whole face of the earth been changed by the application of steam to locomotion; and even in this one direction it would be difficult to estimate the benefits conferred upon the race by science. The train and the steamer have done much to lessen and stamp out old national and district prejudices and animosities, by making the men of various nations more thoroughly and generally known to each other, have enabled populations to circulate more freely, and men to bring their talent or their craft to the market where it was most in demand, have made emigration possible to almost all who care for it, and thus peopled and civilised the waste and barbarous lands of the earth, brought the products of the most remote quarters within easy reach, brightened the existence of many thousands by putting it in their power to see some of the many beauties of this lovely earth—made the world, in short, more manageable, drawn its inhabitants closer together, increased decidedly the sum of human happiness, and helped to bring on the time "when man to man the warld o'er shall brithers be, and a' that."

As to the manner in which Mr. F. Trevithick has written his father's life, we are sorry we cannot speak favourably; either he does not know how to write biography, or he has been either so lazy or so short of time as to give to the public the crude and tedious material out of which a biography might have been constructed, instead of a well-digested and clearly-arranged narrative. The two volumes number upwards of 750 pages, and we believe not 50 of them are Mr. Trevithick's own. It is one of the most confused, most ill-put-together books we were ever compelled to read, and were it not for the intrinsic interest of the subject, it would certainly be the most tedious. The illustrations, type, and paper are excellent, and the book will no doubt be found useful by engineers

The next book on the list, Sir Arthur Helps's "Life of Mr. Brassey," is, it is needless to say, as a work of literary art, infinitely superior to the former, though the subject is one of not nearly so great intrinsic interest and importance. Had the work of writing the life of Mr. Brassey fallen into any other hands, it might have been a very dull and uninstructive affair indeed; but it is impossible for the author of "Friends in Council" and "Thoughts upon Government" to write uninterestingly or inartistically about anything. This life of the great railway contractor has all the quiet and soothing charms of Sir Arthur Helps's well-known style; and dry as the subject looks at first sight, it is full of interest and novelty, of details that few are acquainted with, and which are yet well worth knowing. The book is worth publishing, were it for nothing else than to make the world acquainted with a man of the late Mr. Brassey's exceptionally superior character and great power of organisation; indeed the author tells us that it is as an example of skilful organisation that the life of Mr. Brassey has especial interest for him. We do not intend to give any sketch of the life of the great and universally loved and respected railway contractor; his life, in one sense, takes up the story of the steam-engine where Trevithick's leaves it off. Trevithick and such as he show how steam may be applied to the purposes of locomotion, which brings into play a new set of men, a new profession, as it were, that of contractor, whose business it is to see that suitable roads are made for this horse of man's creation to run upon. That this may be done it is necessary for the contractor to procure an army of navvies and others, officered by all grades of superintendents, from the ganger up to the sub-contractor or agent. Of how much importance careful organisation is in a case like this must be seen by every one, and of as much importance is it that every one, from the engineer and contractor down to the ganger should have a thorough and intelligent—in other words, a scientific knowledge of the department of which he has immediate charge. This was what distinguished Mr. Brassey above many others in his position; he did not work merely by rule of thumb, but raised the business of contractor almost to the dignity of a science; and the thoroughness which was the result of this, combined with the man's noble character, obtained for him the great reputation and extensive employment which he all along possessed. One lesson which this work teaches above others is that the better educated, the more intelligent, in short, the more scientific, are all, from the highest to the lowest, who are concerned with the practical carrying out of the application of any scientific principle, the more thoroughly and satisfactorily will the work be performed.

The author does not say much—indeed little could be said—about Mr. Brassey's personal life, the greater part of the book being occupied with exceedingly interesting and varied details as to railway construction. Mr. Brassey, as a contractor, we believe, was more or less intimately connected with the construction of many thousand miles of railway in all parts of the world—Europe, Asia, North and South America, Australia—thus bearing a large share in carrying one of the greatest blessings science has conferred upon humanity, to all the ends of the earth. Not only was the railway itself a great benefit to the country into which it was brought, but the mere process of construction was a boon to thousands of its inhabitants. There are extensive districts in France in which the material prosperity of the inhabitants has been permanently raised by the savings which the French labourers realised at the construction of the Paris and Rouen railway. With whatever railway, in whatever country, Mr. Brassey had to do, he always liked to have the principal work done by English navvies; this was even the case in Canada; and some of the most interesting pages of the biography are those in which the railway labourers of other countries are contrasted, in point of character, powers of work, &c., with those of England. Taking them all in all, the latter cannot be beaten for quantity and quality of work. Sir Arthur Helps adds a chapter on "Railways and Government Control," in which he seems to think that it is now high time for Government to take the railways out of the hands of ignorant, irresponsible, conscienceless speculators, and work them itself solely for the public good, which is the last thing thought of by the present managers. One sentence is worth quoting here; we wish we had room for more:—"It has always appeared to me to be one of the most miserable instances of the hide-bound nature of our official system, which is hampered by so many checks and so much dread of small expense, that the most needful undertakings have to be passed by, or touched but lightly, which require the best intellectual force of the nation to be brought to bear upon them."

Mr. Fairlie's book, "The Battle of the Gauges Renewed," proves the truth of what we have already said—the necessity there is that all who are officially connected with railways should be able to perform their work on the basis of scientifically-grounded knowledge, and not in the light of tradition and custom, by unintelligent rule-of-thumb. Both by Mr. Herbert Spencer and Mr. George Darwin it has been recently pointed out that the construction of our railway carriages, instead of being based on a rational attempt to adapt them to new and previously undreamt-of circumstances, is simply a continuation, or rather development, of old forms belonging to the lumbering stage-coach and rude tramway days. That it is so with regard to the common gauge of wheels (4 ft. 8½ in.) is well known, though one would at first sight be inclined to believe that the odd half-inch was significant that this gauge was the result of a careful calculation grounded on the best mechanical principles. That this is not so can be learned from Mr. Fairlie's vigorous, clear, and, notwithstanding the apparent dryness of the subject, really interesting book. When railways were first constructed, about forty years ago, men were too much absorbed in the excitement of the new means of locomotion to give any thought to such an apparently trifling detail as the width that should be maintained between the two rails; and thus the gauge which was in general use among the old and rudely constructed tramways was adopted at mere haphazard, without any thought as to whether there was any good reason for adopting the 4 ft. 8½ in. How old the gauge is, and how it originated, probably no one knows, though we believe that even now not a few railway directors, and even engineers, will be found who maintain that it is a heaven-born institution, and that to alter it would be the height of irreverence and sacrilege, betraying an independence of thought and action worthy only of a nation like America, destitute of tradition. It is not our purpose here to advocate any one gauge as preferable to another, but simply to say that one lesson taught by the three books at the head of this article is, that the present condition of our railways is, to a large extent, the result of mere guess-work, and that only when the construction of every detail, from the steam-engine down to the gate at a crossing, or a pointsman's box, is conducted on rational, *i.e.* scientific principles, can the public, as well as the shareholders, be able to reap without drawback all the advantages which the great application of the power of steam is calculated to produce. On every ground it seems difficult to resist Mr. Fairlie's arguments on behalf of the 3 ft. 6 in. gauge, or even a narrower gauge under certain circumstances; it has been extensively adopted in America, and exclusively in Norway; and, we dare say, most people would be astonished to hear that at Festiniog, in Wales, on the face of a steep mountain, with gradients of 1 in 68 and 1 in 79, and with curves varying in radius from 8 chains to 1¾ chains, there has been at actual work for some years a railway with the miniature gauge of 1 ft. 11½ in. "It is, in fact, the most perfect miniature railway in existence, and deserves to be studied in all its details." Both as a passenger and mineral railway, it has done hard and perfectly efficient work for some years. This, at least, shows that the question of "gauge" is worth being inquired into, as indeed ought every other point connected with the construction and management of railways.

The revelation which results from one inquiry after another, and the harrowing effects of the dreadful accidents which are almost daily occurring, will doubtless have their effect on the public mind, and urge the people of this country either to compel Government to take the railways into their own hands, or, at least, to see to it that they are managed in some kind of rational and intelligent way, for the good of the public, and not for the sole benefit of a few obtuse directors.

M. de Quatrefages, in his opening address at the meeting of the French Association, was too sanguine when he said, "Science is at present supreme," though we believe he was right in asserting, "She is becoming more and more the sovereign of the world;" and only when Science reigns supreme over all the practical affairs of men, shall we be on the high road to perfection, in this direction at least.

OUR BOOK SHELF

Elementary Geology. A Course of Nine Lectures, specially adapted for the use of Schools and Junior Students. By J. C. Ward. (London: Trübner & Co.)

This little volume is a praiseworthy attempt to popularise the study of Geology. The descriptions and explanations are, for the most part, well done, and will be easily followed by those for whom the book has been written. The introductory "lectures," which treat of the origin and classification of rocks, of geological agents, of waste and renovation, and of physical geography, are the most satisfactory. When the author comes to deal with the geological history of the English formations, the necessity for condensation often leads him into obscurity; but upon the whole he has managed to give a more readable account than will be found in other introductory lesson books. As the lectures are addressed to a popular audience, we ought not, perhaps, to object to the fine writing in which the author is prone to indulge. But if his little book should come to a second edition (as we hope it may), he might tone down the "beauties," and his work be none the worse, but all the better for the process. Especially would we advise him to expunge the absurd and incoherent "Geological Dream on Skiddaw," and substitute for it a simple and intelligible summary, such as we are sure he is quite capable of giving. The illustrations are unequal; none of them are very creditable works of art, and some are so smudgy as to be almost illegible; but for the most part they serve their purpose.

J. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

London University Examinations

Mr. Christopher Heath, in his Introductory Address at University College, puts forward the following question, set in the Preliminary School Examination, in support of his remarkable statement that Mechanical and Natural Philosophy have little to do with Medicine:—

"Calculate the quantity of heat lost per hour from each square metre of the surface of an iron steam boiler 0·8 centimetres in thickness, when the temperature of the inner surface of the boiler is 120° and that of the outer surface $119\frac{1}{2}°$, the coefficient of conductivity of iron being 11·5° (referred to 1 cm. as unit of length, 1 min. as unit of time, and the quantity of heat required to raise the temperature of 1 gramme of water from 0° to 1° c. as unit of heat).

"*Solution:* A difference of temperature of 1° in a thickness of 1 cm. of iron will give a loss of 11·5° in 1 min. from a surface of 1 sq. centimetre."

But a difference of $\frac{1}{2}°$ in the thickness 0·8 cm. is at the rate of $\frac{1}{2} \div 0{\cdot}8 = \frac{5}{8}°$ in the thickness of 1 cm. Hence the loss in 1 min. from 1 sq. centimetre is $11{\cdot}5 \times \frac{5}{8} = 7{\cdot}1875$. Hence the loss in the same time from 1 sq. metre is 71875 units. The loss of heat per hour will now be evident.

Now this is what Mr. Heath designates as a problem on steam boilers which a medical man can never want to solve; would he say that it is entirely foreign to the subject, and expect any member of Senate or Convocation to bear him out, if the examiners inquired how much heat a man would lose through a blanket or through a sealskin coat 0·8 cm. in thickness, &c.? Yet the question is the same, and the iron jacket of the boiler or the sealskin coat are only accidents. Should not a medical student have some idea of the relation between the surface temperature of the body, the quantity of heat passing away from it, and the amount of heat generated in the body by the food given to a patient? Is the production of heat in the human body by the consumption of food carried on on principles so entirely different from those of the production of steam in a boiler that a medical student can afford to be ignorant of and to despise the simplest principles of heat, and to be unable to answer the mildest questions in that subject? Moreover, is it so clearly shown that "the two learned professors have such singularly incorrect ideas as to the requirements of medical students" when they ask a simple question as to the loss of heat from a hot body? It may be that Mr. Heath passed his first M.B. examination before it was considered a matter of importance to note the changes of temperature of the body, or before the use of thermometers by the Faculty, and that he regards those who are guided by such things in their treatment of a patient as altogether Utopian in their ideas. The above consideration of the question may perhaps be a sufficient answer to the shallow statement of the editor of the *Lancet*, in support of Mr. Heath, that "the relation of the question to medical requirements is absurd on the face of it."

As regards the examiners, Mr. Heath is not quite correct in his statement of facts, for the present examiners are not the examiners on whom the sub-committee of Convocation reported four years ago. With regard to the candidates who are rejected at the preliminary scientific examinations, has it ever occurred to the sub-committee of Convocation to inquire of the examiners what standard is actually required for the Pass Examination? If they have not obtained this information from the examiners themselves, their decisions can have very little weight, for they cannot be in a position to judge whether it is from the high standard set by the examiners or from the bad quality of the work that so many are rejected.

What stronger evidence could be adduced of the great value of the Preliminary Scientific Examination than the report of this Committee that "it has tended to give prominence to theoretical and scientific knowledge," seeing that it is in consequence of such knowledge that medical science has advanced with such rapid strides, and that in many cases the whole course of medical treatment has been changed.

The pages of the number of the *Lancet* in which Mr. Heath's lecture is contained, show clearly that to the surgeon, as well as to the doctor, a knowledge of mechanical as well as natural philosophy is of the first importance. Take, for instance, the case reported on page 490 of that journal.

How natural for a man who understands the laws of pressure of air, to apply the cupping-glass for the elevation of depressed cranial bone, in place of an operation which kills in seventy-five cases out of a hundred! Can a surgeon dare to be ignorant of these laws, when the consequences of neglecting them may be so disastrous

It is satisfactory to find, on turning to other medical schools, that it is not the general opinion that the study of Natural Philosophy may be neglected, but rather that "it is matter for regret that more prominence is not given to Physical Science;" for "it is in Physics that we find the explanation of a great mass of medical phenomena; and to the student who has not attained considerable proficiency in that science, many of these phenomena must be unintelligible."

Such being the case, the student will readily see that it will be for his best interests, and will best promote his future usefulness as an intelligent medical man, to acquire a thorough knowledge of the first principles of Mechanical and Natural Philosophy: in so doing, he will have the additional advantage that he will not run such risk of being landed among those who are rejected at the examinations of the University of London, and that not by a severe examiner, but through the ill-advice of which he has been the victim.

W. G. Adams

Physical Laboratory, King's College, Oct. 19

Solar Spectroscope Observations

In Nature of the 17th inst. there appear letters from Col. Tennant and Mr. Capron, who seem to doubt that the solar prominences can be seen in England with the facility described by Capt. Herschel in India. I might almost apply Capt. Herschel's words to my own experience last month. With a seven-prism direct-vision spectroscope of Browning (open slit) attached to a $2\frac{1}{4}$ inch glass mounted on a drawing-room stand, not only the bright lines, but the forms of the prominences, could be plainly seen and were sketched. Of course there were many cloudy days which prevented observations, and there would be many more such in England than in India; but it does not require exceptionally fine weather, only a great deal of practice. Experience only will tell the exact distance at which the slit must be from the sun's limit, and the slightest movement will either put the prominence out of the field or swamp it with a flood of light.

Blackheath, Oct. 18

J. P. Maclear

An Additional Note on American Arrowheads

As Mr. Evans has commenced his remarks on North American arrowheads with the assertion, "A prevailing type," &c., it naturally leads one to conclude that that form to which he refers is "the" prevailing type. I judge, further, that by "prevailing," he means "predominant," *i.e.*, in excess of other types or shapes of these relics. I do agree with Mr. Evans that it is *one* of the principal forms, but not so far a "prevailing" type as to merit the assertion of Mr. Evans, made *as* that assertion is.

As to the leaf-shaped form, I have but to remark that, in my own collecting experience, the true leaf-shaped, *i.e.*, the rounded-based, straight-sided, acutely-pointed form, such as Mr. Evans's Fig. 282, constitutes about 4½ per cent. of those gathered in my own neighbourhood; and this I know to be the experience of other collectors, in other and distant localities. This, I submit, is sufficient to show that this form is not even "comparatively rare," but may be better described as "not uncommon."

And so far as workmanship is concerned, I have only to say that if narrow, thin barbs, acute points, sharp edges, and smoothness of the broad surfaces of arrowheads, constitute what has been termed "delicacy of workmanship," then the American forms in horn-stone, jasper, chert, quartz, agate, and some finely-grained slates, cannot be excelled by similar relics found elsewhere, or made from other material. This remark I base on the specimens collected by myself here in New Jersey, which State is not the best locality, by any means, for gathering these objects; and I have found that the western, northern, and southern specimens have excelled those I have collected here at home.

From the above paragraph I have purposely omitted the mineral obsidian, because the arrowheads of this material excel all others wheresoever found, and I wish to make good my assertions without the help of Californian specimens.

I take the liberty of referring those persons interested in these matters to a large series of arrowheads collected here in New Jersey, and now in the collection of Sir John Lubbock. If some of these are not equal to *any* English specimens, I must simply "give in." Chas. C. Abbott

Trenton, New Jersey, U.S.A., Oct. 10

Merrifield on the Deviation of the Compass

As a review of my little manual on "Deviation," &c., has appeared in Nature for October 17th, in which I am accused of having "written with looseness and inaccuracy," perhaps you will kindly allow me a small space to answer my critic. He has selected a most unfortunate example to bear out his assertion; and I contend for the accuracy of my statement. "Vertical iron, at the same place, will produce the same deviation in whatever direction the ship's head may be." Here I do not pretend to say (as my reviewer insinuates), that the whole deviation is the same in every position of the ship's head; but I maintain that that part due to *vertical* iron remains constant for the same place until a change of secular variation becomes cognisable. I am at a loss to discover either looseness, inaccuracy, or substitution of cause for effect in what follows, and I am inclined to think, if any exists, it must be on the part of my reviewer

Again, I am not aware of any "singular statements and conceptions" in the larger work on "Navigation and Nautical Astronomy," which are at variance with the matter contained in the small manual under consideration. Perhaps you will kindly permit my reviewer to mention some.

I grant there may be differences of opinion on the merits of any work (as is fully proved in the present instance); but when public statements are made of "inaccuracies," these should either be substantiated or withdrawn.

John Merrifield

Navigation School, Plymouth, Oct. 19

Earth Currents

It may be interesting to record that during the past few days we have been subject to electric storms, I think I may say unparalleled in their frequency, intensity, and duration. On Oct. 14 a severe one raged from 9.30 A.M. to 1 P.M., and in the evening from 10.20 P.M. to past midnight. On the 15th a still more severe one raged from 9.30 A.M. to 1 P.M., and in the evening from 7 P.M. to 9.5 P.M. On the 17th inst. the currents were very embarrassing from 11.20 A.M. to 12.45 P.M., and from 2.10 to 9.0 P.M.; and on the 18th they were again troublesome.

They differed but little in their character from those usually observed, the currents continually varying in intensity and duration during the periods named. The interruptions to business on the 15th were serious, and many stations could only be communicated with by looping the wires, when more than one existed, into metallic circuits. Circuits running east and west were mostly affected, those running north and south, for instance, between London and Brighton, being but little disturbed. I regret to say that no precise measurements of the strength of these currents have yet reached me.

Southampton, Oct. 21 W. H. Preece

Aurora Borealis

On Saturday, Oct. 6, I was walking in our large playground with a friend, about 8.40 P.M., when we saw above us a magnificent red "way" whose direction was E.N.E. When we first looked, this broad band was bifurcated towards the E. end, one fork going more to the east, and the other to the north. In a minute or two this bifurcation disappeared, and in three minutes more the whole had disappeared, leaving the sky as before. It could be nothing but an aurora; at any rate, it was not the light of any fire, it was too magnificent, and for the time that it lasted of too great a length. I took no notes at the time, but feel that I have given you a correct account of the phenomenon, as far as it goes.

Christ's Hospital, Oct. 20 F. Jeffrey Bell

Ocean Currents

It is to be regretted that the correspondents of Nature, who for some weeks past have been writing on the subject of Ocean Currents, should ignore the consideration that it is primarily a question of geographical fact, and that any theory which runs counter to that needs no more elaborate confutation than a bare statement of the fact, supported, if necessary, by authoritative evidence. Thus, then, when we find the effect of the earth's rotation put forward, in the way it has lately been, by Mr. Ferrel and Prof. Everett, it is quite needless to examine the calculations which have been adduced; it is sufficient to say that the conclusions arrived at are contrary to geographical fact; that currents do not by any means universally turn to the right in the northern, or to the left in the southern hemisphere;—to name a few amongst many, the Gulf Stream turning to the left round Cape Hatteras, and again towards the coast of Ireland, Rennell's current, the Agulhas current, the Cape Horn current as it turns south near Chiloe, the current through Behring's Strait, are cases in point, currents turning in a manner exactly opposite to that deduced from the theory.

Similarly, when Dr. Carpenter, whether supported or not by several distinguished phycisists, argues from the effect of great differences of temperature in a small trough, as to the effect of much smaller differences of temperature in the incomparably larger ocean, it is unnecessary to follow him into his reasonings, for the conclusion, as he has lately stated it, that there is "necessarily an upper flow from the equator towards the poles," is geographically false. Over a very great part of the North Atlantic there is no prevailing set at all; in the South Atlantic and in the South Pacific the set has a general though slight tendency towards the north; the East Greenland current, the North African current, the South African current, the Peruvian current, and many others, run strongly towards the equator; there is nothing at all resembling a general "upper flow from the equator towards the poles."

As a question of abstract mathematics, Mr. Ferrel is undoubtedly at liberty to prove that every current in the northern hemisphere turns to the right, as much as he was, a few years ago, to prove that there is no air within some twenty or twenty-five degrees of the poles; and as a point of experimental science, Dr. Carpenter's illustrations are pretty, and can be understood without the authority of the distinguished men whose names he brings forward; though I should be loth to believe that Mr. Hawksley, or any other experienced hydraulic engineer, would agree with his idea that water will always find its own level.

But the application of the mathematical problem or the experimental illustration to the case in point is quite a different thing;

they lead to conclusions which we know to be geographically false, and we therefore refuse to accept them.

Royal Naval College, Oct. 16 J. K. LAUGHTON

Fossil Oyster

Ostrea callifera from the Hampstead beds is described at page 145, and figured on Plate I., of Forbes's "Tertiary Fluvio-marine Formation of the Isle of Wight." Perhaps this is the one "Inquirer" has found. T. G. B.

THE PENNATULID FROM WASHINGTON TERRITORY

I PRESUME this disputed organism, referred to in two communications in your number for September 26, is specifically identical with a specimen from Frazer River, British Columbia, presented to me in the autumn of last year, for the Museum of the University, by Mr. Selwyn, Director of the Geological Survey of Canada, and which had been obtained by Mr. Richardson, one of his assistant geologists. I at once recognised it as the axis of a Virgularia, or some similar creature; but there being no means of reference here for the West Coast species, I submitted it to Prof. Verrill, of Yale College, who had no doubt as to its nature, but believed it probably to belong to an undescribed species. There being no sufficient materials for its description, Mr. Whiteaves of this city, who undertook the description of the marine animals procured by the Survey in British Columbia, merely noticed it in his report as an undescribed pennatulid. Its characters were stated by him in a paper read before the Natural History Society of Montreal last winter, and printed in abstract at the time. Mr. Richardson, who returned to British Columbia in the spring, has undertaken to procure, if possible, a perfect specimen, and to have it preserved in alcohol. Should he succeed, we may hope soon to have materials for the description of the species. Mr. Selwyn's specimen, though it has probably lost several inches of its length, being broken at both ends, is five feet one inch in length. It retains, attached to the granulated lower extremity, some traces of animal matter, in which I think I can detect, under the microscope, a few club-shaped spicules.

McGill College, Oct. 11 J. W. DAWSON

DR. HOOKER'S REPLY TO PROF. OWEN

THE Blue Book issued in August last, containing the correspondence between Dr. Hooker, Mr. Ayrton, and others respecting the management of and control over Kew Gardens, included also, in the form of an appendix, a statement addressed to Mr. Ayrton by Prof. Owen, containing various allegations detrimental to the present management of the gardens, herbarium, and museum. The following reply by Dr. Hooker to these allegations has just been printed by order of the House of Commons:—

"Prof. Owen divides the 'aims and applications' of the Royal Gardens of Kew, according to his view of them, under seven heads.

"It is sufficient to state that some of these are recognised by the Government, and specified in their instructions under which the Director carried out his duties; but that others, and those of a most comprehensive nature, have no place there, and are not such as pertain to botanical gardens elsewhere. Amongst these are the agricultural operations specified by Prof. Owen, 'the application of manures, demonstrations of the fittest species of grasses for particular soils . . . methods of irrigation, subterranean pipe, conveyed liquid manures, and so forth,' all of which are being carried out with vigour and success by various agricultural societies and private individuals throughout the country.

"To establish such operations at Kew would involve an enormous expenditure, and occupy many acres of ground now devoted to the legitimate purposes of a botanical garden.

"Illustrations of rock-works, garden sculpture, and ornamental waters, also recommended by Prof. Owen, appear to be equally out of place.

"Prof. Owen is in error in stating that the arrangement of plants in natural groups, with conspicuous labelling, &c., is at Kew 'at present limited to the herbaceous grounds;' as he is also in implying that there is no illustration of 'geographical distribution,' which is, in truth, carried out to an incomparably greater extent at Kew than in any other garden known to me at home or abroad. Prof. Owen cannot have visited the houses devoted to ferns, orchids, succulents, aroids, &c., nor the arboretum, fruiticetum, and pinetum, nor observed the arrangement on the shelves of the two great buildings, the palm stove and the temperate house.

"The fact that a first-rate herbarium and library must be maintained for the purposes of a botanical garden, and in immediate proximity to it, has not only been uniformly admitted and acted upon by successive Governments, but is so universally recognised by naturalists everywhere, that I am surprised that Prof. Owen should dispute it.

"I am sure that were he acquainted with the nature and amount of the duties devolving on this establishment, he would abandon his opinion without hesitation.

"In support of the contrary opinion he refers to that early period in the history of Kew, when its new and rare plants were named at the Banksian herbarium in London. But the naming of a few new and rare plants cultivated at the beginning of the century in a private garden of nine acres, probably at no one time containing more than 4,000 species, is a very different matter from keeping accurately named public collections that occupy 300 acres, and are estimated to contain 20,000 species; and this in an establishment that is annually called upon to name literally thousands of plants from other botanic gardens and nurseries in England and similar institutions abroad. A great deal of the naming, and keeping correctly named, the plants at Kew, can be conducted only by skilled botanists visiting the grounds daily. Large classes of plants are now cultivated that must be named in the houses where they grow; and many more, the tropical especially, could not be sent to a distance to be named, without serious damage *in transitu*.

"To this must be added the necessity of naming and ticketing with copious information the vegetable products of economic interest, in three museum buildings, the illustration of which products by specimens, Prof. Owen admits to be a legitimate object of the Gardens of Kew.

"Nor was the naming of the Kew plants carried out in London, as is supposed; there was a large herbarium in constant use at the Royal Gardens at the very period alluded to, the breaking up of which, when it was proposed to give up the Gardens, necessitated the formation of another.

"No comparison whatever can be instituted between the needs in these respects of the Royal Gardens at Kew and the Zoological Society's Gardens in the Regent's Park.

"The reflections that follow on the conduct of the late and present Directors of Kew Gardens are not suited for official discussion.

"Prof. Owen is in error in asserting that the main end or drift 'of Dr. Hooker's evidence before the Scientific Commissioners is to impress upon them the necessity of the transfer of the collection of dead plants' from the British Museum to Kew.

"My evidence is unequivocally opposed to such a transfer.

"Herbaria are not costly establishments, but the least expensive of all natural history collections; and the objects and applications of botany in its largest sense, are now so numerous and so important, as to render a division of the subject necessary; whence the expediency of maintaining a country and a metropolitan department, each with a herbarium, as the most essential, but least expensive of its adjuncts, may readily be demonstrated.

"So far from desiring that the British Museum herbarium should come to Kew, I should propose to recruit it from that at Kew, which could be done to its very great advantage.

"Prof. Owen's approval of the saying of 'a great wit and original thinker,' that 'the net result' of a herbarium is the 'attaching barbarous binomials to dried foreign weeds,' will not find an echo amongst those conversant with the subject. Had it been otherwise, successive ministers would hardly have tolerated the existence of the Kew herbarium, or of that at the British Museum either.

"The disparaging remarks that follow on the views of his duties held by the late director, and on his performance of them, are not best dealt with by the counter assertions of his son; they are best disposed of by certain passages in the Treasury Minute that follows Prof. Owen's statements, and by the unanimous verdict of the late director's countrymen and foreigners everywhere.

"The suggestion is offered that an official inquiry should be made of leading gardeners to ascertain 'the kind and degree of information and aid which they derive or have derived from the National Establishment.'

"The answer to this has already been given, in the addresses to the Premier by the Royal Horticultural Society as a body, and separately by its Floral, Fruit, and Scientific Committees; and by the meeting of botanists and horticulturists held in London; and by the concurrent evidence of gardening periodicals throughout this country.

"The statement that the Royal Gardens had not fulfilled their function of introducing new, rare, and beautiful plants is best met by a reference to the pages and illustrations of the *Botanical Magazine*, a work that has issued monthly (and without a month's intermission) from Kew, ever since 1840, edited by the Director, and which is devoted to new, rare, and interesting plants, the larger proportion of which have flowered at Kew.

"The passage relating to the avenue of deodars and limes along the Syon vista, the formation of which is censured as a failure at the cost of 'hundreds or five hundreds' of trees, is founded on a complete misapprehension. Without going into detail, it is sufficient to state that not twenty deodars have been sacrificed, and no limes at all.

"The censuring of the Director for removing the araucarias from Richmond Park to Kew is equally founded on a misapprehension. These araucarias were twice offered to Kew before they were accepted; they stood in a private piece of ground, whence their removal was considered by their possessor to be a necessity; and the alternative of removal to Kew was their destruction.

"My predecessor is censured for neglect of the great araucaria, which, it is implied, is consequently inferior to that of Dropmore. The facts are as follows:—

"This araucaria, with four others, was brought to Kew in 1796, and kept in a greenhouse.

"In 1808 it was planted out in a poor sandy soil, and being supposed to be tender, was enclosed in a wooden house for many months in the year, in consequence of which its growth was checked, and it was rendered so weak that it was almost killed in 1838.

"It was not till the late Director took office in 1840 that the house was abandoned, good soil given to it, and other means taken (which have been sedulously repeated ever since) to encourage its growth.

"It is now a striking object 30 feet high and 90 in girth of the branches; and if not nearly so handsome an object as the Dropmore araucaria, this is partly due to the fact that the Dropmore tree was planted out at once, in a soil and situation as admirably adapted to araucarias as those of Kew are naturally unsuited to them; and partly to the fact, probably unknown to Prof. Owen, that there are two very distinct forms of this species, a conical, and a round-headed, of which the Dropmore specimen belongs to one, and the Kew specimen to the other.

"Of the other four plants, one is that now at Dropmore; a second was killed by cold at Kew early in the century; the third was given to Sir Joseph Banks at Spring Grove; and the fourth at a later period, to Prince Albert, and taken to Windsor.

"In the contrast drawn between the herbarium establishments at the British Museum and at Kew, it is stated that the staff of the former consists of three officers, with aggregate salaries of 850*l*., and 'that their time is exclusively given to the duties for which they are paid;' whereas the aggregate salaries of the three herbarium officers at Kew is 750*l*., and that one is Professor of Botany in University College, and another a lecturer at a London Medical School.

"I am surprised that Prof. Owen should be unaware that one of his own three officers is botanist to the Royal Agricultural Society, and another a lecturer at a London Medical School, and editor of a valuable botanical journal.

"Nor does Prof. Owen in his comparison take into consideration that the Kew herbarium is open from 8.30 A.M. till 5 P.M. in winter, and 6 P.M. in summer, whereas the British Museum herbarium is open only from 10 to 4 in winter, and 10 to 5 in summer; as also that the Kew officers have not only the keep of the largest and most frequented herbarium in the world, but of a very large library, and have the duty of naming all the plants throughout the gardens and museums, together with many other duties that do not fall upon the British Museum officers.

"The fact is, that the exigencies of this establishment require that the herbarium should be open during that long period, but the officers are not required to be in attendance, and at their work, for more than seven hours daily throughout the year.

"Those seven hours (and to their honour be it said, often many more) are devoted exclusively to the duties of their respective offices.

"That the officers both of the British Museum and of Kew should be chosen to conduct the very brief professional and other duties which they perform elsewhere (at their own time), is both honourable to themselves, and in many ways advantageous to the establishments with which they are officially connected, always assuming that these vocations do not interfere with their working hours at Kew, and at the British Museum, or with their powers of work during those hours.

"The statement that there are at Kew 'a special curator of the museum, &c., and an assistant at 315*l*. per annum,' is an error.

"There is but one curator for the three museums, and his salary is 120*l*., rising to 150*l*., without a house or any other advantage; he has no assistant, and never had one.

"The last of Prof. Owen's statements to which I shall allude are the following, which I quote *verbatim*:—

"'Dr. Hooker has been enabled to publish, or aid in the publication of, 130 volumes on botanical subjects. . . .

"'To the extent or proportion in which the Director's time has been diverted from the immediate aims of the Royal Gardens to this foundation of his scientific fame, the proportion of his salary of 800*l*. per annum must also be placed to his credit of the superaddition of the dead plants to the Botanical Department under the Board of Works, competing with the Botanical Department under the Trustees of the British Museum.'

"The first statement in this extract has no foundation in fact; it would ill befit me to notice the insinuation contained in the last.

"(Signed)
"JOS. D. HOOKER, Director
"Royal Gardens, Kew, Aug. 6, 1872"

THE NEW RHINOCEROS

THE acquisition of a living Rhinoceros belonging to a species hitherto unknown to science, is certainly a remarkable event, and one that may well give comfort to those who may have supposed that the field of zoological discovery is worked out. If so large a beast has hitherto escaped the observation of naturalists, how many smaller animals must there still remain for the zoological explorer. But the truth is that we know less about some of these very bulky animals than those of more moderate dimensions, as their very size renders the collection and preparation of specimens of them more difficult. The importation of such monsters in a living state is a still more serious undertaking, and it is only within the last few years that the Zoological Gardens of Europe have become wealthy and enterprising enough to find funds for such expensive luxuries.

The animal of which we are now speaking, was originally captured near Chittagong, at the northernmost extremity of the Bay of Bengal, in January 1868, by some officers engaged in the supply of elephants for the Indian army. Some natives came into the station, and reported that a rhinoceros had fallen into a quicksand, at a place about sixteen hours' journey to the south, and had been unable to extricate itself. They had pulled it out by ropes attached to its neck, and had bound it between two trees, but were fearful of its breaking loose. Captain Hood and Mr. H. W. Wickes accordingly started with eight elephants, and brought the rhinoceros into Chittagong, where she was kept in a stockaded enclosure, "having a good bath excavated in the ground, and a comfortable shed attached to it." Here "Begum," as she was named, remained for nearly four years. Various negociations were entered into between the Zoological Society of London and the capturers, for her removal to this country, but these never came to any definite result. Besides the difficulty of arranging terms at such a distance apart, there seemed to be some question about the true ownership of the animal, which created additional embarrassment in the transaction. At length in the autumn of 1871, Mr. William Jamrach, the well-known dealer in living animals, being personally in Calcutta, was able to conduct the negociations to a successful result, and on his return to England, in February last, brought the animal with him. Upon her arrival "Begum" was immediately purchased by the Zoological Society, to whom, it was understood, the first offer was to be made, for the sum of 1,250*l.*

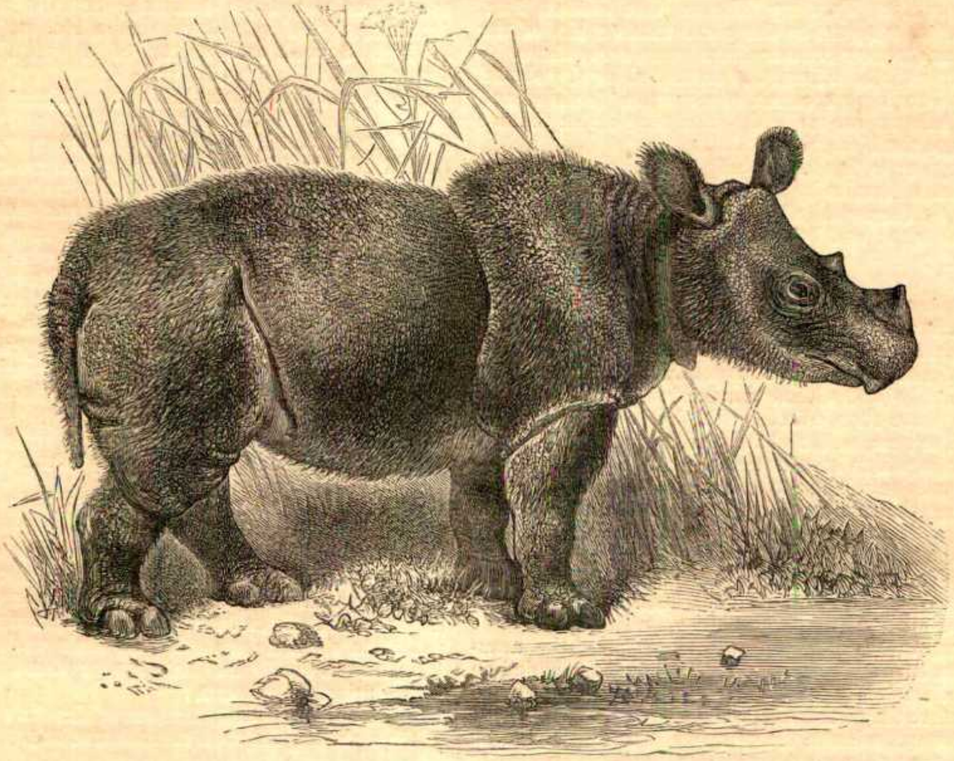

FIG. 1.—SUMATRAN RHINOCEROS

During the transit through Calcutta, this rhinoceros was examined by Dr. John Anderson, the Curator of the Indian Museum in that city. Dr. Anderson, thinking it possible that the animal might not live to reach England, had some figures of it made by a native artist, and drew up some notes on its external characters, which he communicated to the Zoological Society of London. In these notes, which have been published by the Zoological Society in their Proceedings (P. Z. S., 1872, p. 129), Dr. Anderson supposes the animal to be a Sumatran Rhinoceros (*Rhinoceros sumatrensis* of Cuvier), but comments upon several points in which it seemed to differ from former descriptions of that species, and upon its occurrence so far north of the hitherto known range of that species. When the rhinoceros arrived in London it was likewise referred to *Rhinoceros sumatrensis*—that being the only known Asiatic species of Rhinoceros with two horns, and was entered under this name in the Zoological Society's register of accessions, and is so spoken of in the new edition of the "Garden Guide." It is likewise mentioned and figured under this name in an article on Rhinoceroses, published in this journal for the 28th of March last. The cut there given is now reproduced (see Fig. 2), in order to give an opportunity of comparing it with the figure (Fig. 1) of the true *R. sumatrensis.*

In July last Mr. William Jamrach received a female two-horned rhinoceros from Singapore, which is said to have been captured in a pitfall near Malacca, and placed it on deposit in the Zoological Society's Gardens. On comparing it with the female previously received from Chittagong, it became at once apparent that the two animals belonged to distinct, though nearly allied, species. The Malaccan animal, although undoubtedly adult, is much smaller—nearly as much as one third—than that from Chittagong. The fringe of long hairs on the posterior rim of the naked ears, which is very conspicuous in the Chittagong animal, is not present in the Malaccan example, in which, however, the whole interior of the ears is filled with short hairs. The whole body of the Malaccan animal is covered with coarse granulations, which are hardly apparent in that from Chittagong. The tail of the Malaccan animal is shorter and nearly naked; in that from Chittagong it is longer and tufted at the extremity. The head of the former animal is much narrower than that of the latter, as is particularly apparent when the distance between the ears of each is examined from a front view, and there can be no doubt that the skulls of the two animals, whenever they can be compared, will exhibit marked differences in size and structure.

Under these circumstances the Council of the Zoological Society thought it would be advisable to add the second animal also to their living collection, and accordingly agreed to purchase it of Mr. Jamrach for the sum of 600*l.* Unfortunately it did not live long in the Society's gardens.

Upon reference to authorities upon the Sumatran Rhinoceros, which was first described by Mr. William Bell in the Philosophical Transactions of the Royal Society for 1793, and afterwards by Sir Stamford Raffles in this country and by Cuvier and other writers in France, it became evident that the Malaccan animal was the true *Rhinoceros sumatrensis* of authors. This would be presumably the case, because the Fauna of the Britsh settlement of Malacca is nearly identical with that of the adjacent island of Sumatra. The Chittagong animal, its northern representative, is therefore proposed to be called the Hairy-eared Rhinoceros (*Rhinoceros lasiotis*), from its peculiar ear-fringe of long hairs, which has been already spoken of. How far the Sumatran Rhinoceros extends north along the Malayan peninsula is not yet ascertained, because, although Two-horned Rhinoceroses are known to occur in several intermediate localities, it is uncertain to which of the two allied species they belong. The range of *R. lasiotis* is likewise quite a matter of uncertainty at present, the animal being utterly unknown except from the individual in the Zoological Society's Gardens. But it is probable that it extends into Assam, where there are reports of the occurrence of a Two-horned species of Rhinoceros.

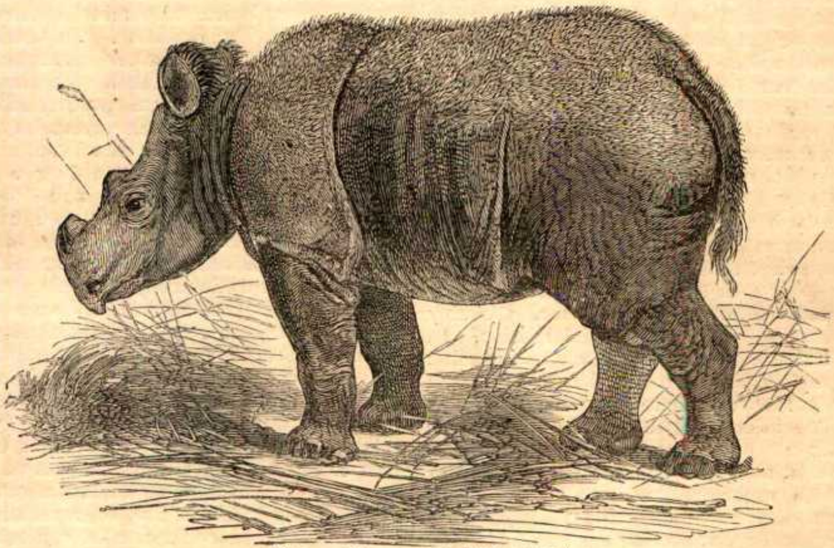

FIG. 2.—HAIRY-EARED RHINOCEROS *

Besides the two Rhinoceroses just spoken of, two other specimens of Asiatic Two-horned Rhinoceroses have been imported alive into Europe since the commencement of the present year. One of these was purchased by an agent of one of the American travelling menageries, and exported to New York; the other is now in the gardens of the Zoological Society of Hamburgh. Both these animals are said to have been received from Singapore, and to resemble exactly the Malaccan animal in London. Of the second a figure and description have been published in a Hamburgh journal (*Der Reform*), which shows that the animal is certainly the true *R. sumatrensis.*

By the addition of these two animals to their Menagerie the Zoological Society have now been able to exhibit side by side specimens of four (out of the six certainly known) living species of Rhinoceros—a wonderful advance, when we consider that a very few years ago the Indian *Rhinoceros unicornis* was alone known in Europe in a living state. The two species remaining to be obtained are the Javan Rhinoceros (*Rhinoceros sondaicus*), a smaller representative of the One-horned Indian, and the White Rhinoceros of Africa (*Rhinoceros simus*). It need hardly be added that any correspondents of NATURE who may be able to assist in supplying these desiderata will not only be conferring a benefit on science, but will be liberally dealt with by the Council of the Society. P. L. S.

RECENT FALLS OF METEORITES IN FRANCE AND ITALY

THE French Academy of Sciences has recently received several important and interesting accounts of the fall of two or three meteoric masses in France and Italy. On the 23rd of July, about half-past five on a still afternoon, with a perfectly clear sky and a bright sun, a violent report, followed by a rumbling noise, was heard in the commune of Lancé, canton of St. Arnaud (Loir-et-Cher). On the following day it was ascertained that the noise had been heard over a wide area of country, and had caused much uneasiness; and a letter arrived from a landowner of Lile-Bouchard, announcing that he had seen a "fiery lance" shooting across the sky in a direction from S.W. to N.E. with great swiftness. Whilst on its way its point appeared to split, giving rise to two meteors, which continued their way parallel to each other for some distance. Another observer south of Tours had also seen them, and described them as having the shape of a bottle, and being of an orange colour. M. De Tastes, who communicated the first account to the Academy, on proceeding to St. Arnaud, was fortunate enough to learn that one of these meteorites had been seen to fall near Lancé, and he was also successful in finding it. Its weight was 47 kilogrammes (about 103 lbs.), and it had penetrated to a depth of 1·40 metres (about 5 ft. 9 in.). On being removed, it broke into three pieces. Of the second meteorite nothing was heard for some time, but it was ultimately found at a place called Pont-Loisel, about 12 kilometres (7½ miles) to the south-west of the place where the other had fallen; and an account of it is given to the Academy by M. Daubrée. It is of exactly the same mineralogical character as the one first found, thus showing it originally belonged to it, but its weight is only 250 grammes, and it had only penetrated to the depth of about half a metre. On ascertaining the course of the meteorite, it was found that this, the smallest portion, had fallen first, and that the larger one had continued its course for some distance farther. In this respect it resembled the meteorite which fell on March 14, 1864, near Orgueil (Tarn-et-Garonne), in which the smallest portion, weighing about 15 grammes, first fell, and then the heavier one, weighing 40 grammes.

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* It should be stated that this figure is drawn on a smaller scale than that of the Sumatran Rhinoceros, the latter being really the smaller animal.—ED.

M. Daubrée has recently analysed the meteorite, and his results are somewhat remarkable. The largest piece is of an unequal spheroidal shape, with a rounded surface; it is covered all over with a crust, probably caused by the incandescence and superficial fusion. In appearance the fracture is black, and almost basaltic looking, showing a globular structure and numerous small spheroidal grains. Here and there small metallic grains are to be seen, yellow in colour, like iron bisulphide, these and other metallic-looking grains showing much better when the surface is polished. Its specific gravity was 3·80. Treated with water, a very small quantity of chloride of sodium dissolved out, and M. Daubrée remarks that this is not the first time that this salt has been found in meteorites; and he brings forward evidence to show that it could not have been derived from the soil in which the meteorite was buried, but that it must have formed part of it when it fell. No traces of any salts of potash, nor of any sulphates or hyposulphates could be found. Dissolved in nitric acid, a silicate was found, which was proved to consist chiefly of magnesium and protoxide of iron, and there was an undissolved residue, part of which was colourless, the remainder dark black. By means of spectrum analysis, copper was thought to be recognised; but calcium, barium, and strontium were shown to be absent. No carbon was found; but, as usual, cobalt and nickel accompanied the iron. The following is the complete analysis:—

Free iron combined with nickel and cobalt...			7·81
Iron and other metals combined with sulphur		9·09	14·28
Sulphur combined		5·19	
Silicate	Silica	17·20	42·42
	Magnesia	13·84	
	Iron protoxide	11·33	
	Manganese protoxide	0·05	
Part unattacked by acid			33·44
Sodium chloride			0·12
Water			1·24
		Total ...	99·31

In its general appearance this meteorite resembled that which fell July 11, 1868, at Ornans (Doubs), but differs from it in the absence of free iron oxide. Other characters distinguish it from the black meteorites of Rutlam (East Indies) and that of Tadjera, near Sétif (Algeria).

Several meteorites have also lately been seen in Italy, which have excited considerable attention. One on the evening of the 8th of August, at about eight minutes past 11, was seen at Rome, and also at Velletri, Naples, and Palermo. A more interesting one than this was seen near Rome, at about 5.15 mean time, on the morning of the 31st of August, of which Padre Secchi has communicated a long account to the Academy. At about 5.15 in the early morning on that day a globe of fire, well marked and a little red in colour, appeared on the horizon towards the S.S.W., proceeding towards the N.N.E. Its progress was at first slow, but this gradually increased, and it left behind it a luminous train like a cloud lit up by the sun. When it had reached its highest point, E.N.E. from Rome, it suddenly expanded and took the shape of a cone having its base rounded in front; it brightened up greatly and finally disappeared. Three or four minutes after its disappearance a tremendous detonation was heard, which caused, in many places, houses and glass to rattle. This explosion was dull, different to thunder, and resembling more the explosion of a mine, and was followed by a rolling sound like file-firing. This noise was heard by Padre Secchi himself, but he did not see the globe of fire. The vapour-like residue left by the meteorite was at first in the shape of a long straight line, but it soon enlarged, and turned about like some great serpent until it disappeared about ten or fifteen minutes afterwards.

This meteor was also seen a long way from Rome, at Viterbo and at Veroli, but the noise of the explosion in each place was equally strong, and caused houses and glass to rattle. A small piece of the meteorite which fell near him was picked up by a curé soon after the explosion at Affile, near to Subiaco, where the ball of fire and the noise of the explosion were well seen and heard. The fragment has been recognised as a piece of a very ferruginous meteorite, very hard, and covered over with a crust. It is also said that at Orvinio "black stones" have been picked up. But this is not all. A well-instructed farmer had assured me, says Padre Secchi, that the same morning at 3.30, being at Casale S. Lorenzo, near to Porto d'Anzio, whilst he was waiting for his men, he saw out at sea, at an elevation of about 30° or 40°, a mass of fire or light like a fire, of a round form, apparently fixed, and which could not be confounded either with a lighthouse or any fire at sea. The position of this fire was exactly the same as that from which the meteorite afterwards appeared, and which he saw very distinctly in the heavens at 5.15, when he was so much struck with the coincidence of direction that he judged it to be the same mass of fire which had then reached the earth. The size of the meteorite at its first appearance and at the moment of explosion is represented as little less than the diameter of the moon. The extreme distances at which it was seen are 150 kilometres (93 miles) apart.

Another meteorite was seen at Subiaco on August 6, at four in the morning; and another near Ascoli on the 18th of September.

J. P. E.

DARDANELLES AND BOSPHORUS UNDER-CURRENT

IT will be in the recollection of such of your readers as have followed the discussion on Ocean Currents, that I ventured nearly two years ago* to predict the existence of an Under-current of dense Ægean water into the Black Sea, "on the double ground of *a priori* and *a posteriori* necessity;"—that is, I affirmed it to be a necessary result of the excess of Specific Gravity in the water of the Ægean above that of the Euxine; whilst, I argued, if the salt continually passing out of the Black Sea by the surface-current were not thus replaced, the continual excessive influx of River water would, in time, wash the whole of the salt out of its basin.

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* Proceedings of Royal Society, Dec. 8, 1870, § 123.

My position was assailed by Captain Spratt, who affirmed (1) that his own experiments in the Dardanelles had shown the existence of still water beneath twenty fathoms; and (2) that the return of salt to the Black Sea was effected by a *surface* in-current during the winter, when the rivers are low, and when the wind sets *from* the Ægean along the Dardanelles, the Sea of Marmora, and the Bosphorus.

On an examination of Captain Spratt's experiments, however, I came to the conclusion that, when rightly interpreted, their results bore out *my* view of the case; and, as I stated in my letters of Nov. 14, 1871, my interpretation of them had the sanction of three eminent Naval Surveyors. Captain Spratt maintained that because a surface-buoy from which a "current-drag" was suspended at a depth beneath twenty fathoms *remains stationary*, the waters in which the "drag" hangs must also be *motionless*. To me, on the other hand, it appeared indisputable that if the surface-buoy is floating in a *current* which puts a strong strain on the suspending line, that strain would draw the "current-drag" through *still* water; so that the stationary condition of its suspending buoy can only be accounted for on the supposition that the action of the surface-current on it is *neutralised by some pressure in the opposite direction*, which can be nothing else than that of an under-current *meeting* the "current-drag."

The question is discussed in an Appendix to the forthcoming Report of my last year's work in the *Shearwater*, of which the following (written on board of her a year ago) is an extract:—

"Now since, according to Captain Spratt, this stationary condition of the 'current-drag' was shown at all depths below forty fathoms in the Sea of Marmora (even down to 400 fathoms), and at all depths below twenty fathoms in the Dardanelles, it seems an irresistible conclusion that whilst there is a rapid superficial *out*-current, running in the Dardanelles at the rate of 2¼ miles per hour, there is a deeper *under*-current from twenty fathoms to the bottom, running more slowly *in*wards from the Ægean into the Sea of Marmora through the Dardanelles, and thence, it may be presumed, through the Bosphorus, into the Black Sea. And this conclusion finds complete confirmation in the results of a comparison between the respective Densities and rates of movement of the Dardanelles water at different depths, as observed by Captain Spratt himself. For whilst the progressive decrease in the movement of the 'current-buoy,' from 2¼ knots at the surface to almost nothing at twenty fathoms, indicates (as just now shown) first a cessation of all movement in the stratum in which the 'current-drag' hangs, and then a reversal in the direction of the current as the lower depth is approached,—the Density increased from 1,020 at the surface to 1,028 at twenty fathoms, and 1,029 at forty fathoms; the surface-water thus corresponding with that of the Sea of Marmora, whilst thewater of the entire stratum from twenty fathoms to the bottom was equal in density to that of the Mediterranean. I hold, then, that the existence of an Under-current of dense Mediterranean water through the Dardanelles into the Sea of Marmora, is incontestably proved by the very experiments and observations which have been adduced by Captain Spratt as demonstrating the unsoundness of the Under-current doctrine."

Having understood that the *Shearwater*, on the completion of the Survey of the Gulf of Suez, would proceed to the Dardanelles, I requested the Hydrographer to direct that the question of the Under-current should be thoroughly examined; and he issued instructions accordingly.

I yesterday learned through the *Levant Herald*:—(1) that the existence of a strong Under-current has been placed beyond all question, a boat having been carried along by the "current-drag" suspended from it, *in opposition to the surface-current*: (2) that the rate of this Under-current is estimated as greater than the speed of the *Shearwater's* steam-launch; and (3) that it runs at a depth of twenty fathoms,—precisely that at which my interpretation of Captain Spratt's experiments has led me to predicate its existence.

I venture to think that this verification of my prediction will be regarded as a confirmation of the general Physical Theory of Under-currents on which it was based; and it is now for those who oppose that Theory to show by what other force than the difference in the *weight* of the Ægean and the Black Sea columns, consequent upon their great difference in Specific Gravity, the Dardanelles Under-current can be sustained.

WILLIAM B. CARPENTER

NOTES

MR. C. MELDRUM writes from Mauritius that he has been looking into the subject of the West Indian hurricanes, and he believes that they show a periodicity of frequency corresponding nearly with that of sun-spots. So far as he has yet examined the subject, the maximum of cyclone frequency is a year or two after that of sun-spots. It was so ten years ago, and is so again probably, the mean cyclone frequency occurring in 1862, and this year, 1872, being the most marked for hurricanes since. We greatly regret to hear that Mr. Meldrum is suffering severely from illness brought on by over-work. It will scarcely be credited that the only allowance made to him by the Government for an assistant is 50*l*. per annum!

WE learn from the *Gardener's Chronicle* that M. Milne-Edwards has undertaken a task for which all naturalists owe him thanks. The archives of the Museum of Natural History in the Jardin des Plantes contain a collection of 6,000 volumes and more than 1,500 manuscripts, which are almost entirely unknown to the scientific world; for nearly 60 years the dust that lay upon them has never been disturbed. In 1803 it was proposed to create a special department for these and other works and documents, but the idea was abandoned, and since that time the collection has remained huddled away in a corner, on account of space being wanting in the library of the museum. M. Milne Edwards has determined that such a state of things shall not be perpetuated, and has arranged that the collection shall be carefully examined, catalogued, and placed at the disposition of the scientific world. The manuscripts include a considerable number by Buffon, Cuvier, and Daubenton; there is a series of 24 pen-and-ink drawings by the last-named naturalist, representing the various types of Merino sheep, and exhibiting great artistic ability, and many albums filled with drawings of plants and flowers. It is proposed to add the books to the library of the museum, but there is so little space to be disposed of there, that it is expected the MSS. will be transferred to the great National Library, in the Rue Richelieu.

SIR DAVID BAXTER, who endowed in his life-time a Chair of Engineering at the University of Edinburgh, has by his will left the munificent gift of 40,000*l*. for the general purposes of the University.

THE death is announced, on the 16th inst. at Torquay, at the age of 75, of Lady Hooker, widow of Sir W. J. Hooker, K.H., formerly Director of the Royal Gardens, Kew.

MR. RAY LANKESTER, M.A., Fellow of Exeter College, Oxford, writes to correct the statement in our University Intelligence last week, that he has been appointed Deputy to the Linacre

Professor of Anatomy and Physiology in that University. He is merely delivering a course of lectures at the request of the Linacre Professor, which the Professor has hitherto been in the habit of delivering himself.

BY a majority of two to one the managers have decided to remit the question of the admission of lady medical students to the practice of the Edinburgh Royal Infirmary to a committee, to report "whether, and to what extent, it is practicable to give instruction to females within the wards of the Infirmary." The minority desired that arrangements should at once be made for the instruction of lady students. *Apropos* of this subject, a moiety of the 1,000*l*. recently promised by Mr. Walter Thomson to the funds of the Committee for securing a complete Medical Education of Women in Edinburgh has been forwarded to the Executive Committee; (1) in payment of expenses that have to be incurred in prosecuting the claim of women to the highest medical education obtainable in the University of Edinburgh and elsewhere; and (2) in assisting or encouraging lady students who have been subjected to extra charges by the obstacles interposed in Edinburgh.

A NEW University is to be opened on November 1, at Klausenburg, the capital of Transylvania.

MR. THOMAS HOWARD, of the King and Queen Iron Works, Rotherhithe, who was for 37 years an Associate of the Institute of Civil Engineers, has bequeathed to it the sum of 500*l*., free of legacy duty, which sum he has, by will, directed "to be invested, and the interest thereof to be applied in such manner and under such conditions and instructions as the Council of the said Institution may think most expedient, for the purpose of presenting, periodically, a prize or medal to the author of a treatise on any of the uses or properties of iron, or to the inventor of some new and valuable process relating thereto, such author or inventor being a member, graduate, or associate of the said institution."

THE following are among the publishers' announcements for the coming season :—By Mr. Murray :—The Expression of the Emotions in Man and Animals, by Charles Darwin, F.R.S. (with photographic and other illustrations); Records of the Rocks, a series of Notes on the Geology, Natural History, and Antiquities of North and South Wales, Siluria, Devon, and Cornwall, by the Rev. W. S. Symonds, F.G.S. (with illustrations); Travels in the Eastern Caucasus, on the Caspian and Black Seas, especially in Daghestan, by Lieut.-Gen. Sir Arthur Cunynghame, K.C.B. (with map and illustrations); The Geography of India, Ancient and Modern, an Elementary Manual for Students and General Readers, by Col. Yule, C.B. (with maps and woodcuts); The Longevity of Man, its Facts and its Fiction, including Observations on the more Remarkable Instances, and Hints for Testing Reputed Cases, by William J. Thoms, F.S.A.; Metallurgy of Gold, Silver, and Mercury, by John Percy, M.D., F.R.S., Lecturer on Metallurgy at the Royal School of Mines (with illustrations); The Geological Evidences of the Antiquity of Man, by Sir Charles Lyell, Bart., F.R.S., 4th edition revised; Metallurgy of Fuel, Wood, Coal, Copper, Zinc, &c.; also, Metallurgy of Iron and Steel, by John Percy, M.D., F.R.S., Lecturer on Metallurgy at the Government School of Mines (new and revised editions, with illustrations, 2 vols.); Siluria: a History of the Oldest Rocks in the British Isles and other Countries, by Sir R. I. Murchison, F.R.S. (5th and cheaper edition, with maps, plates, and woodcuts). By Messrs. Macmillan :—The Forces of Nature, a Popular Introduction to the Study of Physical Phenomena, by Amédée Guillemin, translated from the French by Mrs. Norman Lockyer, and edited, with Additions and Notes, by J. Norman Lockyer, F.R.S. (illustrated by 11 coloured plates, and 450 woodcuts); Papers on Electrostatics and Magnetism, by Prof. Sir W. Thomson, F.R.S.; The Depths of the Sea: An Account of Investigations conducted on board H.M.'s Ships *Lightning* and *Porcupine* in the Years 1868-9, under the Scientific Direction of W. B. Carpenter, M.D., F.R.S., J. Gwyn Jeffreys, F.R.S., and Wyville Thomson, LL.D., F.R.S., edited by Dr. Wyville Thomson (with illustrations). By Messrs. L. Reeve and Co. :—Lahore to Yarkand, Incidents and Natural History of the Expedition of 1870, by Dr. Henderson; On Harvesting Ants and Trap-door Spiders, by J. T. Moggridge, F.L.S.; Vol. 2 of the English Edition of Prof. Baillon's Natural History of Plants; and the Fifth Part of Hanley and Theobald's Conchologia Indica. By Messrs. Blackwood and Sons :—A Manual of Palæontology for the use of Students, by H. Alleyne Nicholson, M.D., D. Sc. Professor of Natural History and Botany, University College, Toronto (with 400 engravings); Advanced Text-Book of Botany for the Use of Students, by Robert Brown, M.A., Lecturer on Botany under the Science and Art Department of the Committee of the Privy Council on Education. By Messrs. A. and C. Black :—School Manual of Zoology, by Andrew Wilson (with illustrations; New Edition of Jukes' Scotch Manual of Geology, edited by Alfred J. Browne; New Edition of Elements of Mineralogy, by James Nicol, Professor of Natural History in the University of Aberdeen. By Messrs. H. S. King and Co. :—The Forms of Water in Rain and Rivers, Ice and Glaciers, by John Tyndall, LL.D., F.R.S. (with 32 illustrations); Physics and Politics, by Walter Bagehot, being vols. 1 and 2 of the International Scientific Series. By Messrs. Lockwood and Co. :—A rudimentary Treatise on Coal and Coal mining, by Warington W. Smyth, M.A.; Weale's Dictionary of Terms, new and enlarged edition, edited by Robert Hunt, F.G.S.; Waterworks for the Supply of Cities and Towns, with a description of the Principal Geological Formations of England as influencing Supplies of Water, by Samuel Hughes, new edit.; Projection, Orthographic, Topographic, and Perspective, giving the various Modes of Delineating Solid Forms by Constructions on a Single Plane Surface, by J. F. Heather, M.A.; A First Book of Mining and Quarrying, with the Sciences connected therewith, for use in Primary Schools and Self-Instruction, by J. H. Collins, F.G.S.; Places and Facts in Physical and Political Geography, for the use of Candidates in Public and Private Examinations, by the Rev. Edgar H. Rand; a Course of Analytical Chemistry, specially prepared for Universities and Science and Art Departments, Advanced and Honours Examinations, by W. W. Pink. By Messrs. S. Low, Son and Co. :—The Arctic Regions, illustrated with Photographs, taken on an Art Expedition to Greenland, by Wm. Bradford; with descriptive Narrative by the Artist; in 1 vol. royal broadside, 25 inches by 20 inches, bound in morocco; The Atmosphere, by Camille Flammarion, translated under the superintendence and revision of James Glaisher (with numerous woodcut illustrations and 10 beautiful chromo-lithographs). By Mr. Maclehose :—A Class-Book of Qualitative Chemical Analysis, by John Ferguson, M.A. By Trübner and Co. :—Mythical Zoology, or the Legends of Animals, by Angelo de Gubernatis, Professor of Sanskrit and Comparative Literature in the Instituto di Studii Superiori e di Perfezionamento, at Florence (2 vols.); A Practical Treatise on Pure Fertilisers, and the Chemical Conversion of Rock Guano, Marlstones, Coprolites, and the Crude Phosphates of Lime and Alumina generally, into various valuable Products, by Campbell Morfit, M.D., F.G.S. (with 28 Illustrative Plates or Construction Plans, drawn to Scale Measurements); Human Physiology, the Basis of Sanitary and Social Science, by T. L. Nichols, M.D. (with illustrations).

PROF. GALLOWAY, of the Royal College of Science, Dublin, has two works nearly ready for the press :—"How the Natural Sciences are Taught, and how they ought to be Taught; with a Scheme for rendering more efficient the Government Science Schools;" and "A Manual of Applied Analysis.'

THE first meeting for the season of the Victoria Institute took place on Friday last, when a number of new members were elected, the Society being stated to be greatly on the increase. The first paper to be read during the coming session will be by Mr. Charles Brooke, F.R.S., on Force and Energy.

THE Geologists' Association will hold its first meeting for the season on Friday evening, Nov. 1, when Dr. Hyde Clarke will read a paper on the Influence of Geological Reasoning on other Branches of Knowledge.

AMONG the courses of University Lectures and other means of scientific instruction announced to be given at Harvard University, Cambridge, U.S.A., during the present session, are the following:—On General Entomology, by Prof. Hagen—Mondays, Wednesdays, and Fridays, at the Museum of Zoology. Geological Excursions, by Prof. Shaler, on Saturdays—about eighteen during the year. On the Structure and Affinities of the Brachiopoda, by Prof. E. S. Morse—Mondays and Wednesdays, in Roylston Hall. On General Ornithology, by Mr. J. A. Allen—Wednesdays, at the Museum of Zoology.

THE following is the programme of papers to be read at the Winter Session, 1872-73, of the Glasgow Society of Field Naturalists:—On the present tendencies of Science, by J. Allan, Oct. 15.—On the Distribution of Plants, by D. Gregorson, Oct. 29.—A Life History of *Nematus saliceti*, together with some account of its Parasites, by P. Cameron, jun., Nov. 12.—On Spiders, by S. M'Donald, Nov. 26.—On Zoophytes, by W. D. Benson, Dec. 10.—On the Exotic Plants of Clydesdale, by R. M'Kay, Dec. 24.—On the Definition of Species, by Alex. Watt, Jan. 7, 1873.—Notes of Observations on Marine Zoology, by John Harvie, Jan. 21.—Notes of Observations with the Microscope, by G. Barlas, Feb. 4.—Botanical Gleanings from the Rubbish Heaps of the City, by G. Horne, Feb. 18.—On the Cynipidæ of the Glasgow District, by P. Cameron, jun., March 4.

A SERIES of short lectures is about to be delivered at the Ipswich Museum by the Curator, Mr. J. E. Taylor, and other gentlemen, illustrative of the objects in the museum. They will be held on Friday evenings throughout the winter, and admission will be free.

THE first of Abbé Moigno's long-contemplated *Salles du Progrès* was inaugurated on Tuesday evening, Oct. 15, at 30, Rue du Faubourg Saint Honoré, by a distinguished assembly, including M. Otto Struve, the Russian Astronomer. The praiseworthy object of the Abbé in establishing these assemblies is to popularise Science by means of lectures, exhibitions, conversaziones, &c., in which the instructive is combined with the entertaining. On Tuesday he detailed the programme which he intended carrying out at future meetings, and those present were entertained by the performance of some pieces of music. This last rather novel feature in scientific assemblies forms a regular part of the Abbé's programme. One or more pieces from the works of great masters of music will be performed at each meeting.

THE British Association Meeting at Brighton has already begun to bear fruit in that town. A desire has been aroused among several of the inhabitants to know more of Natural Science, and a course of science lectures in the Dome, chiefly to working men, has been projected. But the ladies have taken the initiative, and the germ of a Ladies' Educational Association has already been planted. Prof. W. F. Barrett has been invited to give the first course of lectures on Experimental Physics. The introductory lecture on the "Study of Natural Knowledge," was given last Friday afternoon, when, in spite of the wet, upwards of 50 ladies assembled. Miss Goulty, of 2, Sussex Square, Brighton, to whom it is right to add the effort is mainly due, has permitted the use of her spacious schoolrooms for these lectures. The second lecture on "Magnetism" will be given to-morrow (Friday) afternoon.

SIEBOLD'S NEW RESEARCHES IN PARTHENOGENESIS *

II.

SIEBOLD'S experiments extended over four years, and although some hundreds of nests were more or less observed, only thirty-seven—but these amply sufficient—gave the answer to his questions, passing successfully through all the stages above noted. Firstly, they furnished a virgin colony in a nest absolutely free from eggs and larvæ—except a few advanced larvæ purposely left in some nests and noted down—which colony laid eggs; secondly, these eggs produced without exception (some few eggs not developing) males.

The method of recording which was used must be mentioned to give a notion of the accuracy of the observations. A series of plans of each nest was kept, each cell being represented and its contents at different dates. Successive plans were used for recording the successive changes in the number of cells of the nest, and in their contents at different periods of the observations. Signs jotted down in the plan cells indicate such facts as these—*e.g.*, the cell contains a "parthenogenetic egg," or "a second parthenogenetic egg which was laid after a first one had disappeared," or "a larva sprung from the queen," or "a parthenogenetic male larva," &c., &c. A second record was kept, and is given for twenty-two cases, in which the following facts were noted:—Number of the nest, date it was made moveable, number of cells at that time, day of emergence of first worker-female, date of destruction of queen, eggs, and grubs, number of larvæ and pupæ left undestroyed at this date, date of first laying of parthenogenetic eggs, date of first emergence of parthenogenetic larvæ, date of first emergence of drones born from queens' eggs (these were null in most cases, and were always so late as not to affect the experiments by possibly impregnating the worker-female), number of the same, number of cells observed when the experimental conditions were established, date and duration of the experiment, maximum number of female workers employed in the affairs of the nest, number of larvæ, pupæ, and wasps of the parthenogenetic brood found at the conclusion of the experiment. After the account of the artificially obtained results, two cases are recorded in which Siebold found a parthenogenetic colony naturally established by the same accident which had destroyed their queen and comb.

Before concluding this chapter of his book, Siebold makes the very important observation that the facts observed in the parthenogenesis of Polistes are in opposition to the view maintained by Leydig, viz., that the sexual differentiation of the egg is independent of its fertilisation, and that the evolution of the male sex is due to diminution of nutrition and warmth. Bessels has already, in opposition to Landois, shown that this is not the case in the bee. If it were true for Polistes, the eggs laid in the early year, when it is cold, and when there is only the queen to attend to the larvæ, should produce drones. On the contrary, they produce females, and the drones appear precisely at the time when warmth and nourishment are most abundant.

Siebold concludes, therefore, that (1) the eggs bring with them from the ovary the capacity of differentiating themselves as males, and (2) of developing themselves, independently of male influence into male individuals; (3) but the same eggs can be changed in these properties by the influence of the male sperm elements, and proceed to develop as female individuals.

The second chapter, very short, is on Parthenogenesis in *Vespa holsatica*, which was inferred to occur from the observation of a naturally-produced queenless colony, the larvæ in the cells of which were all male.

The third chapter is on Parthenogenesis in *Nematus ventricosus*, the larva of which is known as the Gooseberry-caterpillar. Since three or more generations of these leaf-wasps occur in the season, they furnished abundant material, and the old supposition of parthenogenesis first put out as regards them by Robert Thorn, in the *Gardener's Magazine*, 1820, is shown by Siebold to be justified by carefully conditioned experiment. Some valuable observations on the anatomy of the generative organs, and on the curious increase in the size of the egg after it is laid, are given. The parthenogenetically produced progeny are in this case also male. The results of the *Nematus* experiments were not ready for publication until after the issue of the present work, and we have received, through the kindness of Dr. Dohrn, a copy of the Sitzungsberichte of the Munich Academy of November 4, 1871, in which they are fully given. It appears that though an occasional female appeared among the male broods produced by unfertilised females, this was, in every case where it happened, fully accounted for by the accidental access of a fertilised female, or some such misadventure, duly noted in the records kept of the observations.

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* "Beiträge zur Parthenogenesis der Arthropoden." Von C. Th. E. von Siebold, Professor der Zoologie und Vergleichenden Anatomie in München. (Leipzig: Engelmann, 1871.)

Of the fourth and fifth chapters, treating of Parthenogenesis in the Lepidoptera, *Psyche Helix*, *Solenobia triquetrella* and *lichenella*, we have not space to speak in detail. The same intimate inquiry, and the same very necessary prodigality in the amount of material subjected to experiment, which we noted above as to Polistes, characterise Prof. Siebold's treatment of these cases. The parthenogenesis in these cases produces female broods, and though the male of *Psyche Helix* has been discovered since Siebold's former researches on this moth, his conclusion is by no means invalidated, for the males are excessively rare. They were first discovered by Claus, of Marburg, who has indicated characters by which future observers may distinguish the sex of the caterpillars. Out of many hundreds of broods reared by Siebold, taken in various places, ranging from the Baltic to the plains of Lombardy, only once did he obtain males. There appear to be thus broods which are entirely female, and broods which are of mixed sexes. The conditions under which the male sex makes its appearance are not yet ascertained. It is exceedingly desirable that those who may be fortunate enough to come across a mixed brood, should make experiments to ascertain if all the eggs which are fertilised produce males. The females of the purely female broods are completely developed in every respect, having perfect copulatory organs, and the egg is furnished with a micropyle; therefore, as Siebold maintains, they must not be called pseud-ova. It should be mentioned that the inquiries necessary to establish the identity of the species, and the distinctive characters with regard to these little moths, have occupied a great deal of our author's time and attention, and are here recorded. In regard both to Psyche and Solenobia examination with the microscope was employed to determine the absence of male elements from the *receptaculum seminis;* and we have moreover an account of the structure of the ovaries. In relation to this matter, Professor Siebold takes the opportunity of replying to some criticisms of his former work by M. Plateau, who appears to have made little of the arguments based on the proof thus obtained of virginity, without knowing the real extent and nature of Siebold's studies, having, in fact, only read of them in an imperfect abstract. It appears also that M. Plateau took "ein einziger Fundort" to mean "un naturaliste collecteur," an amusing mistake to which our attention is drawn in a note, p. 155. We may briefly mention here with regard to Solenobia, that it appears that *S. lichenella* is only the female brood of *S. pineti*, of which males and females regularly occur. No structural difference appears to exist between the two kinds of females, but the former, on escaping from the chrysalis-sac, at once proceed to lay eggs, which produce invariably females; whilst the latter wait for copulation, and if that be withheld, die, and dry up without laying their eggs. These insects offer most promising material for further researches on the conditions attending the differentiation of sex.

We now come to the sixth and last chapter, on "the Parthenogenetic Reproduction in Apus and allied Crustacea." Already, in 1856, Siebold had stated his supposition that *Apus cancriformis*, *Limnadia gigas*, and *Polyphemus oculus*, in which species no males had been observed, presented examples of true parthenogenesis, and were not to be regarded as bud-producing "nurses," in a so-called alternation of generations. Leuckart subsequently expressed the same opinion with regard to the reproduction, independent of males, observed in *Daphnia*, *Apus*, and *Limnadia*. Ever since that period Siebold has continually kept an eye upon Apus. In 1858 the males of Apus were discovered by Kozubowski, and Siebold received specimens from various localities. He thus learned to distinguish with perfect facility the two sexes, and was enabled now to convince himself that, as with the Lepidoptera above spoken of, so with Apus, broods occur which are entirely destitute of males, and go on reproducing parthenogenetically, whilst other broods occur in which both sexes are present. The number of Apus of two species—*Apus cancriformis* and *Apus productus*—examined by Siebold, amounts actually to some thousands. He received quantities taken from various ponds in middle Europe (Apus occurs in shallow pools which dry up during parts of the year, and it can be taken in immense quantity), and had the opportunity of studying one pond—that at Gossberg, near Munich, with minuteness, from the year 1864 to the year 1869 inclusive, besides casual examinations of the same pond in 1857 and 1858. Time after time, taking several hundreds of the Apus from the pond, he never found a single male amongst them. On one occasion he had the whole contents of the little pond removed with the greatest care, so as to feel sure that he had obtained every Apus present. He received on this occasion 5,796 specimens of Apus, *every one of which being carefully examined* proved to be female. At the same time 2,576 specimens of Branchipus were obtained from the pond, which were, as usual, of both sexes. In those cases where ponds afforded both males and females of Apus, it is remarkable that the proportion of the sexes was very variable. The highest proportion of males appears to be in a case recorded by Sir John Lubbock, who found thirty-three male and thirty-nine female *Apus productus* in a pond near Rouen, whilst among 193 specimens of *Apus cancriformis*, from a locality near Krakow, only one male occurred. What is most important about this variation in the proportion of males to females is that in two or three localities, furnishing mixed generations of Apus, from which he has received, year after year, numbers of specimens, Siebold has observed an apparent constantly-augmenting disproportion of males to females, and he is led to the supposition that in these cases the males will at last cease altogether, and thus a female generation be produced which will continue to reproduce itself parthenogenetically, as in the Gossberg and a great number of other ponds. This is, however, by no means proved; and we have no idea at present as to how the males may make their appearance again, or what are the conditions affecting their development and extinction. It occurred to Siebold that an objection might be urged against parthenogenesis in Apus, in that, although he examined consecutive generations and found them always female, he could not be sure that males had not been present before he took his specimens, and had not died and decomposed after having fertilised the females. To meet such an objection, he firstly made himself thoroughly acquainted with the male generative organs and the spermatozoa, and secondly with the ovaries and their development. He found the spermatozoa to be motionless like those of other Crustacea, and he never succeeded in detecting any of them in the female genitalia amongst the specimens belonging to supposed female generations. But he equally failed to find spermatozoa or a receptacle for them in the female genitalia of the specimens of mixed generations, and therefore no conclusion could be drawn from the observation. The structure and development of the ovum, however, made this observation decisive, since it was found that an egg-shell forms round the ovum in the uterus, and, in the absence of a micropyle, fertilisation, if it takes place at all, must be accomplished before this shell is hardened. A further proof of another kind was obtained by experiment. Having removed eggs from females, which certainly at the time contained no spermatozoa, Siebold placed them in a small tank, and from these obtained Apus-embryos. Others were reared to maturity from eggs taken in the pond.

The relative size of male and female is a question about which there is some interest; differences which have been observed seem to depend on this, that Apus continues growing as long as the pond in which it lives does not dry up, and hence the eggs which hatch soonest give the largest-sized progeny. In his tabular statements Siebold gives measurements of the specimens examined by him at different times from various localities.

A few words must be said here upon the very extraordinary history of the ovum of Apus made out by Siebold, the structures being identical, whether the female examined belonged to a parthenogenetic or digenetic brood. The essential female organs of reproduction in Apus may be roughly described as two large tubes placed on either side the alimentary canal, opening externally at the posterior end, and giving off towards the other end primary and secondary branches. On the ends of these short secondary branches are situated the egg follicles. Four cells appear in each egg follicle in a very early stage of its development, and one of these takes on more rapid growth—becoming the egg-cell—whilst the others disappear as deutoplasmogen or vitellogenous cells; the egg then acquires some size and a red colour, and has a visible germinal vesicle. But such eggs are much smaller than the eggs observable in the main stem of the ovarian tube, and this appears to be the very startling explanation. The eggs escape from their follicles as a matter of course, and pass along the canal leading from it to a primary branch of the ovarian tube, and there two and sometimes three of these eggs *fuse into one mass*, around which a shell is secreted, and which thus forms the actual egg—really a threefold egg; and from such a wonderfully formed egg only one embryo develops. Unfortunately we are not told what becomes of the germinal vesicles; according to the drawings they seem to disappear at this stage. We know of the development in the tunicate *Pyrosoma* of five embryos from one egg, here we have the converse case of one embryo developing from three eggs. Siebold appears to have convinced himself that the fusion is a normal thing, and not due to any pressure or osmotic action taking place during the microscopical examination. The structure of the ovary of Apus is figured in a plate.

As to the other crustaceans named, which are *Artemia salina* and *Limnadia Hermanni*, the occurrence of parthenogenetic broods is inferred from the descriptions of other writers whose works are criticised at some length, and also from examination of specimens. It seems not impossible from an observation of Zenker that in *Artemia salina* parthenogenetic alternate with digenetic broods. In the beginning of the year 1851 this observer found three males among one hundred females, later in July the same pond furnished thousands of females, but not one male.

In conclusion, Prof. Siebold, whilst adopting Leuckart's term "Arrenotoky," to designate the phenomenon of the parthenogenetic production of male offspring, as seen in the Hymenoptera, proposes the parallel term, "Thelytoky," for the pathenogenetic production of female offspring as demonstrated now conclusively in some Lepidoptera and Crustacea. It seems to us that a third term should also be available for the case of mixed offspring (that is of two sexes) such as "Amphotoky;" and the terms need not be limited to parthenogenetic cases. In his concluding remarks, whilst repeating the expression of his conviction that parthenogenesis will be found more and more to be of frequent and fixed occurrence in various classes of animals, Siebold alludes with caution to the list of cases in which parthenogenesis is stated to occur, given by Gerstaecker in Bronn's "Classen und Ordnungen des Thierreichs." Gerstaecker rightly enough distinguishes cases in which parthenogenesis has been observed as an accidental and rare exception, and those in which it has a definitely recurring place. Siebold considers (and after the great pains he has himself expended on the cases recorded in this book, he is fully warranted in so doing) that many of the examples put forward by Gerstaecker require a more careful testing, and he offers some remarks on parthenogenesis in the gall-flies, and in the silkworm moth. Finally, he alludes to cases among Vertebrates in which indications of a power of development in the egg, independent of the male element, have been observed. The most remarkable of these is that quoted by Leuckart in his work already cited, which Siebold omits here, but has done justice to in the short supplementary paper read at the Munich Academy since the publication of this book. In 1844 Prof. Bischoff found ova in the uterus of an unimpregnated sow, which exhibited segmentation of the yelk, some into two and four, and others into sixteen and twenty divisions. Other cases here given are as follows:—In the oviduct of a three-year-old rabbit, thoroughly separated pathologically from the uterus, Prof. v. Hensen of Kiel found ova in various stages of yelk-division, and some of their cells had even advanced into a branched condition. Dr. Oellacher of Innsbruck has observed stages of yelk-division in unfertilised hen's eggs. In fishes, in 1859, Agassiz observed yelk-division occurring in the eggs of Gadidæ, whilst yet in the ovary, and considered it to be due to impregnation, even stating that he had seen certain fishes place themselves in such a position as to favour this supposed intra-ovarian fertilisation. Burnett has since investigated the case, and concludes that the yelk-division is independent of fertilisation, a supposition which is rendered in every way probable from other researches on the fish egg; but, curiously enough, Dr. Burnett thinks these eggs should be regarded as "germs," and not as "true eggs," an opinion to which Siebold, of course, is completely opposed, and which, in invertebrate cases, has been shown to be untenable.

Siebold does not allude to those cases of ovarian cysts found occasionally in the unfertilised human female, and containing hair and teeth—a phenomenon which we should be glad to see further discussed and investigated, since, as far as we can remember, the origin of the contents of such cysts from irregularly developing ova is probable. The eel is suggested as a possible parthenogenetic vertebrate. It is a very strange fact that we are still ignorant of the ripe eggs and embryos as well as of the males of the eel, even as in the time of Aristotle. With the following words of that greatest naturalist, addressing them to those who still refuse to accept the existence of Parthenogenesis, Siebold ends his book:—"More belief must be given to observation than to theory, and this last is only worthy of belief when leading to the same result as experience."

E. RAY LANKESTER

ON SOME NEW POINTS IN THE MOUNTING OF ASTRONOMICAL TELESCOPES*

THE very great inconvenience attendant upon the use of the ordinary position circle of a micrometer divided on a metallic limb, and the necessity of having small lamps hung on to the micrometer for producing that very useful character of illumination of the wires known as the "dark field," has induced me to introduce some modifications in this (to the observer at least) very important part of an equatorial instrument.

These modifications have already been applied with success, and for the first time (as far as I am aware) to a 7-inch refracting telescope now in course of erection at the Observatory of the Royal Artillery Institute, Woolwich; and I have (in consequence of this success) been ordered to adapt them to the Great Equatorials now in course of construction for the Royal Observatory, Edinburgh, and the Observatory of the Lord Lindsay, Aberdeen.†

The rack and pinion tube carrying the eye piece or micrometer revolves freely in the casting which forms the lower end of the telescope tube, and carries a brass plate (all cast in one piece), on which is cemented a flat ring of plate glass, muffed on back, and in front varnished with an opaque varnish. Through this varnish the divisions are cut, so that on being illuminated from behind, the divisions appear bright upon a black ground. The vernier is similarly treated, and the whole of this circle, being covered with a cap, with a glazed window only sufficiently large to expose the vernier and about 15° of the circle, is protected from possible injury and is read most conveniently through this window, being illuminated by a beam of light constantly directed upon it from a lamp hanging on end of the declination axis, as will be afterwards explained.

Between the fixed casting which forms the end of the telescope tube and that which revolves in it is another metallic circle cut into 360 teeth on edge, and with 90 holes drilled accurately on face: into the teeth on edge is geared a screw which is mounted on fixed casting, one revolution of which is of course equal to an angular movement of 1°.

In the other (outer) moveable brass circle is mounted a steel pin working up and down in a small cylinder; this pin, being pressed down by a small spiral spring, enters into one or other of the 90 holes in the intermediate circle, and thus clamps the whole eye-end to the intermediate circle, in which condition a slow motion is obtained by the endless screw. When it is desired to move the eye-end through a large angle, the rack and pinion tube is grasped by the hand, and in doing so the hand almost necessarily grasps also a small steel trigger which lifts the steel pin out of the hole, frees the moveable circle, and allows it to be placed in any angular position. When the desired position is approximated, and the trigger relieved, the pin drops into the nearest hole, and the endless screw is then used for final setting.

The diagram will I think explain the various matters of illumination.‡

From a lamp hanging upon the end of the declination axis is sent a beam of slightly divergent light through this axis, which is hollow; this slightly divergent beam is utilised for six different purposes, three portions of it being reflected out in different directions to illuminate portions of the declination circle, of which one is for a long reader for setting from eye-end, and the other two for micrometer microscopes subdividing the $10'$ division of circle into single $1''$ arc.

None of these are shown in diagram, but the other three purposes for which the light is utilised, viz., for position circle, bright field illumination, and dark field illumination of micrometer, are shown.

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* Paper read before the British Association at Brighton in Section A, Aug. 20, by Howard Grubb, C.E., F.R.A.S.

† The breech-piece and position circle of the Woolwich Equatorial were here produced.

‡ The original diagram showed all three illuminations, and of different colours. Here it has been thought better to show the dark field by itself, and the bright field and position circle illuminations in a separate diagram.

The position circle illumination is very simple (see Fig. 1), a single reflector R, attached to the inside of the tube directs a constant beam of light on the back of the glass circle at P.

The bright field illumination is effected by a very small central reflector, R′, which sends the light directly into the field of the micrometer.

This method is, I believe, now generally considered to give the best results, and has, as far as I am aware, but one disadvantage, viz., that the arm which supports the small mirror produces a little diffraction, and consequently deterioration of definition.

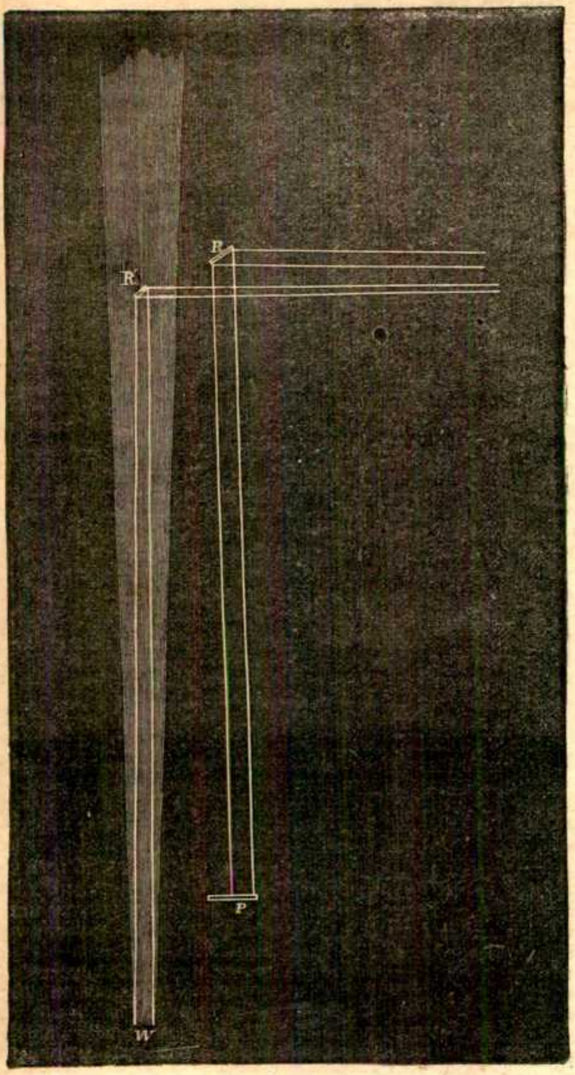

FIG. 1

This objection I have in some measure reduced by making the arm and mirror removeable at pleasure by pulling or releasing a string, so that while actually observing, it can be removed and replaced instantaneously.

In devising the dark field illumination, I started on the hypothesis that there were two essential points to keep in view, viz., that the lines should be illuminated on both sides (not one), and that the angle at which the light should be thrown upon the wires should be very great, so that the blackness of the field as seen through the eye-piece should not be injured.

I found that the best result is obtained by placing four prisms of total reflection round the field of the micrometer, just behind the wires, and of such an angle that the light thrown upon them should be reflected upon the wires at an angle such as is shown in the diagram Fig. 2, where W is the position of the wires in the focus of the objective.

In order that this scheme of illumination should be carried out effectually from the light of a single lamp hanging on the declination axis, it is necessary that a certain annular portion of the micrometer which embraces these prisms should be constantly illuminated from this lamp, and this is effected in the following way: a portion of the slightly divergent beam of light, shown in Fig. 2, proceeding from the lamp on the declination axis, is passed through a very low power convex lens, *l*, which renders the beam slightly convergent.

This is not necessary, but a mere matter of convenience, as it reduces the necessary size of the reflector and lens afterwards required.

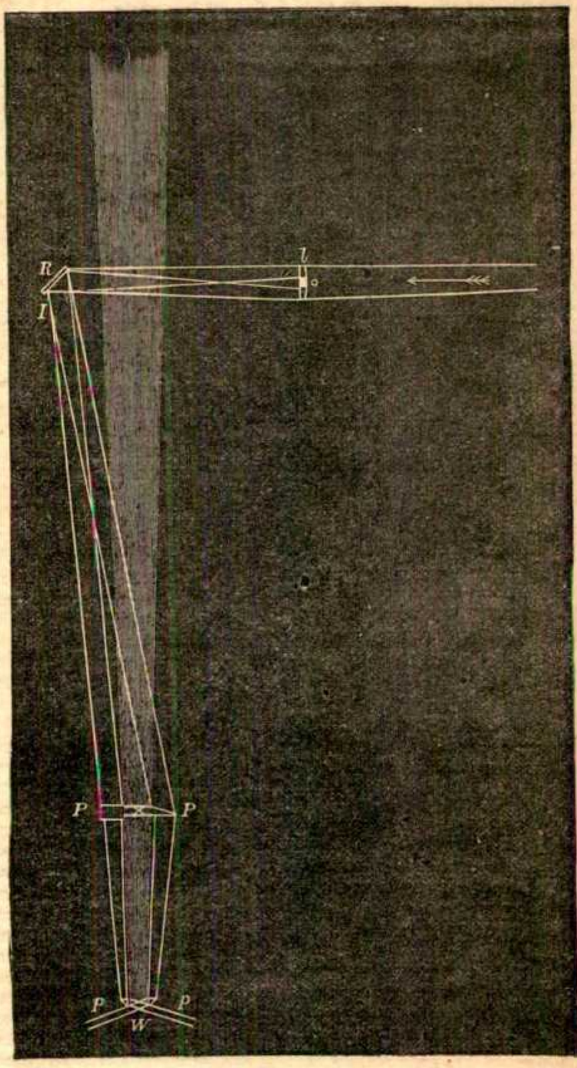

FIG. 2

The light is now taken up by a reflector, R, within the tube, and directed towards the eye-end at such an angle that it crosses the axis of the telescope just at the inner end of the eye-piece tubes, X; hence it is passed through a piece of glass of a peculiar shape, P P, which I call, for want of a better name, an annular prism lens. This piece of glass has a hole cut in it large enough to admit the whole pencil of light from the object glass.

The use of this annular prism lens is twofold:—

1st. It has to alter the direction of the beam of light before diagonally thrown across the tube, RX, to that parallel to the axis of the telescope; and

2nd. It is necessary that it should have a slightly converging effect to reduce the size of the illuminated circle it produces.

This arrangement so far performs perfectly in all but one particular. It throws a strong beam of light constantly upon the four prisms *p p*, and illuminates the lines well; but although no direct light can enter into the field from the mirror placed so far out of the cone of rays from the objective, still the light thrown against the side of the eye-piece tube is sufficient to completely destroy the effect of this illumination. The difficulty, however, has been completely removed in this way :—

I should first mention that the eye-piece or micrometer tube is made double, an outer parallel tube and an inner taper one, and it is between these two that it is required that the light should be brought to the four prisms or micrometers, any light shining into the inner tube doing mischief by injuring the blackness of the field.

On the lens used to give a slight convergence to the light is placed a circular opaque disc, O, of a certain size easily ascertained. A lens, I, of a suitable focus being then placed near the reflector, an image is formed of that opaque disc just over the eye-piece tube at X, and of such a size, when properly adjusted, that no light can possibly enter the inner tube.

Thus, while not a single ray of light can by any possibility enter the inner tube, a flood of light is sent down between the inner and outer tubes, and directed upon the four prisms in whatever angular position they may be.

It only remains to say that both the intensity and colour of the light for both characters of the illumination are under complete control of the observer while actually observing.

One other matter is perhaps worthy of note.

The want of a convenient method of mapping nebulæ or faint stars by a reticulated diaphragm of bright lines in the field of view has long been felt, and the various methods of using diamond scratches on glass or illuminated lines are subject to objection, and troublesome to manage. A simple method of using an image of such a diaphragm instead of the actual diaphragm itself here suggests itself.

Referring to the portion of the rays used for bright field illumination, and shown in Fig. 1, suppose the small diagonal mirror, R′, to be replaced by an equally small prism having such a convex power that it forms an image of any object at the end of the declination axis exactly in the same plane as the image formed by the objective—then any kind of reticulated diaphragm of bright lines on dark ground can be placed on the end of the declination axis which would have a suitably prepared carrier for them, and their image would be seen in the field of the telescope of any colour and any intensity desired.

SCIENTIFIC SERIALS

THE *Scottish Naturalist* for July is rich in articles of interest, mostly brief, and chiefly relating to Entomology and Ornithology. Many deserve notice, but we have been especially interested in one on the nest of *Formica rufa* and its inhabitants by the editor, Dr. Buchanan White.

IN the *Journal of Botany* for August, Dr. Trimen describes and draws the genuine *Ranunculus chærophyllos* Linn. which has been detected in Jersey, but was not heretofore known as a native of Britain. Dr. Hance describes a new species of Iris, *I. tomiolopha*. The Rev. J. M. Crombie contributes some notes on the Lichens in Sowerby's Herbarium.—In the September number, Dr. Hance describes another new species belonging to the Bignoniaceæ, *Spathodea cauda-felina*. Mr. T. R. A. Briggs contributes Notes respecting some Plymouth plants, and Dr. A. Ernst Notes on a small collection of Alpine plants from the summit of Naiguta in the mountains of Caracas.—The first article in the October number is Mr. Hayne's paper, read at the Brighton meeting of the British Association on the Flora of Moab. Mr. J. G. Baker, who has paid great attention to the Liliaceæ, has a monograph of the two genera *Dasylirion* and *Beaucarnea*. Another British Association paper, Mr. Hemsley's Summary Analysis of the Phanerogamic and Fern Flora of Sussex, is reprinted. The Rev. E. O'Meara contributes a continuation of his recent researches in the Diatomaceæ; and the Rev. J. M. Crombie, a description of a new erratic British *Parmelia*. Mr. Leo Grindon forwards a suggestive paper on the non-occurrence near Manchester of certain common British plants.

THE last part of the *Proceedings of the Swedish Academy of Sciences* for 1871 (Öfversigt af Kongl. Veterskaps-Akademiens Förhandlingar, Arg. 28, No. 7), opens with a notice by Prof. Lilljeborg of the occurrence of a South European species of Bleak (*Leucaspius delineatus*, Heckel) at Landskrona in Scania.—The same author has also a notice of the occurrence of *Limnadia gigas* (Hermann) in Sweden, which will prove of considerable interest to the student of Crustacea, as in it he gives a very detailed description, illustrated with good figures, of the structure of this curious species, and also gives a list of the other species of Phyllopoda, six in number, which inhabit Scandinavia. Prof. Lilljeborg is inclined to identify this species with the *Monoculus lenticularis* of Linnæus.—Prof. Nordenskiöld publishes a short paper, containing a table, on the fixed and variable atomic volumes of simple bodies.—The Swedish expedition to Greenland of the year 1870 originates two papers, namely, a valuable essay on the Phanerogamic flora of Disco Bay and Auleitsivik Fjord by Prof. S. Berggren; and a series of calculations of geographical positions worked out by M. E. Jäderin.—M. L. K. Daa discusses the origin and meaning of the name of Grumant applied by the Russians to Spitzbergen, and cited as an evidence of the independent discovery of that inhospitable land by the Russians; M. Daa states that Spitzbergen was named East Greenland by its earliest English and Dutch visitants, and he maintains that "Grumant" is merely a corruption of "Grönland."—Mr. H. D. J. Wallengren publishes a Contribution to the knowledge of the Lepidopterous fauna of the island of St. Bartholomew in the West Indies. He gives a list of 34 species belonging to various families from the Rhopalocera to the Crambidæ, with remarks on their characters and distribution. Three species are described as new, namely, *Graphiphora bartholemica*, *Micra Stålii*, and *Palthis Walkeri*.—M. L. J. Igelström notices the discovery of sandstone *in situ* in the Gefleborg district.

SOCIETIES AND ACADEMIES

PHILADELPHIA

Academy of Natural Sciences, April 9.—Prof. Leidy directed attention to some fossils upon which he made the following observations :—Several teeth and jaw fragments from the Loup Fork of the Niobrara River, Nebraska, obtained by Prof. Hayden, appear to indicate a large species of Felis, not previously described. The most characteristic specimen consists of an upper sectorial molar about as large as that of the Bengal tiger, and consequently much too large for either of the largest American cats, the panther and the jaguar. It is as much too small to have pertained to the American lion, *Felis atrox*, for its breadth is but slightly greater than that contained in the lower jaw, from which the latter was described. Breadth of the crown of the tooth is 15½ lines; its thickness in front 8 lines. The measurements in the corresponding teeth of a Bengal tiger are, 16 lines in breadth, and 7½ lines in thickness in front. The form of the fossil tooth is the same as in the other feline species. The extinct species may be named *Felis augustus*. A distal extremity of a humerus, from the Niobrara River, about the size and construction of the corresponding part in the Bengal tiger, may belong to this species. Another fossil, consisting of a detached body of a vertebra, apparently indicates an extinct reptile allied to *Plesiosaurus* and *Discosaurus*. The specimen, recently received from Prof. Hayden, was obtained in 1870, on Henry's Fork of Green River, Wyoming. It is free from attached matrix, and was the only specimen pertaining to the animal which was found. It probably belonged to a formation of earlier date than that of the same locality, which has yielded other fossils previously described. The vertebra is from the base of the tail, and is much shorter in relation to its other dimensions than in *Plesiosaurus* or *Discosaurus*. The extremities are concave, and encircled near the margin of the articular surfaces with a narrow groove. Posteriorly there are two larger articular facets, as widely separated as the bone would permit, for the junction of a chevron. Anteriorly there are no marks of chevron attachment. The roots of strong transverse processes or diapophyses project from the sides of the body just above the middle. The neural arch was completely co-ossified with the body, leaving no trace of its earlier separation. The breadth of the body is 23 lines, its depth 19 lines, and its length 1 inch. Viewing the specimen as probably representing a genus different from those mentioned, I propose to name it with the species as *Oligosimus grandævus*. Another fossil is a remarkable specimen, obtained by Prof. Hayden in the "Black Foot Country" at the head of the Missouri River. It looks as if it had formed part of the dermal armour of some huge saurian or perhaps of an armadillo-like animal. It is imperfect, and looks as if it were half broken away. In its present state it is hemiovoid, about two inches in diameter, concave below and convex above, where it is covered by about fifteen large mammillary bosses. Accompanying this specimen there is a distal phalanx, which may belong to the same animal. It is rather less than two inches long. The articular surface is transversely elliptical, 1¼ inch wide, and 11 lines deep, and feebly depressed, so as to indicate a moderate degree of mobility. The upper surface of the bone slopes to the end, and is transversely convex. The extremity is expanded at the borders. Beneath are several vascular perforations. Though the specimens are not sufficiently characteristic to determine positively whether they belong to a mammal or a reptile, or whether they even belong together to the same animal, the former one is so peculiar that I am disposed to regard it as representing a genus and species, which may be named *Tylosteus ornatus*.—Mr. Cope made the following remarks on a curious habit of a snake:—"I had for some time a specimen of *Cyclophis æstivus*, received from Fort Macon, N. Ca., through the kindness of Dr. Yarrow, living in a wardian case. The slender form of this snake, and its beautiful green and yellow colours, have led to the opinion that it is of arboreal or bush-loving habits. It never exhibited such in confinement, and instead of climbing over the caladia, ferns, &c., lived mostly underground. It had a curious habit of projecting its head and two or three inches of its body above the ground, and holding them for hours rigidly in a fixed attitude." In this position it resembled very closely a sprout or shoot of some green succulent plant, and might readily be mistaken for such by small animals.

PARIS

Academy of Sciences, October 7.—M. Faye, President. M. A. Trécul read a paper entitled "Observations on the various parts of the Flower of *Campanulaceæ*," and his long paper was followed by an account of some "new experiments intended to show that the germs of the ferment which produces wine come from the exterior of the skin of the grape," by M. Pasteur. The author prepared forty flasks with long necks, which were twisted and bent in the now so well known fashion first used by this chemist. Ten flasks were partly filled with grape-juice, and allowed to rest; ten others, also containing juice, had introduced into them a few drops of water, in which a small piece of grape-skin had been washed; the next ten had juice and water from the skin like the last, but were boiled; and the last ten contained juice and a few drops of the interior of a grape carefully extracted by means of a glass tube, without bruising the skin. The series containing the unboiled juice and grape-skin washings were soon full of mycelium and beer-yeast, and a few days after of *Mycoderma vini*, within forty-eight hours of the appearance of which they were in a state of violent fermentation. None other of the flasks were changed in the slightest degree, even after days; and the author states that they will remain unchanged for years. M. Fremy replied to this in a note on ferments, in which he states that M. Pasteur confounds ferments with the spores of mould. M. Fremy believes the ferment to be generated in the fermentable liquor, and that fermentation can also be started by mould spores by a secondary action, hence he considers that M. Pasteur has only proved that this latter kind of fermentation is produced by the grape-skin. M. Pasteur replied that he only intended to prove that the juice of the grape is not of itself alone capable of fermenting, and that neither the albuminous matters of the juice nor the parenchyma cells are developed into ferment cells by the action of atmospheric oxygen alone. At the request of M. Dumas, M. Pasteur then read an account of some "new facts serving to elucidate the theory of true fermentation." M. Fremy again criticised the paper, and after a reply from M. Pasteur, the subject dropped. M. A. Trécul then read a note confirming several of M. Pasteur's observations, and was followed by M. Faye with a note on a memoir of Mr. Clerk-Maxwell, "On the stability of the Saturnian Rings."—A note from M. Otto Struve, "On the exactness which should be attributed to the constant Coefficient of Aberration determined at the Pulkowa Observatory," was then read, and next came "Researches on Crystalline Dissociation" (continuation), by MM. P. A. Favre and C. A. Valson. This paper, containing a great number of numerical results, was followed by "Studies on the Echinodermata," by M. S. Lovén, and by a paper on the structure of heterogeneous vegetables, by M. Th. Lestiboudois.—M. de Caligny then read a note on the effects of the communication of a lateral movement to a stream of water traversing a reservoir, and on the sand-banks which thence result. This was an account of some experiments made by the author. He finds that banks are deposited almost parallel to the stream.—M. Chevreul then read a note relating to a work on colour, by M. P. Havrez, which he presented to the Academy; and M. Dumas presented a pamphlet by M. de Jacobi, entitled "On the galvanic deposition of iron by a powerful electro-magnetic solenoid." The author hoped by these means to deposit permanently magnetic iron, but failed; the deposit, however, was composed of agglomerations of crystals, whilst iron deposited in the ordinary way is smooth and amorphous.—M. J. M. Gaugain then presented his second memoir on the induction currents produced in M. Gramme's machine, which was referred to the Physical Section.—A note on the efficacy of lightning conductors, by M. W. de Fonvielle, was sent to the commission on that subject. A note from M. Laborde, on auroræ, storms, and waterspouts, was sent to the Physical Section, and the Aerostatic Commission received memoirs from M. Reynal and M. Babé and a letter from M. Braconnier, all on aërial navigation.—A note from Raoult de Couesquelon on a "New System of Masked Batteries" was sent to the Commission on Military Art, and two notes from M. Duclaux, two from M. Cornu, and an article from the Journal *La Gironde* by M. Laliman, all on *Phylloxera*, were sent to that Commission.—M. de Saint-Venant then presented a note from M. J. Boussinesq on "Lines of Summit (*faîte*) and Thalweg," which was followed by a note from M. Béchamp "On the action of borax on fermentation." The author demonstrates that the boric acid of the borax is not the cause of the peculiar action of this body, as that acid does not produce the effects of borax. Hydric sodic carbonate, however, acts in a strictly analogous way; hence the author decides that it is the sodium present in the borax which determines its action. —A note from M. E. Monier "On the determination of the amount of vegetable matters in contaminated potable waters" then followed. The author uses a method now abandoned by all the best analysts of water in this country, namely, titration with potassic permanganate.—M. E. Gouriet then read a paper "On certain exterior characters which distinguish the different sexes of the River Craw-fish (*Astacus fluviatilis*)." The author finds the following differences:—If the length of the animal be taken as 100, then the antennæ in the male are 67·83, and in the female 57·18. The weight of the animal being 100, the great claws are in the male 27·81, in the female 12·92; moreover, the female abdomen is much more developed than that of the male —A note from M. Brown "On the relations between electricity and mephitic emanations" closed the session.

BOOKS RECEIVED.

ENGLISH.—The Clematis as a Garden Flower: T. Moore and G. Jackman (Murray).—The Travelling Birds: Cuthbert Collingwood (C. Bean).—Synopses: Physical Geography, Geology, Mineralogy, and Palæontology, D. Page (Blackwoods).—Revised List of Vertebrate Animals in the Gardens of the Zoological Society.

FOREIGN.—Grundriss der Chemie gemäss der neueren Ansichten: der unorganischen Chemie dritte Auflage: C. Rammelsberg.—Incendio Vesuviano del 26 Aprilo, 1872: L. Palmieri.—Der Ausbruch des Vesuv vom April 26, 1872: L. Palmieri.—Vereins für Erdkunde zu Dresden, Nos. 8 and 9.

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THURSDAY, OCTOBER 31, 1872

THE GREAT CIRCUMNAVIGATING EXPLORING EXPEDITION

PREPARATIONS for the expedition which is about to be despatched by the Admiralty for the purpose of dredging, sounding, and otherwise scientifically investigating, the deep sea, have been for some months past in active progress, and are now rapidly approaching completion. The vessel set apart for the purpose is H.M.S. *Challenger*, a main-deck corvette of 2,306 tons, now lying at Sheerness. Her commander is Capt. G. S. Nares, R.N., well known as the author of a valuable work on seamanship, greatly in use in the Royal Navy. Capt. Nares has seen a great deal of active service, including exploration in the Arctic regions, and he has left the charge of the Survey in the Gulf of Suez for the purpose of taking charge of the present expedition. Second in command is Commander J. P. Maclear, R.N., son of Sir Thomas Maclear, late Astronomer Royal at the Cape of Good Hope, who has also seen a great deal of service in various parts of the world, and whose name is familiar to our readers from his having taken part in the Eclipse Expedition to Spain, and also in that to Ceylon. Commander Maclear will undertake the magnetic observations, which will form part of the work of the Expedition. The other naval officers are—1st Lieut., Pelham Aldrich; 2nd Lieut., Arthur C. Bromley; 3rd Lieut., George R. Bethell; Navigating Lieut., Alfred E. Tizard; Sub-Lieutenants, H. C. Sloggett and Lord George Campbell; Pay-master, R. R. Richards; Chief Engineer, James H. Ferguson.

On the scientific staff of the Expedition, the following have received appointments from the Admiralty, bearing date Oct. 1872:—Prof. Wyville Thomson, F.R.S., &c., Director of the Scientific Staff. Under him the following have been appointed:—Mr. J. J. Wild of Zurich, who will accompany Prof. Wyville Thomson as private secretary. (Mr. Wild was for some time private secretary to the Abbé Moigno, and is an accomplished artist); Mr. J. Y. Buchanan, M.A., Edinburgh, Chemist to the Expedition. (Mr. Buchanan has been until now Senior Assistant in the Chemical Laboratory of Edinburgh University, and has had the advantage of pursuing the study of his subject in Germany, at Leipsig and elsewhere); Mr. H. N. Moseley, M.A., Oxon, Naturalist (Mr. Moseley is a pupil of Prof. Rolleston, and has been enabled, by a Radcliffe Travelling Fellowship, to study biology further at Vienna and Leipsig: he was a member of the late Eclipse Expedition to Ceylon); Dr. Von Willemoes Suhm, Naturalist (Dr. Von Willemoes Suhm, who has been some time Assistant to Prof. Von Siebold, of Munich, is a distinguished naturalist, and is well known from his papers in Siebold and Kölliker's "Archiv" on Annelids); Mr. John Murray, of Edinburgh University, Naturalist (Mr. Murray, a Canadian by birth, has had great experience in taxidermy and the collection and preservation of Vertebrata generally, has travelled in Canada, and has also been far into high latitudes on a whaling cruise). Prof. Wyville Thomson will of course devote all his time not consumed by the superintendence of the scientific investigations in their various branches, preparations of reports, &c., to zoological work.

Of the three Naturalists, Dr. Von Willmoes Suhm and Mr. Moseley will undertake especially the Invertebrata procured during the expedition, whilst Mr. Murray will direct his attention principally to the Vertebrata. Mr. Moseley will also undertake botanical collection whenever an opportunity presents itself of landing in interesting localities, and all the scientific staff will give to this branch of biological investigation as much of their time as possible. Algæ, &c., obtained by dredging and otherwise at sea, will also be entrusted to Mr. Moseley. An experienced photographer, a noncommissioned officer of Engineers, forms one of the party. The expedition is under the immediate direction of the hydrographic department of the Admiralty.

The Admiralty authorities having applied to the Royal Society for advice in the conduction of the expedition, a committee of the Society was formed for the purpose of considering the subject, and counselling the Admiralty in the matter. The Committee consists of the President and Officers of the Royal Society, with Dr. Carpenter, Dr. Frankland, Dr. Hooker, Prof. Huxley, the Hydrographer of the Admiralty, Mr. Gwyn Jeffreys, Mr. Siemens, Sir William Thomson, Prof. Wyville Thomson, Dr. Williamson, and Mr. Alfred R. Wallace, and has the power of adding to its number.

The *Challenger* has had her timbers put in thorough repair, and has been specially fitted out for the work for which she is intended. She has an auxiliary screw, with engines of 400-horse power (nominal), and carries about one month's coal. She carries two cutters, a steam pinnace, a South Sea whaling or surf boat, a jolly boat, two gigs, and a dingy. Stages have been erected amidships, from which the dredges will be worked, and immediately aft of the stages is the steam winding-in apparatus. Prof. Wyville Thomson has been several times down to the ship to give directions for the special arrangements for scientific work. The fore magazine is prepared for the stowage of the large quantity of spirits which will be required for the preservation of natural history specimens, and of the many thousand stoppered bottles which will contain them. A chemical laboratory and naturalist's workroom have been fitted up in the afterpart of the vessel; and spirit is laid on to the workroom by means of a pipe leading from a metal cistern placed in the nettings. Several hundred miles of best whaling line have been prepared at Chatham for the *Challenger*, for dredging, and she carries about forty dredges. Amongst the stores are traps of various forms, harpoons, a harpoon gun, and fishing tackle of all kinds, including trawls, trammels, a seine, shrimp-nets, fish-traps, and lobster-pots. From the latter, used in deep water, great results are expected; and it is not improbable that living specimens of Nautilus may thus be procured. Prof. Wyville Thomson is now superintending the construction, in Edinburgh, of the various forms of apparatus required for physical research. Several beautiful instruments of this nature have been devised by Mr. Buchanan; and notably a new deep-sea pressure-gauge, and an instrument for bringing up samples of water from the bottom, which is provided with two taps which are closed by the contact of the apparatus with the bottom, and a safety-valve to allow of the expansion consequent on decrease of pressure as the apparatus is hauled up. A hydraulic machine will be carried on board, capable of testing the accuracy of all the physical apparatus, thermometers, pressure-gauges, &c., from time to time, in a chamber in which a pressure of three tons on the square inch can be obtained. The attempt will be made to use piano wire for sounding, after Sir William Thomson's method. A small aquarium has been devised by Mr. Moseley, which will be used for the study of the development of interesting animals. Except in absolutely calm weather it will be entirely closed, and a constant stream of water will be passed through it from a reservoir by means of finely perforated roses made—at the suggestion of Mr. Lloyd, of the Crystal Palace—of vulcanite. The route to be followed by the *Challenger* is not yet definitely fixed, and is still under the consideration of the Admiralty, who will be guided very much in the matter by the advice of the Royal Society Committee; but it will be very nearly as follows:—

The vessel, which is at present at Sheerness, will probably go round to Portsmouth about the middle of November, and sail from thence in the beginning of December for Gibraltar, the first haul of the dredge being made in the Bay of Biscay, if the weather should chance to be favourable. From Gibraltar she will proceed to Madeira, thence to St. Thomas, the Bahamas, Bermuda, the Azores; from thence to Bahia, touching at Fernando Norohna; then across to the Cape of Good Hope, and, after a stay in that neighbourhood, southwards to the Crozetts and Marion Islands and Kerguelens Land. A run southwards will then be made as far as possible to the ice, and the course thence be made to Sydney. New Zealand, the Campbell and Auckland groups, Torres Straits, New Guinea, and New Ireland will then be visited. A long cruise of perhaps a year will then be made amongst the Pacific islands; thence the expedition, passing between Borneo and Celebes, and visiting Luzon and its neighbourhood, will proceed to Japan, where a stay of two or three months is expected. Thence northward to Kamskatka, whence a run will be made northwards through Behring's Straits, and then through the Aleutian Islands, southward to Vancouver's Island, and so through the deep eastern region of the Pacific by Easter Island, and possibly by the Galapagos Archipelago to the Horn, and thence home. The voyage is expected to take about three and a half years.

It is difficult to over-estimate the immense benefit which science must derive from an expedition such as this. Apart from the results of intense interest which may be expected from the deep-sea work, the principal object of the expedition, and which must go far to elucidate a subject on which our knowledge is at present of the most imperfect description, abundant opportunity will offer for the accurate investigation of the animal and vegetable life of many highly interesting and yet imperfectly known or totally unexplored regions. The investigation of the floras of such islands as Fernando Norohna and the Marion and Crozett groups cannot fail to yield most instructive results; and it is needless to speak of the intense interest which centres in New Guinea.

No expedition has ever started under such favourable auspices as the present for yielding valuable scientific results, and great praise is due to the Government for the very liberal and thorough manner in which all arrangements have been carried out.

FIGUIER'S VEGETABLE WORLD

The Vegetable World: being a History of Plants, with their structure and peculiar properties. Adapted from the work of Louis Figuier. New and Revised Edition, with 473 Illustrations. (London: Cassell, Petter, and Galpin.)

NOTWITHSTANDING its ambitious title, this is, on the whole, a satisfactory book. If, however, in dependence on the title, it is ordered in the expectation of finding anything that will replace Lindley's "Vegetable Kingdom," or Baillon's "Histoire des Plantes"—at least, what this latter will be when finished, if it ever is finished—the purchaser will be disappointed. We have here a repetition of the old plan of attempting to compress into one small octavo volume an account of the Morphology, Physiology, Classification, and Geographical Distribution of plants. As far as can be, as we have said, the execution is good; some parts are even exceptionally well done; the defects are those of the plan. The style of Figuier's original work, florid and Gallic to excess, is entirely unsuited to the English reader; the "adapter" has used his pruning-knife with judicious severity, and has produced a book that may fill a useful place in popularising the study of botany, and leading the way to fuller and more special treatises.

The first part of the work, "Organography and Physiology," treats of the structure and different forms of the various organs in a systematic manner, and yet with a fresher style than is usual in text-books. It is, moreover, a relief to find that the majority of the illustrations, which are excellent throughout, are not those which have wearied the eye in many a familiar book. The details of terminology are relieved by information on many interesting points which we do not find in ordinary text-books. Thus in the very early pages we have an account of Knight's and Dutrochet's experiments with vertical and horizontal wheels to determine the effect which gravitation exercises in determining the downward tendency of the roots. Further on is a description and drawing of Hales' apparatus for measuring the force of ascending sap, by which he claims to have determined that the force which impels the sap in the vine is five times as great as that which impels the blood through the large arteries of the horse.

The portion devoted to the "Phenomena of Fertilisation and Germination" is full, and about the best in the book, the illustrations being especially superior to those found in most other works of a similar character. The cut here reproduced (Fig. 2) represents the very curious arrangement by which the fertilisation of *Vallisneria spiralis*, a favourite water plant in aquaria, is effected. The female flowers are borne on long spiral stalks which uncoil when the flower is ready to open, so as to allow it to float on the surface of the water. The male flowers, on the contrary, have very short stalks which are entirely submerged, but detach themselves when mature, rising to the surface, and then only discharge their pollen in the immediate proximity of the female flower; after which the stalk of the female flower again coils up, and carries it down to the bottom of the water, where the seed ripens.

The second part, on "Classification of Plants," is occupied with a brief description of the various systems adopted at different stages of botanical knowledge, with short biographies and portraits of the most distinguished botanists, from Tournefort to Robert Brown.

The third part is devoted to the "Systematic Arrangement of Plants," commencing from the lowest and proceeding to the highest forms. The account of the different orders of flowerless plants is the best with which we are acquainted in any elementary work, and will give the beginner an excellent outline of the immense variety of structure and physiological phenomena to be found among the lower forms of vegetable life. We find a good description of the different modes of reproduction in the Algæ, which are often so enveloped in technical terms as to be barely intelligible to the student. The illustrations also are excellent, as is the case throughout this section. Those here given of the early stages in the life of a fern taken from Thuret (Figs. 3—7), are such as have been heretofore unknown to English text-books. This section is, in fact, altogether thoroughly satisfactory.

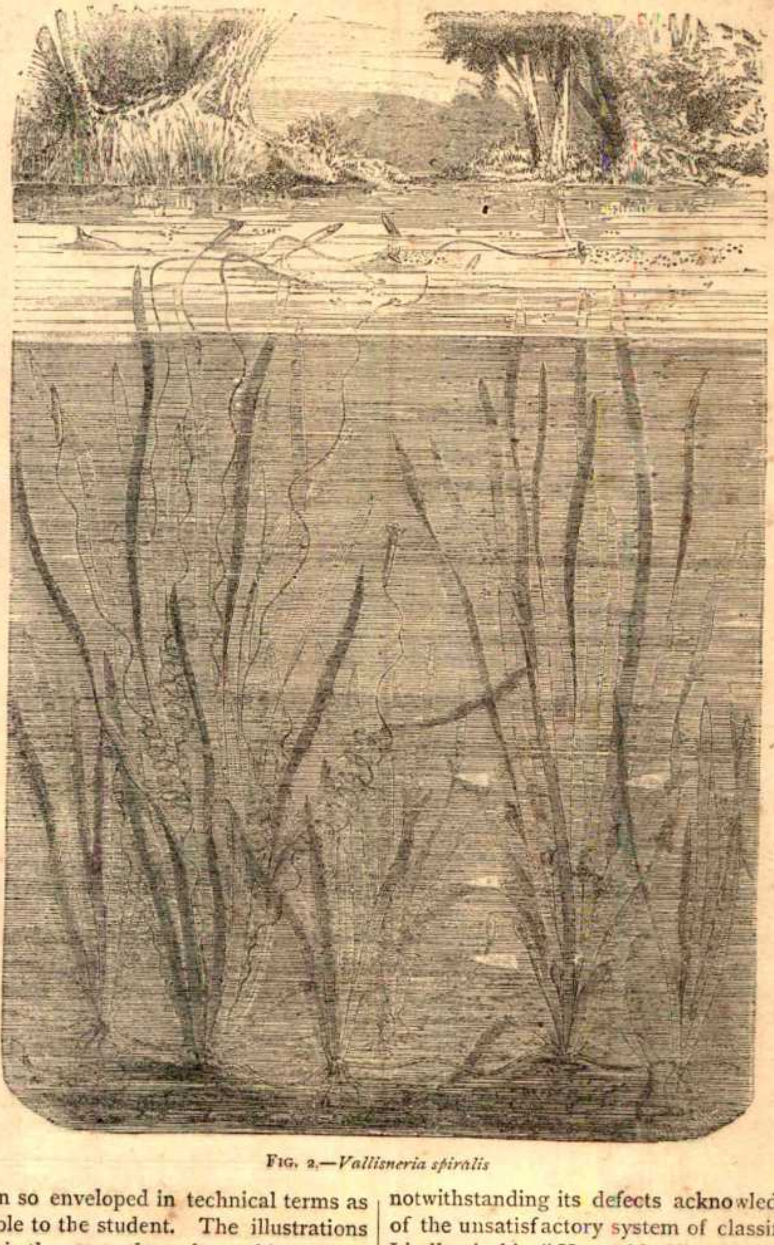

FIG. 2.—*Vallisneria spiralis*

More exception may be taken to the portion which treats of flowering plants, especially to the continued use, notwithstanding its defects acknowledged by the author, of the unsatisfactory system of classification adopted by Lindley in his "Vegetable Kingdom." The retention at the present day of such classes as the Rhizogens and Dictyogens as of the same value as Monocotyledons and Dicotyledons, is altogether indefensible. An equally serious defect is the very inadequate amount of attention given to the Gymnogens, and to the elucidation of the peculiarities of their structure which seem to form a connecting link between flowering and flowerless plants. It seems to us a mistake to attempt in an introductory text-book even an outline of the characters of the natural orders of flowering plants ;[1] but if this were necessary to the plan, this portion should have been completely re-arranged, to make it conform to the present state of botanical science. The chief advantage of the systematic portion of a course of botanical lectures is that it gives the teacher the opportunity of demonstrating the value in classification of certain peculiarities of structure, by placing side by side species belonging to the same and to allied natural orders, in a manner better calculated to arrest the attention of the student than if presented to him in any other way. This advantage, however, is not gained by the introduction of a section devoted to the details of classification into a text-book.

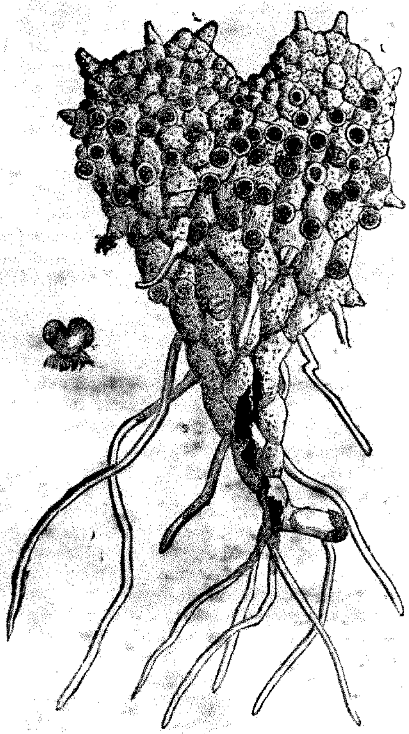

FIG. 3.—Prothallium of *Scolopendrium*, with Antheridia (magnified)

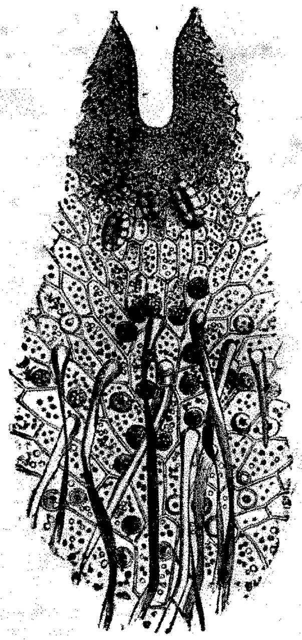

FIG. 4.—Portion of Prothallium of *Pteris serrulata*, showing Antheridia and Archegonia

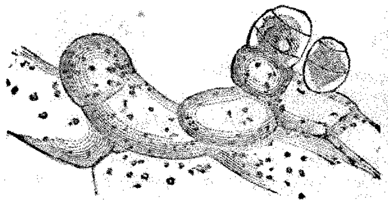

FIG. 5.—Antheridia (magnified)

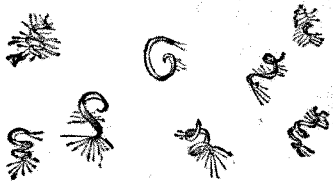

FIG. 6.—Antherozoids

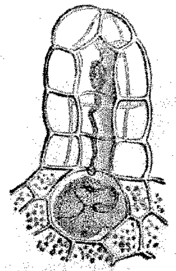

FIG. 7.—Isolated Archegonium, showing the action of the Antherozoids upon the Embryonal Cell

The fourth part of the work, on the "Geographical Distribution of Plants," though too brief, is interesting, and contains much valuable information in a small space.

Altogether, this English version of Figuier's "Vegetable World" is a good book to place in the hands of an intelligent student who is desirous of learning something of the laws which govern plant life. A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Ocean Currents

IF Mr. Laughton will take the trouble to read my previous Reports with attention, he will find that I have based no *argument* upon my "trough" experiment, which I have used merely as an *illustration*. The argument in favour of the *vertical* Oceanic Circulation which I advocate rests upon the *facts* of Deep Sea Temperature. In my forthcoming Report, these facts (including many which have not been hitherto published) are discussed, in connection with the Temperatures of Inland Seas; and if Mr. Laughton will frame a better hypothesis for the explanation of them than that of the Thermal Circulation first advanced by Pouillet, and latterly accepted by Herschel and Sir William Thomson, I will gladly accept it.

Mr. Laughton will also find that I am not ignorant of the geographical facts he mentions respecting the *horizontal* or *superficial* movements of the Ocean. But he must be well aware that a current may be flowing in one direction *on* the surface, and a tidal or other movement in a contrary direction *at a small depth* beneath it. A very careful observer told me a few days since that at a time when the *surface*-current in the Dardanelles, urged on by a south-westerly wind, was blowing *inwards*, he had distinctly seen the movement of the water *at a short distance beneath the surface* to be *outwards*—this being indicated by the direction of the water-weeds. Below this, again, as the researches of the *Shearwater* have shown, there is a deep under-current *inwards*.

In confirmation of this last statement, my friend Mr. Redhouse, who resided many years at Constantinople as Translator to the Embassy, has informed me that the existence of the deep under-current in the Bosphorus has long been perfectly well known to the native fishermen of Constantinople, as well as to European residents who amuse themselves with the sport.

William B. Carpenter

University of London, Oct. 29

London University Examinations

Prof. W. G. Adams, in order to controvert my statement that mechanical and natural philosophy have little to do with medicine, enters into theories with regard to the production of animal heat which I must leave him to settle with his colleague, the Professor of Physiology in King's College. As he insinuates a doubt as to my own acquaintance with the thermometer and its uses, on my own behalf I may venture to say that not only did Professors Graham and Brande require a knowledge of this and kindred matters of candidates for the Matriculation and First M.B. Examinations of the University of London, but that years before Mr. Adams was connected with King's College, I was rather a "swell" at natural philosophy and chemistry under the late Dr. Miller's tuition.

Mr. Adam's temperature must, I fear, have been abnormally high, or his barometer of propriety correspondingly low, when he penned the paragraph relating to the report of the sub-committee, and endeavoured to gain support for his views by suppressing the latter half of the quotation. The sentence really stands as follows: "The preliminary scientific examination has tended to give prominence to theoretical and scientific knowledge *at the expense of a sound practical acquaintance with medicine, surgery, midwifery*," &c.; but by the omission of the words in italics Mr. Adams makes the report (contrary to its whole tenor) support his view that "it is in consequence of such knowledge that medical science has advanced with such rapid strides." The illustration of the application of a cupping-glass is not a very happy one, for cupping has for years been notoriously a purely mechanical art entrusted to medically-unqualified men, who could in no sense claim a knowledge of natural philosophy.

In conclusion, may I say that the Senate of the University of London at its session of the 23rd inst., took action in the matter to which I have called attention, and appointed a committee to consider it; and may I express a hope that, should Mr. Adams be really ill, he may not be unfortunate enough to fall into the hands of one of his own medical philosophers?

Cavendish Place Christopher Heath

Can the Stature be in any way affected by the Will?

It is written that "no man by taking thought can add one cubit unto his stature;" but if there be any truth in the following extract from Babbage's "Passages in the Life of a Philosopher," it appears that man can, at all events, voluntarily deduct nearly an equivalent amount from his height.

At the opening of chapter xviii. of the work just cited, Mr. Babbage makes the following statement respecting the celebrated thief-taker Vidocq, with whom he had an interview:—"He had a very remarkable power, which he was so good as to exhibit to me. It consisted in altering his height to about an inch and a half less than his ordinary height. He threw over his shoulders a cloak, in which he walked round the room. It did not touch the floor in any part, and was, I should say, about an inch and a half above it. He then altered his height, and took the same walk. The cloak then touched the floor, and lay upon it in some part or other during the whole walk. He then stood still, and altered his height alternately several times to about the same amount.

"I inquired whether the altered height, if sustained for several hours, produced fatigue. He replied that it did not, and that he had often used it during a whole day without any additional fatigue. He remarked that he had found this gift very useful as a disguise. I asked whether any medical man had examined the question, but it did not appear that any satisfactory explanation had been arrived at."

Now if this had been the statement of an unscientific person, or one whose powers of observation were presumably untrained, it might be put aside unheeded; but coming, as it does, from one very unlikely to jump to conclusions, it seems to merit some degree of attention.

This, then, being granted, the question arises, how can we account anatomically for this shortening in height? Of this the solution does not appear to be very clear. The only way in which an individual could alter his height would be either by adopting a stoop of his neck and shoulders, or by bending his knees, and flexing his thighs upon his pelvis, or, lastly, by actually shortening his vertebral column.

The two first methods may be disregarded, as they would be pretty evident, even if a cloak were worn, and, if employed by Vidocq, would scarcely have aroused the curiosity and wonder of Mr. Babbage. The last only, namely, a voluntary shortening of the vertebral column, remains then to be considered.

Now, there seems to be a general impression, both among doctors and the laity so-called—though it is difficult to discover any definite and concrete expression of it in the text-books—that, by virtue of the compressibility of the intervertebral fibro-cartilaginous discs, the stature of a man when he goes to bed is shorter than when he gets out of it, the amount of shortening varying, I suppose, according as the individual dangles a cane in the "Row," or is employed somewhat more actively as a "fellowship porter" at the docks.

Granting, then, that there may be some passive, involuntary shortening of the vertebral column to the extent of an inch or an inch and a half[*] after the application of a weight to its summit for the duration of some hours, how does a voluntary shortening come to be brought about? Since fibrous and cartilaginous structures are not directly acted upon by the will through peripheral nerves, such action must be produced through the medium of muscles; and here we come to the crux, what are the muscles which could be employed in shortening the vertebral column? Hæmally, the only likely muscle in the cervico-dorsal region is the vertical portion of the *longus colli*, which passes from the bodies of cervical vertebræ Nos. 2, 3, 4, to the bodies of the three lower cervical and three upper dorsal vertebræ; and in the dorso-lumbar region there is the *psoas magnus*, which takes origin from all the lumbar and the last dorsal vertebræ, but which, unless the femur (where it is inserted) were fixed, could hardly affect the vertebral column, while neurally there are the numerous dorsal muscles of complex arrangement, such as the *quadratus lumborum, sacro-lumbalis, longissimus dorsi*, &c.

There seems to be, however, nothing in the arrangement of such muscles as would satisfactorily account for a voluntary shortening and elongation, or rather, restoration to the normal length, of the vertebral column, though it is possible that in some individual cases there may be some special endowment of innervation and co-ordination of muscle which permits of such action, just as it might permit of the wagging of the ears, or of the scalp movements, which may be occasionally witnessed in those gifted with such accomplishments.

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[*] Philippus Pieper, in an inaugural thesis, "De Viribus Corporis Human Mechanicis," Berolini, 1821, states, with regard to the elasticity of the vertebral column (p 5), that in a man of middle height who had been carrying weights the difference at the end of the day was only 11‴. In the last edition of Druitt's "Surgeon's Vade-Mecum" it is stated (p. 341) that "the intervertebral substance is compressible to such an extent that an adult man of middle stature loses about an inch of his height after having been in the erect position during the day. Since the united thickness of the intervertebral substances in an adult man is about 3 875 inches we see that they lose nearly one-fourth by compression, which they do not recover till after some hours of rest." Among works which I have consulted in vain upon this point are, Borelli, "De Motu Animalium," and "De Vi Percussionis" (Lugdun Batav 1686), Giraud-Teulon's "Principes de Mécanique Animale" (Paris, 1858); Henle's "Banderlehe;" and the "Traité de la Mécanique des Organes de la Locomotion," by G. and E. Weber, in tom. ii. of the "Encyclopédie Anatomique." W. and E. Weber's "Mechanik der Menschlichen Gewerkzeuge" (Gottingen, 1836), is unfortunately in none of the large libraries to which I have access.

It is in the hope, then, that some one who has studied anatomy from a mechanical stand-point may throw some light upon this somewhat obscure matter, that I have asked a question which, I trust, my be one not unworthy of the consideration of "philosophical" anatomists.

New University Club, Oct. 16 J. C. G.

Magnetic Storm Oct. 14—18

On the 14th of this month a magnetic storm commenced at 10.20 P.M., and lasted until 11 P.M. of the 18th. It was as remarkable for the extent as for the duration of the disturbances. The only lull in the storm was during the afternoon of the 16th.

The general character of the perturbations was the same throughout, consisting mostly of long movements of the needle to and fro on either side of the mean position. There was a very striking coincidence between the curves of the 15th, 16th, 17th, and 18th during the morning hours, the maximum westerly deviation having been attained at about 6 A.M. on each successive day. During the afternoon of the 17th, the greatest movement of the declination needle towards the west was equal to that of the previous morning, whilst the oscillations towards the north were greater than on the other days of the storm. The movements of the vertical-force magnet were frequently too great to be recorded on the photographic paper, and this magnet was several times thrown off its balance. The horizontal-force magnet was more violently disturbed at the very commencement of the storm than at any subsequent period.

The remarkable coincidences that are now being discovered between these magnetic disturbances and other important natural phenomena render it useful to draw attention to those changes in the magnetic force of the earth which present any feature particularly worthy of notice. The storm I am now referring to is on several accounts the most important that has occurred since 1867, and it is to be hoped that some practical spectroscopist has had favourable weather during these few days, as a magnificent array of solar prominences may not improbably have rewarded his interesting labours.

Stonyhurst Observatory, Oct. 23 S. J. Perry

Circular Rainbow

Being, in company with a friend, Mr. Hall, on the east peak of the Berceau (3,640 ft.) on the 25th inst., a circular rainbow was visible at 2.30 P.M. upon the upper surface of light white clouds that drifted from S. up the rocky valley which was E. of where we stood—though the true wind was W. and moderate. The sky was almost of the richly dark Italian blue, across which a few clouds (cirrus) slowly passed.

When first seen the diameter of the outside of the circle was 10°, but it increased gradually up to 15°. The colours of the "bow," which was somewhat less than 1° in width,—were the same as in the common rainbow, and very vivid. When we were a few yards apart, each saw only his own figure within the circle, large and well defined, so that the movement of an arm became visible. Before long the shadow of the mountain on which we stood invaded the lower portion of the circle, depriving it of its colour, but not always destroying its continuity;—and my figure remained complete even where the continuity was destroyed. Light clouds passed across the sun, causing a partial, rarely a complete, disappearance of this most beautiful phenomenon, which we watched with great interest for twenty-six minutes—how long it had existed before 2.30 I know not.

M. Moggridge

Earth Currents and Sun-spots

In the last number of Nature there appears a letter from Mr. W. H. Preece respecting the recent occurrence of electric storms of considerable intensity; and in connection with this interesting subject it may be worth while relating that the solar spots have lately (that is to say, during the last few weeks) been larger than usual. One of them attained great proportions, and was distinctly visible to the unassisted eye (on October 15) through a fog which partially obscured the sun's intense light. On the date mentioned this spot had completed about one-half of its transit across the solar disc, and it is remarkable that on the same day the electric storms attained their maximum, and "the interruptions to business were serious," as remarked by your correspondent. The spot referred to was not so large as several which appeared during the two preceding years, but exceeded in magnitude any of those which have come under my observation during the present year. It disappeared from the sun's western edge on about the 21st of October; but just previously to this the spot had been considerably smaller, and showed indications of dissolution. The spots now visible on the solar surface are not very conspicuous; there are, however, two visible of the larger class—one of these is situated in the north-east quadrant, and the other in the south-east quadrant, and they are situate at about the same distances from the limb. Between these spots there were yesterday two small ones perceptible running parallel with the equator.

Bristol, Oct. 28 William F. Denning

Measurement of Faint Spectra

May I suggest, as a supplement to Mr. Capron's clever arrangement for spectral measurement, a method which I have found useful with faint spectra. It is that a part of the slit should remain fixed, while the upper or lower half, or the middle only, should be movable. In this way two images of the spectrum are formed, one of which may be made to move over the other like a vernier; and thus any line may serve as an index, when, from want of light, it would be impossible to see the brass points. We obtain in this way many of the advantages of Zollner's reversion spectroscope.

With such an arrangement, and with an embossing edge attached by a spring to the movable slit, so as to register on a card when pressed, I have succeeded in making several tolerable measurements of the faint auroral bands, which it was difficult to perform by direct comparison It is, of course, necessary to have at least one line of known position in the field.

North Shields, Oct. 22 Henry R. Procter

Merrifield on the Deviation of the Compass

Will you kindly pardon my again troubling you with an explanation?

Last week, seeing what I considered a harsh review of my little book, in Nature for Oct. 17, I, in the midst of my work and the heat of the moment, penned a reply to my reviewer, without thinking more of the matter. To-day, whilst giving a lesson on the subject to a pupil, I saw my error; and I beg to plead guilty to that "looseness" which has led to inaccuracy in the passage quoted from page 52. Instead of "deviation" I meant, and should therefore have said, "Vertical iron shows the same indirect magnetic force," &c., and to my class I have always used these words. I now tender my apology to my reviewer for my hasty letter, and beg to thank him for pointing out this "looseness and inaccuracy." I trust you will make this letter as public as my last. John Merrifield

Navigation School, Plymouth, Oct. 26

Rainfall in Bombay

As it may interest some of the readers of Nature to know the amount of rain which fell on one occasion during the heavy monsoon rains which recently occurred at Bombay, and which I regret to see in your Notes had so disastrous an effect upon the library of the Asiatic Society, I quote the following from a letter which I have just received from my friend Mr. C. Chambers, F.R.S., Director of the Colaba Observatory, dated 10th Sept., 1872.

"Just a week ago we were treated to 7·20 inches of rain in two hours, which is nearly twice as heavy as I have known before, *i.e.*, had personal experience of."

In order that we may form some idea of the enormous amount of the downpour, we must imagine the whole rain which has fallen in this neighbourhood since June 8, to have been concentrated in the time named; or, perhaps, better still, to suppose that the heaviest part of the shower which fell about half-past twelve, on the 3rd inst., and lasted for seven minutes, had continued for two hours.

Mr. Chambers was some time ago much troubled by the presence of foreign matter, apparently dust, in the interior of his magnetic instruments. He now writes :—"In NATURE, vol. vi. p. 286, Fig. 2, is a thing very like the organic functions I speak of as being seen on the knife edge and plane of the vertical force magnetograph. I have described it as looking like the 'interlacing tea leaf stalks,' doubtless it was beginnings of life."

Can any of your readers state if it is probable that such objects are to be found in the place he names.

Kew Observatory, Oct. 23 G. MATHUS WHIPPLE

THE APPEAL FOR SKELETONS OF WILD ANIMALS

I AM glad to see that Mr. Moseley has started the question of the acquisition of skeletons of wild animals, a subject which has hitherto been too much neglected by those who have charge of museums. Mr. Moseley might have put his case more strongly than he has done; for not only are the two museums he mentions destitute of skeletons of the wild specimens of the larger *Felidæ*, but, so far as I know, no European museum possesses more than skulls. Possibly there may be an entire skeleton in the very rich museum of Leyden, but there are none in the British Museum, nor at Paris, nor Vienna, so far as I have been able to examine those collections. We are better off at Cambridge, for we not only have a considerable series of skulls of tigers, leopards, and the so-called "maneless" lion of Guzerat, but a fine skeleton of a Puma (*Felis concolor*) sent home from Florida in excellent condition by one of that much-abused class, "sportsmen."

There is, however, a subject even more important than the acquisition of foreign animals, namely, the collection of a good series of skeletons of different ages and sexes of all the European mammals. This is no easy matter, even in the case of the commoner species. We have only lately succeeded in acquiring an adult skeleton of the Red Deer (*Cervus elaphus*); but the one we have obtained (through the kindness of Mr. Balfour, of Trinity College) is an adult Royal stag, so fine as to be worth waiting any length of time for. Again, how many museums possess a skeleton of the brown bear of Europe, or the lynx, or the glutton, or the wolf, or even really good skeletons of such comparatively common animals as the badger, the otter, and the numerous small *Viverridæ*? And yet the bones of these occur more frequently in turbaries than do those of the extinct *Felidæ* in caves, while they are certain to become extinct from the pressure of civilisation and the consequent restriction of their range, far sooner even than those large animals which are directly persecuted, as tigers are in India.

I find it easier almost to get skeletons sent from abroad than to have them collected in England. Any gentleman who unites with love of sport a knowledge of natural history—no uncommon combination—will often send home considerable collections, and take great trouble to procure the different animals that he has been asked to look for. Such a collection we have just received from Lord Walsingham (of Trinity College), formed by him in North-west America. It includes complete skeletons of *Ovis montana*, *Antilocapra americana* (Pronghorn), white-tailed stag, mule deer, black bear, beaver, martin, besides a series of separate skulls. Last year we got an *Otaria* from San Francisco, one of the herd which the intelligent citizens of that capital are wise enough to preserve, and a musk-ox from the German North-Polar Expedition. In short, there are few animals that may not be acquired by energy and perseverance; and travellers in distant countries are fond of showing that they have not forgotten their old university; but it is infinitely more difficult to induce gentlemen, or their keepers, in England or Scotland, to collect the wild animals that still linger in their preserves; and this is the direction in which I venture to think an effort should be made.

The "directions for preparing skeletons" given by Mr. Moseley are excellent. Allow me to make one or two additions to them. It is most important to note the sex of each animal, with the locality in which it was taken and the date of its capture. I do not recommend the soaking of the carcase in water after the muscles have been removed. It loosens the ligaments, and makes the after-process of drying more difficult—a process which is difficult enough in Europe, especially in mountainous districts. Moreover, it is difficult to find a suitable place to do it in abroad. I find the colour of the bones not seriously affected by the non-extraction of the blood. The skeleton may be packed up before it is quite dry if sawdust be substituted for hay or straw. Pine sawdust is especially good for this purpose. It is very fine, dry, and slightly antiseptic.

Museum, Cambridge, Oct. 24 J. W. CLARK

THE ZOOLOGICAL STATION AT NAPLES

SINCE the last notice given in NATURE,* the building is almost finished, and all endeavours are now concentrated upon the arrangements of the interior. Two more months, and the fifty-three tanks of the public aquarium will be ready to be filled with the clear and limpid water of the Mediterranean.

The upper story receives still more attention. My plan of letting the tables having met with great applause from all sides, has worked some changes in the general arrangements of the rooms. The room previously intended for the library has been added to the great laboratory, which now measures 40 ft. in length, 25 ft. in breadth, and 24 ft. in height. It has three great arched windows 20 ft. high and 10 ft. broad, to the north, and three smaller ones looking into the small light-court in the centre of the building. The former three windows will give light to six microscopic tables, whilst the three smaller windows will yield enough light to three tables fitted up for common anatomical work. In the centre of the laboratory a wooden stand will be placed, 27 ft. in length and 8 ft. in breadth, and having three stories. This stand will bear tanks of different sizes—the lowest story the heaviest, the upper the smallest. The latter will be moveable, so as to allow close inspection on the working table. Each of them will receive a small current of sea water, and will have its own outlet, so as to isolate completely its contents from the neighbouring tanks. There will be plenty of room for some sixty or eighty tanks. The water running out of them is collected, and runs down into the tanks of the public aquarium. Four doors unite the laboratory to the three adjacent smaller rooms, which are provided each with a working table and with tanks, whilst the fourth door leads to a corridor and to the staircase. A gallery all round the walls of the laboratory, at the height of fourteen feet, will furnish room for the library. Two small staircases unite it to the floor of the laboratory, and four narrow doors to four adjacent small rooms, of which two may be used as reading-rooms for making notes, &c. It will be absolutely forbidden to take any book out of the building.

On the same floor as this great morphological laboratory, the physiological one is to be found; indeed the door which opens to the corridor leads also immediately to the room destined for this purpose. Its length is 20 ft. by 14 ft.; it has several glass doors to the west, opening upon an ante-room as wide as the room itself, and which, in case of need, can easily be transformed into a laboratory, thus enlarging the physiological laboratory to double its present size; it has a separate tube, with a constant supply of sea-water, and a table for microscopic work. Prof. du Bois-Reymond has promised to assist in arranging instruments and apparatus for experimental use.

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* See NATURE, Vol. v. p. 437.

Besides these laboratories, there are rooms with windows and glass doors, all capable of being transformed, when necessary, into laboratories, for every room has its tube with sea-water. But as it is most likely that by-and-by extensive collections will be formed, to assist in working out a most accurate and detailed fauna of the Bay, or even of the Tuscan Sea, these rooms, especially a large one on the south side, will at first be left empty.

Downstairs there is another small apartment on the north side, destined for a botanical laboratory. It has one large and two smaller windows, thus allowing four microscopical tables to be furnished, three of which will be let, whilst the fourth belongs to the botanist of the station, who is to be engaged next winter. In the basement two series of store-tanks will be placed, into which all the animals will be put immediately after being caught by the fishing and dredging expeditions, which will be sent out every day, weather permitting.

The library of the station has received many valuable presents. Thus Prof. Allman, Mr. Darwin, Prof. Flower, Mr. Gosse, Prof. Huxley, Mr. Gwyn Jeffreys, Sir John Lubbock, and Prof. Owen, have promised or sent their biological works; and German publishers, such as Georg Reimer of Berlin, and Braumüller of Vienna, have joined Engelmann, Vieweg, and Fischer, in offering all their biological publications. A catalogue is being prepared, containing a complete list of the actual state of the station library.

By the kindness of Mr. Gwyn Jeffreys, dredges have been procured of the best pattern, such as that experienced zoologist recommended; boats have been built for special dredging purposes, and everything also is being prepared to render the station as efficient as possible.

We hope in our next article to give some information as to the relations, into which the new institute has entered with governments and learned bodies. Here we may still be allowed to point out, that since the foundation of the Naples station has been taken earnestly into hand, similar endeavours have been made both in Austria and France. In both countries the Government has been asked to establish Zoological Laboratories on the coast. We have still to wait the results of such demands.

Naples, Oct. 24 ANTON DOHRN

VESTIGES OF GLACIAL ACTION IN NORTH-EASTERN ANATOLIA

IN a paper dated some months back* I gave an abridged notice of some traces of ice-action, referable to the so-called epoch, in the central plateau of Asia-Minor. A journey undertaken this summer through the north-eastern districts of the peninsula has enabled me to observe several other phenomena of the same class, and to determine in some measure the extent and degree in which that prolonged depression of temperature affected this region.

My route traversed an extensive but rarely visited tract of country, that, namely, of the great Chorok, or Harpagus river-valley from Beyboort to Artween, and the mountain lands that extend beyond that valley east and north up to the frontier of Russian Georgia, returning by the Black Sea coast. The space thus explored extends from long. 40° to 44° E., and from lat. 40° to 42° N.

The valley of the Chorok river, for a distance of about 120 miles—that is, from the neighbourhood of Beyboort to that of the town of Artween—runs almost parallel with the sea-coast in an E.N.E. direction, and is separated from the basin of the Euxine by a lofty chain of mountains, the higher peaks of which reach an altitude of 11,000 feet above the sea-level, and even more. The whole long and narrow strip of land bears the name of Lazistan, or country of the Lazes, a Mingrelian tribe, mentioned by Strabo as tenanting the same region in his time.

Near Artween, long. 42° the valley turns sharp to the north, and finds its way through a narrow and precipitous cleft to the sea.

The southern side is determined by the highlands which form the watershed between the tributaries of the Black Sea and those of the Persian Gulf; but farther east the same range, deflecting somewhat to the north, unites with the prolongation of the Lazistan mountains, and acts as watershed not only to the already-mentioned streams, but also to those of the Caspian, which it separates from the two other fluvial systems. Farther on the Russo-Georgian frontier follows its eastern slope.

Returning to the Chorok valley—one might almost call it trench—I may as well notice that its height above sea-level at Beyboort is about 5,000 feet, and at Artween only 1,000 feet, whence the extreme rapidity of the river, suitably named the Harpagus, may be inferred. The geological character of the mountain chains on either side is extremely varied. Cretaceous and Jurassic strata have in both been extensively superimposed on the plutonic rocks that frequently pierce through and form the higher ranges; volcanic formations, less ordinary in the southern chain, are of frequent recurrence in the northern. Indeed the Lazistan mountains, where they dip into the sea, are almost wholly volcanic in structure. Large tracts of a metamorphic character also occur, but more on the northern than on the southern side.

Roads, in a European sense of the word, throughout all these districts, there are none; even a tolerable horse-track is only an occasional luxury. Hence my entire tour was performed partly on horseback at walking pace, partly on foot; so that I had full opportunity for the most leisurely observation. My route first followed the southern side of the Chorok valley for about seventy miles, then the northern for about fifty more, after which I traversed the eastern highlands to the Russian frontier, a distance of about 160 miles, then turned north till I reached the Black Sea coast, along which I returned.

And having now given these summary indications, which the nature of the country, scarcely ever visited by Europeans, and in general very little known, seemed to make necessary, I will now proceed to the account of the principal phenomena referable to the glacial period.

While travelling at an altitude varying from 3,000 to 7,600 feet according to the exigencies of the route along the southern side of the valley—that is, on the northern slope of the Euphrates watershed—I crossed three large moraines, two of them descending from the slopes of Charmeli Dagh, a lofty granite ridge, streaked with snow all through the year. Their lower extremity was at about 5,000 feet above sea-level, their upper origin attaining nearly 8,000 feet. The mountain sides here are Jurassic or limestone; but the broad streams of angular blocks that follow their depressions were almost exclusively granite of the same kind as that which forms the mountain wall above. Where, however, the general altitude of the chain does not exceed 7,000 feet, as is occasionally the case, no moraines are to be observed, though large angular boulders are not uncommon on the broad ledges. The upper mountain lines are invariably rounded, and, as it were, smoothed off; the sides marked with scooped depressions much too wide for their depth to be attributable to torrent action; low down in the valley the slopes terminate in rifted precipices.

That the epoch to which these moraines belong was posterior to that of the volcanic action which, though long since extinct at the surface, has left so many traces along the north-eastern coast of Asia-Minor, was rendered in one instance sufficiently evident by the constituents of a broad stone-ridge which I crossed near the highest point of the mountain chain, a little to the east of Erzeroom.

Here, at an elevation reaching to upwards of 7,000 feet, the ordinary Jurassic strata were interrupted by a volcanic outbreak of several miles in extent, like a huge patch of black lava and scoriæ extending far up the mountain side, where traces of a large crater were still observable. Above, to a height of full 9,000 feet, towered the granite peaks, and here, reaching down towards the valley, a wide moraine traversed the road. It was mainly composed of volcanic fragments, though mixed with blocks of granite; and must consequently have been formed at a time when the volcano had not only existed, but had also ceased its action.

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* See NATURE, vol. v. p. 444

To assure myself of the true character of the phenomenon, I quitted the path and rode up and along the stone-stream to a considerable distance, till in fact my horse could no longer make his footing sure; and I had fully convinced myself that the moraine was the result of glacial action alone, not of torrents or weather action referable to more recent times.

Not far on I had to traverse the pass called Keskeem Boughaz, or, the entrance of Keskeem; this latter being the name given to the district on either side of the lower Chorok valley. The road here reaches an altitude of 8,200 feet; yet is far overtopped by the granite range of Tortoom, even now streaked with perpetual snow, to the south. Here again I observed a large moraine, winding down from the upper ridge; while the first plateau of Keskeem, about 7,400 feet above sea-level, into which I next descended, was strewn capriciously with large granite boulders, many ten or more feet in diameter. Another volcanic tract succeeds, where the path winds along a valley hemmed in by gigantic cliffs of black lava, dashed with blood-red porphyritic stains. From this point my track followed a level too low to permit of expecting or of finding any further glacial traces in this region.

Summing up the observations made during this stage of my journey, I come to the conclusion that the ice-cap of the north-easterly Anatolian watershed, in post-pliocene times, must have reached downwards, on the northern side of the range at least, to about 7,000 feet above the present sea level; while some of the glaciers issuing from it descended to about 4,500 feet of the same measurement. In what degree the sea-level of the entire eastern portion of the peninsula has changed—it would seem since the epoch referred to—I shall speak further on.

Two phenomena only remain to be noticed;—one, the absence of all organic traces, whether marine or otherwise, in these rocks and strata; an absence which I have heard remarked on by the few natives capable of observing these things; another, the frequent presence, in the moraine or glacial belt, of scratched and striated rocks, especially granite.

Crossing the river, now at its shallowest in the summer season, but still containing, at a distance of a hundred miles from its mouth, as large a body of water as the Thames during high tide at Richmond bridge, my path led to Artween, the chief village-centre of these regions, along the north-western side of the valley; that is, along the inner slope of the coast range. These Lazistan mountains form a very lofty, but comparatively narrow ridge, of great steepness, and ill adapted to the formation of glaciers; and besides they must have been, even in the glacial epoch, exposed to the comparatively mild atmosphere of the great sea, now represented by the Black Sea and the Caspian only, but which then covered so large a portion of what is now Russia.

Here, however, I again found evident traces of the same cold period, but written in different characters. Not moraines indeed, nor the other analogous appearances indicated in the more inland district, but signs of alternating snows and thaws, of weather-change and water-action on a scale much vaster than is possible in the existing condition of climate, even were the most rigorous winter, such as now is, to be succeeded by the warmest summer. Wide and deep clefts, the work of torrents, yet flowing, but dwindled to comparative insignificance; great sweeps of shattered rock fragments down slopes inaccessible from their steepness, due to frosts of a severity unknown at this day, followed by corresponding thaw; and every mark of climatic disintegration, much beyond, though in kind similar to, that which these crags now undergo. And lastly the water level of the Chorok itself, judging by the eroded shelves and like indications left here and there in the cliffs along its shores, must have been from fifteen to twenty feet above its actual level; a circumstance which can scarcely be attributed to other causes than the melting of great supplies of ice and snow; since there is no reason whatever to suppose that any considerable diminution in the forest growth around has taken place from the earliest to the present times.

That there really was such a difference between the glacial conditions of the Lazistan, or coast mountains, and those of the inland watershed in the epoch alluded to, is in a measure confirmed by their actual state. For though the Lazistan peaks considerably surpass in height those of the southern chain, being some of them above 11,000 feet in elevation, whereas the others average from 9,000 to 10,000 only, yet snow lies all summer through on the latter, much more abundantly than on the former; while on the other hand the annual quantity of rain and snow that in the winter months of the year fails on the Lazistan mountains is at least the double of what is apportioned to the southern or Armenian chain. A depression of 15° to 20° centigrade in the average temperature of the year, would now to a certainty cover the latter with glaciers, while it would furrow the former with torrents of the first magnitude.

Leaving the Chorook valley, my road—or track, to speak more properly, for road in our sense of the word there was none—led north-east up to the great water-shed already often mentioned, and which here, turning northwards also, separates from each other not the Black Sea and Persian Gulf river systems only, but a third also, that of the Caspian. For about forty miles my journey, though passing through a district abounding in other geological phenomena of great interest, yet supplied me with none of the class to which these notes specially refer, for the reason that it lay wholly along valleys and through ravines often below 4,000 feet in sea elevation, and never exceeding that height. But at the Karanlik Dagh, or Mountain of Darkness, so called either from the black and dense fir forests that clothe its sides, or from the thick mists that hang for months along its middle slopes, and at a point as nearly as possible opposite the extreme north-eastern angle of the Black Sea, here about fifty miles distant in a direct line, I began at last the ascent of the main ridge, the backbone of the land. While slowly climbing the limestone ledges of the mountain, and at the height of 6,400 feet, I here once more found athwart my way a colossal moraine, formed of worn granite blocks and partly overgrown with forest, descending from an overtopping height, which I afterwards ascertained to be about 8,000 feet. But before we reached it I traversed an intervening ledge, 7,300 feet above the sea, composed of granite rocks, worn and marked with unequivocal ice action, though now wholly bare of snow. A valley divided this ridge from the highest of all, that called Penek, up which a difficult track, called the "Egri Yokoosh" or "crooked ascent"—and it well deserved its name—brought us at last, landing us on a cold, undulating granite plateau of 9,000 to 9,500 feet in elevation. Here and there its depressions were scooped out into deep little oval lakes, full all summer through of clear blue water, and looking the very memorials of vanished ice; while the gently sloping sides of the plateau itself were strewn with boulders of every size and shape, but all granite, seemingly brought there from the higher peaks of the Penek chain, about five miles off. Nor did these boulders cease to occur, sometimes in greater abundance, sometimes less, till we reached the great basin of Ardahan, near the sources of the Kur, or Cyrus river, here a slender stream, on its way to join the Araxes, and enter with it the Caspian Sea.

The height of the Ardahan plain is 6,500 feet; it is, but for a very gentle easterly slope, an absolute water-like level; the bottom of this lake basin, for such it certainly was, consists of deep alluvial soil, mixed with detritus and large boulders; the sides are all rounded and smoothed off in gentle slopes, and bear every mark of having been long ice-covered. They are of various altitudes, but all alike. I climbed one of the lateral plateaus, at the north-eastern corner of the plain, till I had reached 2,000 feet above the Kur stream; boulders everywhere.

These plateaus stretch east to the Russo-Georgian frontier, about twenty-five miles distant. They contain many notable lakes, some of which I visited; that called Childer, in particular, is about ten miles in extreme length by eight in breadth. Its surface is 6,700 feet above the sea, and it is encased in mounded hills, like those already described. The natives declare its depth to be unfathomable, and, somewhat inconsistently, affirm that a submerged city exists below. But a clever Beg, or Chief of the neighbourhood, a friend of mine, had the curiosity to sail across it in every direction, sounding the bottom, and assured me that its greatest depth, near the northern extremity, did not exceed twenty-two fathoms, while he added, laughing, that of the buried city his line had discovered for him no trace. Karzach lake, not far off, a square-shaped sheet of water about four miles in extent each way, seems to be stilll shallower; while Teh Lake, as it is called, close by which my road passed, is now a mere marsh, though of considerable dimensions. Like the other two it has left, however, on its banks the marks of having been once much deeper and wider than at present. The plateau on which these lakes are situated continues, with alternate elevations and depressions, but always bearing the features already described, for about thirty miles to the north, till, having reached its greatest altitude in Kel Dagh, a mountain about 11,000 feet high, it begins to descend step by step to the plains of Georgia and the Black Sea. From this point its whole character changes, rifts, abrupt precipices, and narrow gorges taking the place of the rounded undulating outlines it bore farther inland. Nor is any further trace of boulders or moraines to be seen, at least below an elevation of nearly 8,000 feet.

It is to be remarked that this entire range, like the central Anatolian watershed, is almost uniform in its geological composition; Jurassic on its lower slopes; granite above. One only exception here occurs, and that is along the deep and rapidly descending chasm through which the torrent Kur finds its way; a chasm traversing the plateau in its greater width, from the basin of Ardahan to the Russian frontier. Its sides, and the rock in its neighbourhood to, in some places, a considerable extent, are volcanic.

My return route, from the Russian frontier near the well-known river Phasis, now the Rion, to Trebizond, lay along the coast; thus affording me excellent opportunities for studying on the northern or sea-side the same Lazistan mountain-chain, which I had already, in some measure, examined on its mainland or inner slope. Rarely stratified, its formations are most often volcanic, or metamorphic, gneiss and shale, with granite above. But if the inner and sheltered side had shown, as I have already noticed, no direct trace of glacial action, still less could I expect to discover any such on the outer or sea slope. However this generalisation was interrupted by one remarkable exception.

High up in the Lazistan mountains, about half way between Trebizond and Batoom, is perched the almost inaccessible district of Hamshun, a highland region tenanted by a colony of wholly different origin from the Mingrelian population around them, namely Armenian, though now all professing, not over-zealously, the Mahometan system. How or when they came there, no record tells. This district I resolved to visit; and three days of such breakneck scramble as even Turkish mountain-tracks had never before afforded me, brought me into the very centre of Hamshun.

Here, at the modest height of 6,900 feet above the sea, I stood on a granite-strewn plateau, thinly green with grass, sheltered from the sea by a tolerably lofty series of peaks on the N.W.; and backed to the S.E. by the tremendous jagged cliffs, blackish granite dashed with white snow streaks, else naked in all their savageness, but known by the uncouth names of Onoot Dagh, Alti Parmak Dagh, Jamookh Dagh; and, towering over all in startling resemblance to the Alpine Matterhorn, only more fantastic if possible, in its precipitous isolation of peak, Verehembek Dagh, rising full 12,500 feet above the sea, from which it is visible at a distance of about a hundred miles; a natural and unmistakeable beacon to the sailor. The plateau itself was about forty miles in length; and irregular in breadth; its surface too mounded, and often jotted over with boulders. But just as my track led near under the base of Verehembek, at an altitude of 8,300 feet, it crossed a large broad moraine, descending from the higher slope, and having its base in a broad bare valley not far below; thus indicating that here too, at the highest and widest part of the Lazistan chain, perpetual ice had once existed in sufficient quantity to furnish at least one glacier. But, if warrantable conclusions can be drawn from a single instance, the limited ice-cap of the Hamshun highlands extended no farther down than 8,500 feet at most, perhaps 9,000; thus differing by a line of one to two thousand feet from the glacial covering of the inland range.

What correctness there may be in this as in my other conjectures, I, of course, cannot well estimate: but I have now recorded the chief phenomena of this nature noted by me in these regions; it is for those more versed in such matters than myself to read their meaning aright.

Of the volcanic phenomena in the Lazistan or coast-chain, I shall say nothing here; that subject requiring, from its very copiousness, to be treated apart. But there is one fact connected with it worth noticing, as a corollary to what I have written; though a mere notice is all that can be given it for the present. It is, the elevation or depression of the south-east of the Black Sea coast.

In a former paper I remember having remarked that, judging by the actual position of an old river bar, as also by the height of certain cavernous excavations in the neighbouring cliffs, I am inclined to think that the coast near Trebizond itself has been raised to an elevation of about twelve, perhaps fifteen feet in post-glacial times. Having now ridden along the entire shore up to the mouth of the Phasis, I remark that the traces of similar uprising during, certainly not earlier than, the same period, as written on cliffs now a considerable way inland, on estuaries evidently prolonged, and on crags, still as before, coming sheer down into the sea, but wave-marked higher up than the possibility of the most violent storm could now effect; all these would seem to indicate that the same rising has been continued along the entire easterly line of coast, though not to an equal degree; the greatest elevation appearing to have taken place exactly at the south-eastern angle of the Euxine, near Batoom, from which point east and north it would gradually have diminished. West of Trebizond again it distinctly,—if traces of the kind mentioned be not misleading,—diminishes; till at, and to some distance west of Cape Jason it not only ceases, but is exchanged for a depression of the coast several feet, eight or ten seemingly, below its former level. Farther west again a slight rise would appear to have taken place; but allowance must be made for the effects of currents, which are very strong all along the coast.

Trebizond, Oct. 3 W. GIFFORD PALGRAVE

THE SOURCE OF SOLAR ENERGY

ALL incandescent bodies shrink rapidly if permitted to radiate freely, the rate being nearly proportional to the degree of incandescence. The enormous temperature maintained at the surface of the sun must therefore produce rapid shrinking, although we do not know the rate by actual observation. We know, however, what amount of mechanical energy the sun parts with in a given time, and we know the size and the specific gravity of the solar mass.

Demonstration is not needed to prove that motion of the particles within a spherical body towards the centre caused by attraction, develops a certain amount of mechanical energy resulting in the generation of heat within the mass. Nor is it necessary to show that the fixed relation between heat and energy enables us to determine the extent of contraction produced by gravitation, during cooling, if we can ascertain the amount of heat radiated in a given time by a sphere of known size and specific gravity. With reference to the sun, the elements thus specified are of the following magnitudes :—Heat radiated per minute, 312,000 thermal units from one square foot of surface; diameter, 852,584 miles; specific gravity, 0·250 compared to that of the earth, or 5·50 × 0·250 = 1·37 of water. Hence assuming that the mass is homogeneous, the weight of one cubic foot of the matter composing the sun will be 62·5 × 1·37 = 85·6 pounds. It will be seen presently that, in case the sun's mass is not homogeneous, the want of homogeneity will not materially affect the question of attraction and the resulting energy. At first sight it would appear that no probable amount of contraction of the sun could develop by gravitation towards the centre an amount of dynamic energy of 312,000 × 772 = 240, 864,000 foot-pounds per *minute* for each square foot of the solar surface. Yet, so vast is the mass contained in a spherical pyramid, the base of which is one square foot and whose length is equal to the sun's radius, that a very small longitudinal contraction suffices to develop by gravitation towards the sun's centre the stated enormous dynamic energy. It will be readily understood that the energy developed by the shrinking of a spherical pyramid, the sides of which are sectors of the great circle of the sun, will represent accurately the energy produced by the shrinking of the entire mass. And, in view of the great dimensions of the sun and the formidable array of figures involved in the computation of the energy exerted within the entire sphere, the advantage of considering only the mass covered by a single square foot of the solar surface will be evident. Let *IKS*, Fig. 1. represent the great circle of the sun, $a\,m\,a'$ the spherical pyramid referred to, and Fig. 2 the said pyramid drawn to a larger scale, its axis being divided into ten equal parts. It is proposed to ascertain what extent of longitudinal contraction of the spherical pyramid $a\,m\,a'$ is necessary to produce an amount of dynamic energy corresponding with that developed by the radiation from one square foot of the solar surface in a given time. The investigation will be somewhat facilitated if we compute the amount of energy developed by a definite contraction of the sun's radius, say one foot. Let us therefore suppose that $a\,a'$, the distance of which is $\frac{852,584}{2} \times 5,280 = 2,250,821,760$ feet from m, has fallen through a space of one foot, the intermediate points b, c, d, &c., participating proportionably in the fall. Assuming that the solar mass remains homogeneous during the contraction, it follows from Newton's demonstration ("Principia," lib. i. prop. lxxiii.) that since a particle just within the circumference of the sphere at a is ten times farther from the centre m than a particle at l, the former will be attracted towards m with ten times greater force than the latter. It will be readily perceived that, for a given movement towards the centre, the quantity of matter put in motion at a will be greater than at l, in the ratio of the squares of $a\,a'$ and $l\,1$, or 100 : 1. Hence, in accordance with the demonstration referred to, a given radial depth of the solar mass at a will exert a force towards m

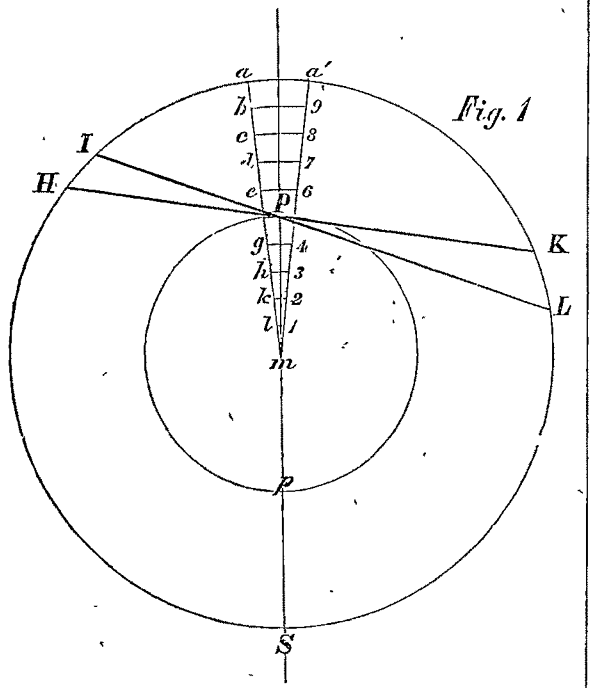


Fig. 2

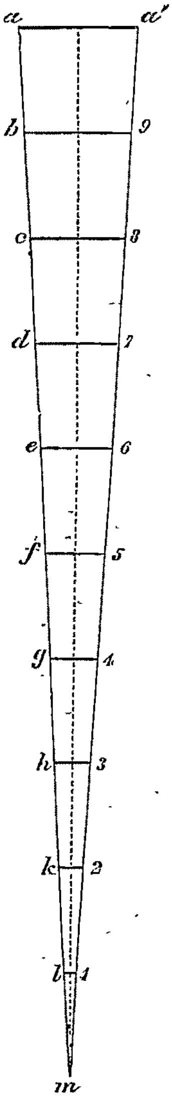
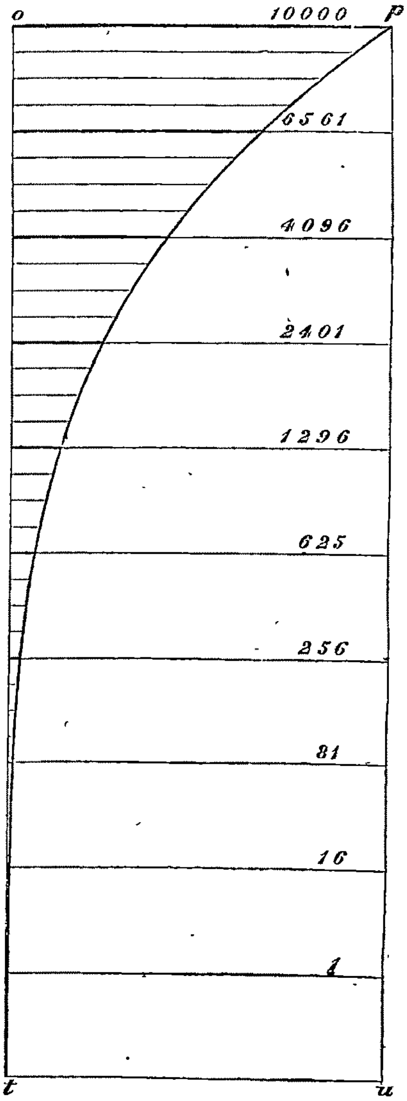


Fig. 3



10 × 100 = 1,000 times greater than an equal radial depth at l. But, in computing the dynamic energy developed by the shrinking of the sun, it must be borne in mind that a particle at a falls through a distance ten times greater than a particle at l. The length of the ordinates of the curve $p\,t$, Fig. 3, representing the ratio of dynamic energy developed at the respective distances from the sun's centre, has been calculated accordingly. A cursory examination of Fig. 2 can scarcely fail to lead to the conclusion that the mass composing the smaller sections of the spherical pyramid towards the centre of the sphere, will be attracted by the larger mass composing the sections towards the circumference. Newton has disposed of this question by a geometrical demonstration which, considering the form of the attracting mass, and the extreme complication arising from the varying direction and unequal magnitude of the attracting forces, may be regarded as one of the most elegant of his masterly demonstrations of important propositions and theorems. It will be evident on reflection that, unless it can be proved that a particle at P is not attracted by any portion of the mass contained within the outer spherical superfices IKS and the interior spherical superfices $P\,p$, the mass composing the sections near the base of the spherical pyramid will exert the disturbing attraction before alluded to. Our demonstration of the energy produced by the attraction of the matter within the sun, during shrinking, falls to the ground, unless it can be shown that every particle composing the spherical pyramid is in perfect repose as regards the attraction exerted by exterior particles. The great geometer thus establishes that repose :—Let $HIKL$ be a spherical superfices, and P a corpuscle placed within.* Through P let there be drawn to this superfices the two lines HK, IL, intercepting very small arcs HI, KL; and because the triangles HPI, LPK are homogeneous, those arcs will be proportional to the distances HP, LP; and every particle at HI and KL of the spherical superfices, terminated by right lines passing through P, will be in duplicate ratio of those distances. Therefore the forces of these particles exerted upon the body P are equal between themselves. For the forces are as the particles directly, and the squares of the distances inversely. And these two ratios compose the ratio of equality. The attractions, therefore, being made equally towards contrary parts, destroy each other. And, by a like reasoning, all the attractions through the whole spherical superfices are destroyed by contrary attractions. Therefore the body P will not be anyway impelled by those attractions.

Referring to Fig. 3, let us recollect that the ordinates of the curve $p\,t$ do not indicate the force exerted by mere attraction. As already stated, their length represents the dynamic energy developed at definite distances between the centre and the circumference of the sphere. The energy actually produced is represented by the superficies $o\,p\,t$, while the rectangle $o\,p\,u\,t$ represents the energy that would be called forth if the force exerted at every point of the axis of the spherical pyramid were the same as that exerted at $a\,a'$. Our space will not admit of introducing the calculations by which the energy represented by the ordinates of the curve $p\,t$ have been computed. It will be proper, however, to call attention to the fact that the energy exerted at each of the divisions of the base line $o\,t$ is definite; hence the length of the ordinates is exact. Calculations based on the data thus furnished show that the superficies $o\,p\,t$ is 0·20015 of the superficies $o\,p\,u\,t$.

We have before stated that the want of homogeneity of the solar mass will not materially affect the amount of energy developed by the gravitating force during the sun's shrinking. Referring to the several figures, it will be seen that the energy exerted at a point half way from m, viz., ordinate 5, is 0·0625, or $\frac{1}{16}$ of that exerted at $a\,a'$; and that the energy developed by the mass contained within the spherical pyramid $f\,m\,5$ amounts to only $\frac{1}{31}$ of that developed by the gravitation of the mass contained within the spherical pyramid $a\,m\,a'$. Now the volume of the spherical pyramid $f\,m\,5$ represents that of a sphere the diameter of which is one half of the sun, while the spherical pyramid $a\,m\,a'$ represents the volume of the entire solar mass. The energy resulting from the gravitation of the central spherical mass $P\,p$ being thus only $\frac{1}{31}$ of the energy exerted by the spherical mass IKS, it will be perceived that the degree of density of the matter towards the sun's centre will not materially affect the result of our calculations founded on perfect homogeneity.

We may now proceed to ascertain the amount of dynamic energy produced by the assumed shrinking of the axis of the spherical pyramid $a\,m\,a'$. Having already demonstrated that the said energy will be 0·20015 of that produced by the gravitation of a homogeneous mass, the section of which is one square foot extending from the surface to the centre, it only remains to determine the weight of one cubic foot at the surface of the sun. The specific gravity of the solar mass being 85·6 pounds per cubic foot, while the sun's attraction is 27·2 times greater than terrestrial attraction, the weight of one cubic foot at the solar surface will be 27·2 × 85·6 = 2328·3 pounds. Multiplying this weight by the sun's radius expressed in feet, we have, 2328·3 = 2,250,821,000 = 5,240,633,000,000, which product, multiplied by 0·20015, shows that the gravitating energy of the matter contained in the spherical pyramid, exerted during a longitudinal contraction of one foot, amounts to 1,048,912,000,000 foot pounds. Dividing this latter product by the solar energy per minute, already stated, we find that 4355 minutes; = 3·024 days will elapse before the energy produced by constant solar radiation equals the gravitating energy exerted during the shrinking of one foot of the solar radius. The length of one year, 365·25 days, being divided by 3·024, we learn that the annual shrinking of the sun's radius amounts to 120·7 feet. The foregoing figures prove that, notwithstanding this apparently great contraction, a period of 1864 years is necessary to diminish the sun's diameter $\frac{1}{10{,}000}$. It hardly requires explanation that this result is reached by dividing the sun's diameter by 10,000 times the stated annual shrinking.

Helmholtz, in accordance with Laplace's remarkable nebular hypothesis, asserts that the continuation of the original condensation of the matter composing the sun develops an amount of mechanical energy capable of generating sufficient heat to make good the present solar emission. According to his calculations, the sun's diameter will be reduced $\frac{1}{10000}$ in the course of 2,000 years. The practical data assumed by the eminent physicist being less accurate than those upon which our calculations are based, the discrepancy regarding time, 2,000 years against 1864 years, necessary to effect the stated shrinking of the sun's diameter, may be satisfactorily explained. It will be well to observe that the intensity of the radiant heat will not diminish with the diminished size of the sun. On the contrary, for a given area of the solar surface, the dynamic energy produced by a given rate of shrinking will be increased, since the mass remains the same, while the attraction is inversely proportional to the square of the distance from the centre. But the *rate* will diminish with the contraction of the sphere; hence a shrinking of $\frac{1}{10}$th of the sun's diameter, instead of occupying 1,000 × 1864 = 1,864,000 years, will require somewhat more than 2,000,000 years. At the end of that period the gravitating energy will continue to develop, as at present, an amount of dynamic energy represented by 312,000 thermal units per minute for each superficial foot; but the radiating surface, *i.e.*, the area of the solar disc, will have diminished in the ratio of 10^2 to 9^2.

The present maximum temperature produced by solar radiation on the ecliptic when the earth is in aphelion, being 67·2, while the intensity of radiant heat diminishes as the area of the radiating surface, it follows that, at the end of 2,000,000 years from the present time, the tropical solar intensity will be reduced to $\frac{9^2 \times 67{\cdot}2}{10^2} = 54{\cdot}4^\circ$, unless Prof. Tyndall's opinion is correct, that the earth, in common with the other planets, must "creep in, age by age, towards the sun."* But the pace is no doubt so slow that our calculations will not be seriously affected; hence, applying the foregoing demonstrations to the past, it will be seen that the temperature called forth by solar radiation 2,000,000 years ago must have been, owing to the greater diameter of the sun at that period, about $\frac{11^2 \times 67{\cdot}2}{10^2} = 81^\circ$ within the tropics. Now we are justified in assuming that the increased evaporation of the sea, and the consequent humidity of the atmosphere, modified the stated solar intensity, calling forth the luxuriant flora of past ages, which geology has made us acquainted with. The computed diminution of solar intensity, 67° − 54° = 13°, during the next 2,000,000 years will probably be deemed extravagant by those who do not bear in mind that the computation must be based on the assumption that a constant power is being exerted during the stated period capable of developing, as at present, the stupendous energy of 240 millions of foot-pounds in a single minute, for each square foot of the surface of a sphere whose diameter exceeds 850,000 miles. This inconceivable amount of work cannot be performed with a less expenditure than the motive energy developed by the fall of a mass equal to the mass contained in the sun, the weight of which is nearly a thousand times greater than the weight of all the planets of the system. Obviously a *continuous* development of such an amount of energy is physically impossible, since there is a *limit* to the distance through which the weight can fall. Now the foregoing demonstration enables us to determine the said limit, with sufficient exactness to prove that although the efficiency of the great motor, during the past, may be measured by hundreds of millions of years, its future efficiency will be of comparatively brief duration.

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* Sir Isaac Newton, in his demonstrations relating to spherical bodies, supposed these to be composed of an infinite number of spherical superficies the thickness of which he thus defines :—" By the superficies of which I here imagine the solids composed, I do not mean superficies purely mathematical, but orbs so extremely thin that their thickness is as nothing, that is, the evanescent orbs of which the sphere will at last consist, when the number of the orbs is increased, and their thickness diminished without end."

* See "Heat as a Mode of Motion," p. 409

Statements relating to the permanency of solar heat, based on the assumption that no diminution has been observed during historic times, have no weight in view of our demonstration showing that a shrinking of $\frac{1}{10}$ of the sun's diameter can only reduce the intensity from 81° to 67°·2, difference = 13°·8, in the course of two millions of years. This period being 500 times longer than "historic times" say 4,000 years, it will be seen that the diminution of the temperature produced by solar radiation, has not exceeded $\frac{13·8}{500} = 0·027$, or $\frac{1}{37}$ deg. Fah. since the erection of the Pyramids.

It will be proper to observe, before concluding our brief investigation of the source of solar energy, that, the development of heat by the shrinking of the sun, however fully demonstrated, leaves the important question unanswered: how is the heat generated by gravitation within the mass transmitted to the surface? If the matter within the sun is a perfect conductor of heat—a very improbable supposition—that fact alone furnishes a satisfactory answer. Imperfect conductivity, on the other hand, calls for, other means of transmitting the energy from within, to make good the enormous loss caused by the external radiation. Besides, the falling of the crust at the rate of ten feet per month, attended by increase of internal pressure, and probably ejection of gaseous matter, together with the disturbance occasioned by contraction at the surface, disclose a mechanism of startling perplexity. But the parting with 312,000 thermal units for each square foot of the solar surface, involving an expenditure of kinetic energy fully 240,000,000 foot-pounds per minute, cannot be made good in that brief space of time, unless the sun shrinks at the rate ascertained by our calculations.

The development of solar energy in accordance with the combustion hypothesis (lately resuscitated by M. E. Vicaire) merits no consideration, while careful investigation has proved the meteoric hypothesis to be untenable. It must be admitted, however, that the mechanical difficulties alluded to, especially those relating to the means of transmitting the heat to the surface of the sun, any temporary local derangement of which must be productive of dark spots for a time, are of such a nature that the absolute certainty of solar radiation may be questioned; nor is evidence wanting to show that the solar mechanism is liable to derangement. History informs us that the great luminary has, during several seasons, partially failed to perform its functions. Herschel states, in his "Outlines of Astronomy," that "in the annals of the year A.D. 536 the sun is said to have suffered a great diminution of light, which continued fourteen months. From October A.D. 626 to the following June a defalcation of light to the extent of one-half is recorded; and in A.D. 1547, during three days, the sun is said to have been so darkened that stars were seen in the day-time." Again, the glacial periods, the ascertained abrupt termination and recurrence of which puzzles the geologist, point to periodical derangement of the solar mechanism in past ages.

J. ERICSSON

EXTRAORDINARY WHIRLWIND IN IRELAND

IN a letter to the *Belfast News-Letter*, Mr. C. J. Webb describes an extraordinary whirlwind which occurred in the district around Randalstown, about six miles N.W. of Antrim, near the shores of Lough Neagh, on the 25th of August last. The same phenomenon was witnessed about an hour and a half earlier the same evening at Banbridge, about seven miles S.W. of Dromore. It was first seen near Randalstown about 5 P.M., between that place and Toome, moving rapidly up Lough Neagh from the south, and presenting the appearance of a defined column of spray and clouds, whirling round and round, and not many yards in breadth, while at its base the water was lashed into a circle of white foam. It was next heard of in the neighbourhood of Staffordstown, about a mile from the lake, where it partially unroofed two houses, and damaged any trees or crops which happened to be in its course. From this point it travelled in a straight line for Randalstown, about three miles distant. It passed across a field close to Mr. Webb's house, levelling eight haystacks, and carried a considerable part of the hay up into the air out of sight. The breadth of the storm could be accurately ascertained at this point, and must have extended about thirty yards, as stacks remained unruffled at either side, while those between were thrown down and carried away or scattered about. Everything it lapped up was whirled round and round, and carried upwards in the centre, while dense clouds seemed to be sucked down on the outside, and came close to the earth. Both before and after there was lightning and incessant peals of thunder; but there was no rain till some time afterwards. Mr. Webb next observed its track in a hollow, some three hundred yards further on, where it knocked down a haystack, and then plunged into a wood of fine old Irish oaks. Here it tore numerous branches and limbs from the trees, carrying some along with it, and throwing others to the ground. One noble tree in the centre of the wood seems to have been a peculiar mark for its vengeance, although it would have been completely protected from any ordinary storm, owing to its position. It next passed across a corner of Shane's Castle demesne. Some who were at a short distance from this point describe its approach as causing considerable alarm. It was accompanied by a wild rushing noise, and the crashing of the trees and branches could be heard becoming louder and louder as it advanced. It crossed the valley over the railway viaduct, close to Randalstown, fortunately avoiding the village. It here presented the appearance of a vast whirling column of leaves and branches, mingled with clouds which looked like smoke.

The railway station next suffered, innumerable slates and two and a half cwt. of lead being torn from the roof in an instant. A great part of the railings surrounding the gardens was torn up, and an iron bar one inch thick, belonging to the gate, was bent to an angle of sixty degrees. A small shed at the rear of the station was unroofed, rafters and slates being hurled to the ground. What will give some idea of the excessive pressure of the wind, is the fact that three boards of the flooring of the waiting-room were forced up, owing to the wind finding an entrance to a cellar underneath, though the only aperture was a round hole about one foot in diameter. All this was the work of a few moments. The storm then passed away, leaving comparative calm behind. It next crossed an adjacent bog, scattering the turf in all directions. The last place Mr. Webb heard of its having visited was a farm house about three miles from Randalstown, between Antrim and Ballymena. It would be interesting to ascertain whether it travelled across to the sea-coast.

NOTES

THE British Association Committee on Mathematical Tables, of which Prof. Cayley is the chairman, has determined to tabulate the Elliptic Functions, or more accurately, the Jacobian Theta Functions, which are the numerators and denominators of the former, and their logarithms. The tables, which are of double entry, will therefore give eight tabular results for each

8,100 arguments; besides certain other quantities, depending only on the modulus, that will be added. Forms have been printed, and the calculation has already been commenced. The Elliptic Integrals (the inverse forms to the Functions) were, as is well known, calculated by Legendre, and published in his "Traité des Fonctions Elliptiques, 1826." It is unquestionable that the Elliptic Functions are the most widely used transcendents in analysis that have not yet been tabulated, and it is believed that the tables will be found very generally useful in all the mathematical sciences. The great labour has no doubt alone prevented any previous attempt. The work proposed by the committee will, when completed, be most likely the largest piece of numerical computation, with general application throughout the whole of mathematics, that has been undertaken since the original calculation of the logarithms of numbers and trigonometrical functions of Briggs and Vlacq, 1620–1633.

Dr. Frederick Welwitsch, the well-known African explorer and botanist, died at his residence in London on Sunday, the 20th inst., in the 66th year of his age. A native of Carinthia, Dr. Welwitsch studied medicine at Vienna, and early devoted himself to botanical pursuits. When on a visit to Portugal, he was induced to take up his residence at Lisbon as Director of the Botanical Gardens there, and in 1853 was despatched by the Portuguese Government to Angola to investigate the natural history of that region, where he remained from 1853 to 1861. His collections of the vegetable productions of that country are unrivalled in extent and completeness, have established a new region in geographical botany, and have been copiously used in the compilation of the two volumes already published under the auspices of the authorities at Kew of the "Flora of Tropical Africa." Dr. Welwitsch was not himself an extensive writer, but the number of species new to science discovered by him and described by others is very large, among the most remarkable of which is one of the most extraordinary vegetable productions known, dedicated to him by Dr. Hooker, the *Welwitschia mirabilis*. He was also an accomplished zoologist, and his entomological collections are of great extent and value. It is understood that the British Museum will have the first option in the purchase of the most valuable part of his collections.

The death on Monday, 21st inst., is announced of the physicist, M. Jacques Babinet, the academician, at the age of 78 years. He was elected to the Academy in 1840. Another member of the Academy, M. Puiseux, has also just died.

Sir John Lubbock and Mr. Grant Duff are now travelling in Asia Minor; and it is expected they will bring home some very important and interesting information on the pre-historic remains of that region, an almost untried hunting-ground.

The *Gardener's Chronicle* states that it is proposed that the sum of 48,000*l.* shall be included by the French Government in the Budget of 1873 for the commencement of the entire rebuilding of the museums and conservatories of the *Jardin des Plantes*, a move which has been long in contemplation. In addition, the vote for civil buildings for 1873 includes a sum of 8,000*l.* for the construction of laboratories of chemistry and zoology in the Museum for the *Ecole des Hautes Etudes*, and for the completion of the reptile house.

The Board of Trinity College, Dublin, has elected Dr. Benjamin M'Dowell to the Professorship of Anatomy and Surgery, and has resolved to found a new Professorship of Comparative Anatomy, endowed with 100*l.* a year and a portion of the fees for dissections. The professor will have to deliver eighteen lectures each year.

The Swiney Lectureship, which has just been vacated by Dr. Cobbold, will be filled up in February. It is a travelling Lectureship, open to Doctors of Medicine of Edinburgh University, and is tenable for five years.

The statue to the memory of Sir Humphrey Davy has just been erected at Penzance, his native place. The statue, which cost 600*l.*, is a colossal one, and stands on a massive granite pedestal in front of the Post Office, and a few yards from the house in which the great chemist, philosopher, and inventor was born.

We learn from the *Mechanics' Magazine* that a committee of the Derby and Chesterfield Institute of Engineers has been appointed to consider the possibility of erecting a memorial hall, to cost 20,000*l.* to 30,000*l.*, in memory of George Stephenson.

In reference to Mr. J. R. Hind's letter to the *Times*, printed in its issue of Oct. 19th, on the subject of the probable existence of a planet revolving round the sun within the orbit of Mercury, we propose to revert to the subject as soon as Mr. Hind has further discussed the subject; as we learn that in consequence of errors in some of the calculations made by some who have previously inquired into the subject, a revision of some of the results announced in the letter in question is necessary.

An admirable article has appeared in *Engineering*, under the heading, "Great Britain in Formâ Pauperis." The burden of the writer is the parsimonious, nay, even ungratefully insolent manner, in which the neighbourly request of Austria for a reciprocity of assistance (such as she and other nations afforded to England in 1851 and 1862) in the forthcoming Universal Exhibition at Vienna has been met by the Government and the Treasury. It appears, that whereas France, torn and bleeding at every pore, votes 60,000*l.*, her conqueror, Prussia, an equal sum, with a supplementary vote, Italy the same sum, Spain 1,200,000 reals, the minor states of Europe in proportion, even little Switzerland voting 16,000*l.*, Great Britain, wealthy and powerful, the ancient friend and ally of Austria, who has contributed to her exhibitions over 100,000*l.*, votes the noble, magnanimous sum of six thousand pounds sterling! We fully sympathise with the indignant comments of our contemporary at the lamentable parsimony for which our Government has made us responsible.

The *Gardener's Magazine* announces its full adhesion to the views enunciated in our article on the potato disease, that it is in its origin cosmical, and probably connected with the great cycle indicated by the recurrence of sun-spots.

The total number of entries at the various medical schools of London for the session just commenced is 1496, of which 476 are of new students, the former number being 21, and the latter 8 in excess of those last year. Guy's and University College Hospitals occupy the first place, each with 83 fresh entries. Westminster brings up the rear with 4.

The prizes of the Charles Science and Art School, Plymouth, were distributed on Friday, Oct. 21, by Sir Massey Lopes, Bart. M.P., who spoke very encouragingly of the success of the schools.

Mr. Thomas Webster, Q.C., F.R.S., will read a paper before the members and friends of the London Association of Foremen Engineers and Draughtsmen "On the Promotion of Practical Science and Technical Education by Museums of Inventions established and maintained by the Surplus of the Inventors' Fee Fund," at the meeting to be held on Saturday, the 2nd of November, at the City Terminus Hotel, Cannon Street, at 8.30 P.M.

The Council of the Institution of Civil Engineers invite communications dealing in a complete and comprehensive manner with such subjects as (*a*) Account of the Progress of any Work in Civil Engineering, as far as absolutely executed —Smeaton's Narrative of the Building of the Eddystone Lighthouse may be taken as an example; (*b*) Descriptions of distinct classes of Engines and Machines of various kinds; (*c*) Practical Essays on Subjects allied to Engineering, as for instance, Metallurgy; and (*d*) Particulars of Experiments and Observations connected with Engineering Science and Practice. A list of thirty seven special subjects recommended for competition is appended. For approved original communications, the Council will be prepared to award the premiums arising out of special funds devoted for the purpose.

THE Crystal Palace Company's school of Art, Science, and Literature, has issued its prospectus for the thirteenth session, 1872-73, of classes for gentlemen, conducted by eminent professors and teachers.

THE following are the Science Lectures for the People to be given this winter at Manchester:—The first on Tuesday, Oct. 29, by Prof. Roscoe, F.R.S., On the Rainbow; and these other lectures will follow:—Prof. Geikie, F.R.S., On the Ice Age in Britain; Prof. Balfour Stewart, F.R.S., The Sun and the Earth; Prof. Clifford, On Atoms; Prof. Barrett, On Faraday's Electrical Discoveries; Dr. J. H. Gladstone, F.R.S., The Life of Faraday; Mr. William Pengelly, F.R.S., Prehistoric Man. The fee for each of these lectures, as before, is one penny! Many people will wish they lived in Manchester.

THE following lectures will be delivered in Gresham College, Basinghall Street, E.C., by E. Symes Thompson, M.D.; On Draughts, Friday, Nov. 8; On Mineral and Vegetable Tonics, Monday, Nov. 11; On Prescriptions, Tuesday, Nov. 12. The lectures are illustrated by diagrams and experiments, are free to the public, and commence each evening at seven o'clock.

THE following lectures are announced to be delivered in connection with the Torquay Natural History Society:—Introductory Address, by Dr. Wilks (President), Nov. 4. The Fertilisation of Flowers, by Dr. Wilks, Nov. 25. "Natural Selection," by Dr. Wilks, Dec. 2. Museums and Our Museum, by the Rev. T. R. R. Stebbing, M.A., Dec. 9. Fossils as characteristic of Strata, by J. E. Lee, F.G.S., F.S.A., Dec. 16. The Share of the Italians in the Progress of the Natural Sciences, by Signor Olivieri, Lit. and Phil. Doctor, Jan. 27, 1873. The Unity and Progress of Man, by J. B. Paige Browne, March 3. Monte Rosa—its Peaks, Valleys, and Glaciers, by Dr. Wilks, March 17. Teleology, by Rev. T. R. R. Stebbings, M.A., March 24. Curiosities of Natural History, by Dr. C. Paget Blake, April 7. Aerostation, by W. Froude, F.R.S., April 21.

IN addition to our announcements of last week, Mr. Van Voorst announces as follows:—Mr. W. Saville Kent, of the British Museum, is engaged upon a new "Manual of the Infusoria:" the treatise will be devoted entirely to the Ciliate, Flagellate, and Suctorial Protozoa, to the exclusion of the Desmids, Diatoms, Rotifers, and other foreign organisms comprised under the above title by Ehrenberg, Pritchard, and other writers; the Rev. Thomas Hincks is preparing for publication "A History of British Polyzoa" with figures of all the species, uniform with his "Hydroid Zoophytes;" "The Birds of the Humber district," by John Cordeaux of Great Cotes, Ulceby, is in the press; a second edition, with new plates and additions, of "Falconry in the British Isles," by Capt. Salvin and Wm. Brodrick, is in preparation.

THE Zoological Society has just issued a revised list of the valuable animals now or lately living in the Society's Gardens. It contains a list of nearly 500 Mammalia, upwards of 1,000 Aves, and nearly 300 Reptilia, Batrachia, and Pisces, with their habitats and dates of acquisition, and is illustrated by 30 very well-executed woodcuts. As the list is published at the very low price of 2*s.*, it ought to be in the hands of every one who is in the habit of using the Gardens.

PROF. MIGUEL COLMEIRO, director of the Botanic Garden at Madrid, announces the publication of the second edition of a "Treatise on the Elements of Botany, organographic, physiological, systematic, and geographical," in two vols., with numerous illustrations. Prof. Colmeiro is the author of thirty separate papers on botanical subjects.

A SECOND edition is announced as in the press of Gardiner's "Flora of Forfarshire," edited by Mr. John Sadler, author of the "Flora of Edinburgh." Many important additions will be made to the original work, in which the editor will be assisted by resident botanists, and the author's valuable notes on the different species, and on various localities of special interest, will be retained.

JOHN HEYWOOD'S recently-published School Atlas is a marvel of cheapness. It contains twelve coloured maps about 10 in. by 8 in., of Europe, the two hemispheres, England and Wales, the British Isles (physical), Scotland, Ireland, Asia, Africa, North and South America, and Australia; and the price is Sixpence!

THE following we take from the *School Board Chronicle*:—"Some curious statistics have been published, establishing a suggestive comparison between the expenses of education and police supervision in the cities of St. Petersburg, Berlin, and Vienna. With regard to education, the expenses of the Russian capital are estimated at one per cent. of the total budget; Vienna stands as high as nine per cent.; and Berlin reaches thirty-one. Costs of philanthropic institutions are represented by the proportions of Berlin, twenty-two; Vienna, fifteen; and St. Petersburg, nine per cent. Of course, the ratio becomes inverted when we turn to the expenses of the police force. Here we find Prussia down for seventeen, Austria for twenty-one, and Russia for fifty-one (figures of comparison). Berlin employs one policeman for every 495 of its inhabitants, Vienna one for every 416, and St. Petersburg one for every 210. The practical teaching of these statistics is, that while Berlin pays twice as much for schools as for prisons and police, Vienna pays two and one-third times less, and St. Petersburg fifty times less."

THE same journal prints the following item of information:—"Reports concerning the four Prussian academies for the scientific pursuit of agricultural knowledge inform us that these institutions have been frequented during the past summer term by 173 students. Of these 63 were newly matriculated, and 10 unmatriculated. Classified according to their nationality there were 117 from Prussia, 13 from other German States, and 43 foreigners. In order of academic population Proskau stands first with 63 students; then comes Poppelsdorf, near Bonn, with 43; after that the institution at Berlin with 37; and last of all the Academy at Eldena with 30 students.

THE following, in reference to education in Greece, is again from the same journal:—"From 1835 to 1869, the number of students at the University of Athens had increased from 35 to 1,205; the number of gymnasia in Greece, which was 3 in 1835, had risen to 16 by the year 1866. During the same period of time the number of secondary schools had increased from 21 to 189, and that of the pupils frequenting them from 2,500 to 7,300; within 33 years, also (1833-66), the national elementary schools had increased from 17 to 1,070, and the scholars from 8,000 to 65,000. Among the secondary schools there were, in 1869, 6 institutions for girls, numbering 680 pupils. As a sorrowful set-off to such cheering news, it must be mentioned that there are still in Greece 240,000 children and youths who receive no education whatever; that is to say, more than three times the number of those who frequent the schools.

INTERNATIONAL METRIC COMMISSION

THE following methodical statement of the resolutions come to by the International Commission on Weights and Measures at its recent meeting at Paris was presented to the French Academy of Sciences by M. Tresca, one of the secretaries of the Commission.

I. *In reference to the Metre*

1. As a starting-point for carrying into effect an international measure, the Commission takes the metre of the Archives, in its present condition.

2. The Commission declares that, considering the actual condition of the platina measure of the Archives, it thinks the marked or line metre (*mètre à traits*) may be deduced from it with certainty. Nevertheless, this opinion of the Commission requires to be confirmed by the different processes of comparison which can be employed in this investigation.

3. The proportion (*équation*) of the International Metre will be deduced from the present length of the metre in the Archives, determined according to all the comparisons which have been made by means of the processes which the International Metric Commission will be in a condition to employ.

4. While deciding that the new International Metre ought to be a line-metre (*mètre à traits*), of which every country will receive an identical copy made at the same time as the universal prototype, the Commission will feel bound afterwards to construct a certain number of standards marked by projections (*étalons à bouts*) for those countries which desire them; and the proportions of such metres to the new prototype *à traits*, will also be determined under the care of the International Commission.

5. The International Metre will have the length of the metre at zero (centigrade).

6. There will be employed for the manufacture of the metres an alloy composed of 90 parts of platina and 10 of iridium, with a margin (*une tolerance*) of 2 to the 100, more or less.

7. In manufacturing the measures, the ingot must be formed by a single casting by means of the processes used in the working of the known metals. The number and form of these measures will be determined by the International Commission.

8. These measures will be annealed for many days, at the highest temperature—notwithstanding that they are never likely to be subjected to anything but the most feeble strains—before taking them to be compared with the standard instruments.

9. The bars of platina alloy upon which the line-metres are to be traced, will have a length of 102 centimetres, and their transverse section will be represented by the model described in a note of M. Tresca.

10. The bars intended for the construction of the projection metre measures (*à bouts*), will have a similar transverse section, but symmetrical in the vertical direction, conformably to the special figure which represents it; the nobs or projections (*bouts*) will then be wrought with a spherical surface of one metre radius.

11. During all the operations which the standard metres must undergo, they will be supported on the two rollers (*rouleaux*) indicated by General Baron de Wrede; but, for their preservation, they will be placed in a suitable case.

12. Each of the International Metres ought to be accompanied by two mercurial thermometers, isolated, and carefully compared with an air-thermometer; it is deemed necessary that these thermometers should be verified from time to time by means of the air-thermometer.

13. The method of M. Fizeau will be employed to determine the dilatation of the platina alloy used in the construction of the metres.

14. The prototypes will be submitted to the processes by means of which the coefficients of the absolute dilatation of the complete metres can be best determined. These measures will be separately made, at five different temperatures at least, between zero and 40° centigrade.

15. The comparison of the prototypes with each other ought to be made at, at least, three temperatures comprised between these same limits.

16. The Commission decides that two apparatus be constructed, the one with a longitudinal movement for tracing these metres, the other with a transverse movement for their comparison.

17. The comparisons will be made by immersing new standards in a liquid and in air; but the standard of the Archives must not be immersed in any liquid before the end of the operations.

18. The tracing of the line or traced metres (*à traits*), and their first comparison with the metre of the Archives, will be for the first effected by means of M. Fizeau's process.

19. For the determination of the proportions of the various standards, there will be employed moreover all the means of comparison already known and approved, according to circumstances, either by actually bringing the different forms into contact, or by the method of Messrs. Airy and Struve, or by that of MM. Stamkart and Steinheil.

20. The relations between the Archive metre and the new International traced metre, as well as the relations between the other traced standards and the International Metre, will be determined by comparing the results of all these observations.

21. Operations will be performed, on the other hand, by setting out from the International Metre for the construction of the standards with projections (*étalons à bouts*), which may be asked for by various states.

II. *In reference to the Kilogramme*

22. Considering that the simple relation established by the authors of the metric system between unity of weight and unity of volume is represented by the actual kilogramme in a manner sufficiently exact for the ordinary uses of industry, and even of science; considering also that the exact sciences have no real need of a simple numerical relation, but only of a determination as exact as possible of that relation; and considering the difficulties which would result from a change of the existing unity of metric weight, it is decided that the international kilogramme will be derived from the kilogramme of the Archives in its present condition.

23. The International Kilogramme ought to be decided by weighing in a vacuum.

24. The material of the International Kilogramme will be the same as that of the International Metre, viz.: an alloy of platinum and iridium, as stated in No. 6.

25. The material of the kilogramme will be founded and cast in a single cylinder, which will afterwards be subjected to furnace heat and mechanical operations, such as will give to its whole mass the necessary homogeneity.

26. The form of the International Kilogramme will be the same as that of the kilogramme of the Archives, viz., a cylinder whose depth is equal to its diameter, and whose corners may be easily rounded.

27. The determination of the weight of the cubic decimetre of water ought to be made under the direction of the International Commission.

28. The balances which will be used for weighing ought to be, not only those which may be placed for the present at the disposal of the Executive Committee by institutions and men of science who possess them, but also a new balance constructed according to conditions of the greatest exactness.

29. The volumes of all kilogrammes will be determined by the hydrostatic method; but the kilogramme of the Archives will neither be placed in water nor in a vacuum before the end of the operations.

30. To determine the weight of the new kilogramme, in comparison with that of the Archives, in a vacuum, two auxiliary kilogrammes will be made use of, as nearly as possible of the same weight and the same volume as that of the Archives, according to the method indicated by M. Stas. Each of the new kilogrammes ought also to be compared in air with the kilogramme of the Archives.

31. When the International Kilogramme is constructed, all others will be compared with it, in air and in vacuum, for the determination of their proportions.

32. For this purpose is employed the method of alteration and that of substitution, with a counterpoise of the same material.

33. The corrections for losses of weight in air will be effected by means of the most precise and least disputed data of science.

III. *In reference to the carrying out of the Commission's Decision*

34. The making of the new prototypes of the metre and the kilogramme, the tracing of the metres, the comparison of the new prototypes with those of the Archives, as well as the construction of the auxiliary apparatus necessary to these operations, are entrusted to the care of the French section, with the concurrence of the Permanent Committee, provided in the following article.

35. The Commission has chosen from its members a Permanent Committee, which will do duty till the next meeting of the Commission, with the following organisation and powers:—(*a.*) The Permanent Committee will be composed of twelve members, belonging to different countries. Five of these members

onstitute a quorum : it will choose a president and secretary, nd will meet as often as it deems necessary, but at least nce a year. (*b.*) The Committee will direct and superintend execution of the decisions of the International Commission, reference to the comparison of the new metric prototypes with ach other, as well as the construction of balances and other uxiliary apparatus necessary for these comparisons. (*c.*) The ermanent Committee will perform the work indicated in (*b*) ith all appropriate means which may be at its disposal ; it will eet for the performance of its task at the International Bureau Weights and Measures, the establishment of which will be commended to the nations interested. (*d*) When the new ototypes will be constructed and compared, the Permanent mmittee will give a report of its work to the International mmission, which will sanction the prototypes before distribug them to the different countries.

36. The Commission suggests to Governments interested how at would be the utility of founding at Paris an International Inution of Weights and Measures, upon the following bases :— . The establishment would be international and declared tral. 2nd. Its seat will be at Paris. 3rd. It would be founded supported at the common cost of all the countries which ere to the treaty that might be made between the interested es for the creation of the establishment. 4th. The establisht will depend upon the International Metric Commission, will be placed under the superintendence of the Permanent mmittee, who will choose the director. 5th. The Internaal Bureau would serve the following purposes :—(*a.*) It will t the disposal of the Permanent Committee for the compariwhich will serve as a basis for the verification of the new otypes with which the Committee is charged. (*b.*) The servation of the international prototypes, in accordance with directions laid down by the International Commission. (*c.*) he periodical comparison of the international prototypes with the national standards and with the tests, as well as that of the standard thermometers, according to the rules laid down by the Commission. (*d*) The construction and verification of the standards which other countries may require in future. (*e.*) The comparison of the new metric prototypes with the other fundamental standards employed in the different countries and in ence. (*f.*) The comparison of standards and scales which be sent for its verification, either by Governments or by ntific societies, or even by mechanicians and servants. (*g.*) e Bureau would execute all the works which the Commission its Permanent Committee would require of it in the interest of metrology and the propagation of the metric system.

37. The Bureau of the Commission is required to apply to the French Government, and request it to be good enough to communicate diplomatically the views of the Commission concerning the foundation of an International Bureau of Weights and Measures to the Governments of all the countries represented in the Commission, and to invite these Governments to conclude a treaty to create harmoniously, and as soon as possible, such an International Bureau upon the bases proposed by the Commission.

IV. Concerning the means of Preserving the Standards and the Guarantee of their Invariability

38. The Commission is of opinion that the International Standard ought to be accompanied by four identical measures, maintained at a temperature as invariable as possible; another identical measure ought to be preserved, for the sake of experiment, at an invariable temperature in vacuo; it would take means to establish tests in quartz and beryl, to be compared at any time with the complete measure, in whole or by portions. (The other means are reserved.)

39. The Commission thinks that in the interest of geodesy the French Government should cause to be re-measured, at a convenient time, one of the new French bases.

All these resolutions were made by the Commission most harmoniously, and in a spirit of complete confraternity; all the votes were nearly unanimous.

BIRTH OF CHEMISTRY

III.

Practical Chemistry of the Ancients.—Metallurgy; gold, silver, electrum, copper, bronze, tin.

IN the preceding articles we have discussed such theories of the ancients as involve the conception of change of matter (notably the assumed transmutation of the elements), and which hence concern the early history of chemistry. Having done with theory, we have now to inquire to what extent the ancients were acquainted with practical chemistry, what metals or other elements were known to them, and what processes dependent upon chemical action. We do not, of course, use the term "practical chemistry" strictly in its present sense, because chemistry as a science was altogether unknown to the ancients. Some have indeed endeavoured to prove that the Egyptians must have been acquainted with the science, from the skill with which they used various metallic oxides for colouring glass; but we have no proof of this. Neither Herodotus, nor Pliny, nor Vitruvius, indicates any knowledge of chemistry as a science among either Egyptians, Greeks, or Romans. Pliny, in his celebrated "Natural History," has laboriously amassed all the practical science and pseudo-science which the ancients possessed, and we find no mention of either chemistry or alchemy. At the same time it is impossible that the Egyptians and Sidonians can have attained their marvellous skill in the manufacture and colouring of glass, and in the extraction and working of metals, without the acquirement of a considerable amount of knowledge of the properties of matter, and of certain chemical changes. But this knowledge could never be worked up into a comprehensive system; it resulted from the labour of artizans, and the gulf between the philosopher and the manipulator was both wide and deep. There could be no union of practice and theory. Between Herakleitos with his theory that fire is the primal element, the actuating force of the Universe, and the man who wrought metals never so deftly, who applied fire to the use and service of mankind, there was no sympathy, no reciprocal transference of ideas. To reason concerning the properties of matter with one's eyes shut was all very well, but to experiment with matter, to endeavour to determine the cause of such and such a change by experiment, was utterly unworthy of a philosopher. Anaxagoras is said to have made an experiment to prove that there is no vacuum. Aristotle found that a bladder of air weighed in air weighed more than the empty bladder (which, if the experiment be properly made, is by no means the case), and hence concluded that the air has weight. But these are solitary exceptions; the way to study Nature, if she is to be studied at all, is, they maintained, to apply the pure, unaided intellect to the study, and to keep mind and matter as distinct as possible. From all this it resulted that your workers in metals and in curious arts, your makers of glass and pigments, kept their knowledge of matter to themselves, as secrets to be handed down from father to son.

Seven metals were known to the ancients, viz., gold, silver, copper, tin, iron, lead, and mercury. The first six are mentioned by Homer, and appear to have been known from remote antiquity, while mercury was not known till a later date; it was, however, common in the first century B.C. The Greek word *μεταλλον*, whence *metallum* and *metal*, signifies *a mine*, hence it was applied to anything found in mines, notably metals; *μέταλλον* is connected with *μεταλλ'ω*, "to search for diligently."

Gold has been valued from the earliest ages, on account of the peculiarity of its colour, its lustre, and its unalterability in air. The metal is invariably found in the native state, that is, uncombined with other substances, hence no metallurgical operation is necessary for its extraction. It is very often met with in surface deposits, and in early times was undoubtedly far more common in alluvium and the beds of rivers than now. It would thus be easily extracted by washing, and the grains could readily be fused together into a mass. Gold mines formerly existed in Ethiopia, in which the gold was found in a matrix of quartz, like much of the Australian gold of the present day. These mines were worked by the Egyptians, who employed large gangs of slaves for the purpose. The quartz was crushed, and the gold obtained from it by washing. We find representations of gold washings, and the subsequent fusion of the metal, on Egyptian tombs, at least as early as 2500 B.C., that is to say, about the time of Joseph in Hebrew history. The woodcut (Fig. 1) on the following page is given by Sir Gardner Wilkinson, and is taken from a tomb at Beni Hassan : it represents gold washing, and the fusion and weighing of the metal.

It is obvious that the process is only indicated, and not accurately or minutely portrayed. Another form of furnace is depicted below (Fig. 2), and a blowpipe somewhat different from that shown in Fig. 1. The raised portion of the furnace is doubtless for the purpose of concentrating the heat upon the crucible, on the principle of the reverberatory furnace.

Gold once obtained was soon made into ornaments; very fine gold wire was used by the Egyptians for embroidery 3,300 years ago. Many of the Egyptian and Etruscan gold ornaments are very beautiful; we may notice particularly the gold myrtle wreath found in an Etruscan tomb a few years ago. The Egyptians also used gold for inlaying, and it was beaten into leaf and used for gilding as early as 2000 B.C. In the Odyssey the gilding of the horns of an ox about to be sacrificed is mentioned.

Silver, like gold, is often found native, and from several of its ores the metal may be extracted by the action of heat alone. It has been known from the earliest ages, and was used chiefly for ornaments and embroidery. Gold was used for money before silver, which was first known as "white gold." The oldest silver Greek coin is a coin of Ægina, and was, perhaps, coined in the eighth century B.C. But the oldest coins in existence are the *electrum* staters of Lydia. Electrum consists of about three parts of gold to one of silver. Probably the metals were first found in nature thus alloyed, and as no method of separating them was then known, they were worked up together. Electrum was so called from its resemblance as regards colour to amber (ἤλεκτρον), which received its name from ἠλέκτωρ, the sun. It will be remembered incidentally that the science of Electricity was so called by Gilbert of Colchester, because the attractive force was first observed in amber. Amber is mentioned more than once by Homer. Electrum as a metal is first mentioned in the *Antigone* of Sophocles. It was found naturally alloyed, as in the pale gold of the Pactolus, which contains a good deal of silver; and was also made artificially. Probably all very pale gold was called electrum; Pliny states that gold containing a fifth part of silver is called electrum. In the British Museum there are many coins made of this alloy.

Copper was in use before iron. It is, as is well known, usual to denote various early ages by the substances then used for domestic implements. Thus we have the "age of stone," the "age of iron," &c. The stone age is followed by the age of copper, this by the age of bronze, and the age of bronze by the age of iron. Homer wrote in the age of copper; the shield of Achilles is made of gold, silver, tin, and copper; the arms and implements and utensils of his heroes are of copper. Mr. Gladstone has argued at some length that by chalcos (χαλκος) Homer mea
copper, not bronze, as it is so often rendered. Chalcos is spok
of as a cheap and common metal, while tin was very scarce an
rare; and it is scarcely probable that so many things, even d
to the commoner utensils, could have contained ten or twelve
cent. of tin. Again, Mr. Gladstone points out that Hom
speaks of chalcos as ερυθρος, red, a term that could not apply
bronze; and he goes so far as to say, "If chalcos be not co
per, then copper is never mentioned in Homer" (*Juvent*
Mundi, p. 530). At the same time we must remember t
copper is very soft for cutting-instruments, and a small quantity
tin hardens it; some of the Greek bronzes only contain 1 per ce
of tin. Dr. Percy found in a bronze bowl of great antiquity fr
Nineveh, copper 99·51, tin ·63. Ancient nails have been fo
containing copper 97·75, tin 2·25; and Mr. Gladstone sugg
that, as tin is sometimes found associated with copper in natu
this may account for their composition. Copper is sometim
found native, sometimes in the form of ores, from which t
metal is easily extracted. It appears to have been both che
and plentiful at an early date. Romulus is said to have co
copper; it was also used for money by the Egyptians. G
confusion exists among old writers regarding the words signify
bronze and copper: Pliny clearly did not understand the di
ence between copper and bronze. The words *æs* and χα
appear to have been applied indiscriminately both to copper
to alloys of copper containing a large proportion of that m
Copper was alloyed with tin at such an early date, because
per is soft and is unsuitable for cutting-instruments, while
addition of tin hardens it. The fusing point of copper is bet
that of gold and silver, and is far below that of iron, wh
fusing point of tin is only 446° F. Thus the two metals
be alloyed without any special metallurgical difficulties
requirement of an inordinate temperature. Copper was firs
tained by the Romans from Cyprus, where it was very plenti
they called it *Æs Cyprium*, which became corrupted in
Cuprum, from which we get our present chemical symbol fo
copper, *Cu*. According to Solinus *æs* was found at Chalkis, in Eubœa, hence χαλκος, the Greek word for copper. We read of "ores of æs," and of brass and bronze being dug out of mines, whereas the term *brass* is applied by us to an alloy composed of copper and zinc, and *bronze* to an alloy of copper and tin. Zinc as a metal was unknown to the ancients, and brass appears to have been made in Pliny's time by heating together metallic copper, calamine (a native carbonate of zinc), and charcoal; the latter reduces the calamine, and the metallic zinc and copper then combine. According to Dr. Thomas Thomson, *aurichalchum*, or golden copper, was the proper name for brass. *Æs* is to be always translated copper or bronze, *not* brass, of which latter very little use appears to have been made. Among other alloys of copper, the ancients possessed the celebrated *Æs Corinthiacum*, which, according to Pliny, was formed accidentally during the burning of Corinth, by Mummius, B.C. 146. There were four varieties of this, one of which contained equal proportions of gold, silver, and copper; the others were most probably various admixtures of copper and tin. The commonest kind of ancient bronze contained in 100 parts, 88 parts of copper, and 12 parts of tin. Two specimens of bronze from Nineveh were found by Dr. Percy to contain respectively—

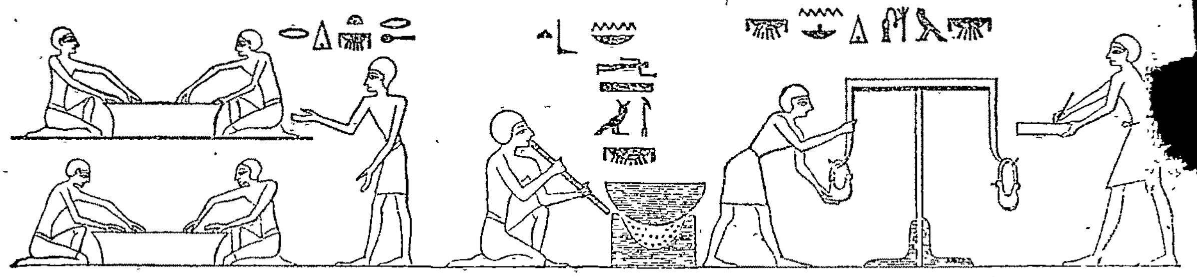

FIG. 1.—Gold Washing · Fusion and Weighing of the Metal, from early Egyptian Tomb

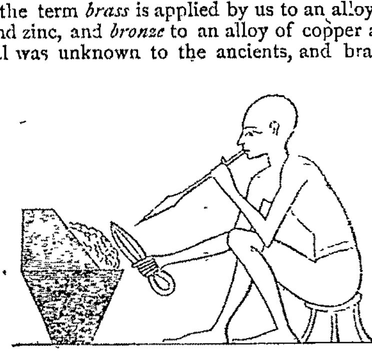

FIG. 2.—Furnace and Blow-pipe from Egyptian Tomb

	Bronze hook.	A small bell
Copper	89·85	84·79
Tin	9·78	14·10
	99·63	98·89

The proportion of copper and tin (about 10 to 1) is, remarks Mr. Layard, the composition of our best modern bronze, while the increase of tin in the case of the bell proves that the Assyrians were well acquainted with the increase of sonorousness produced by changing the proportions of the metals. Modern bell metal contains about 80 parts of copper to 20 parts of tin. Sometimes a small quantity of lead was introduced by the ancients nto their bronzes. Thus, a certain bronze for statues was rmed by fusing together 100 parts of copper, 10 parts of lead, nd 5 parts of tin. In a very ancient bronze armlet (probably hœnician) found in this country, and belonging to a period nterior to the Roman occupation, Prof. Church found—

Copper	86·49
Tin	6·76
Zinc	1·44
Lead	4·41
Oxygen and loss	·90
	100·00

Bronze was very much used in Egypt for vases, mirrors, arms, These, according to Sir G. Wilkinson, usually contain m 80 to 85 per cent. of copper, with from 15 to 20 per cent. tin. By the use of some acid substance, the surface was some ies covered with a green or brown patina. Although the ting of the metals was not known in Greece in the time of mer, bronze was probably cast in Egypt 2000 years B.C.

everal compounds of copper were used by the ancients, both red and black oxide were obtained by heating copper to red s, and allowing it to cool in the air; they distinguished be n the scales which fell off during cooling, and those which caused to fall off afterwards by blows of a hammer. These es were principally used for colouring glass. Verdigris or ate of copper was obtained as now by covering plates of er with the refuse of grapes after the expression of the vine Copper pyrites and a rude kind of sulphate of copper appear from Pliny's obscure account to have also been

ollows from the above remarks concerning bronze, that tin, opper, was known at a very early date. This is the more arkable, because it has always been a comparatively scarce etal, and it was obtained from distant localities. Formerly it as almost entirely supplied by Spain and Britain. The Phœnicians, who were the earliest traders, obtained it first from India and Spain, and afterwards from Britain. The Greek name for tin, kassiteros (κασσιτέρος), was perhaps derived from the Insulæ Cassiterides, or Scilly Islands, from whence the Phœnicians rted that they procured tin; but it has been suggested that l probability they invented the story because they desired a poly of the metals, while in reality they procured all their om the mainland of Cornwall, where it has always abounded. must have been very valuable, or the Phœnicians would not e traded so far for it. Homer evidently considers it of far greater value than copper. In the time of Pliny it was worth about eight shillings the pound. The metal was known in Egypt 2000 B C. Pliny mentions that it was found in the form of small black grains in alluvial soils, from which it was obtained by washing; this account would agree with a description of the so-called *stream tin*, which is tin ore separated from the parent vein, and carried down by streams. It is an oxide of tin, and the metal is obtained from it by strong ignition with charcoal. Tin was used for tinning copper vessels, for making mirrors, and in the manufacture of bronze. In the Iliad the greaves of the armour of Achilles are made of tin, and it enters into the composition of the shield; it was also used for coating copper.

G. F. RODWELL

(*To be continued.*)

SCIENTIFIC SERIALS

THE *Bulletin de l'Académie Impériale des Sciences de St. Petersbourg*, xvii., No. 2, commences with a proposed new classification of the Balænoidea, by J. F. Brandt, with the view of including extinct forms recently met with in Central and Southern Europe, and in Central Asia. He bases it mainly on skeleton structure, with special reference to form of cranium. The next paper contains some algological studies by Christopher Gobi. He describes how moisture, with heat and light, acts on chlorophyll in the cells of Chroolepus, accumulating it at the periphery, and leaving a nucleus of red pigment at the centre. He also describes a new species of the plant, which he terms *Chroolepus uncinatus*. It is found on the maple, ash, and linden, and its chief characteristic is a hook-shaped zoosporangium with subsporangial cell at the end of a series of irregular cylindrical-shaped cells forming the stalk. The growth of the zoosporangia takes place only at night. This new species is most closely allied to the *C. umbrinus*. —C. J. Maximowicz gives a full description, in Latin, of certain plants in Japan and Mandshuria.—The last paper is by C. J. Maximowicz, on the influence of strange pollen on the form of fruit. He experimented with two very distinct species of lily, *L. davuricum* and *L. bulbiferum*, kept in a room warmed by sunlight. He fertilised the flower of each with pollen from the other, and the process was repeated in several individuals. When the capsules developed, each was found to have the form characteristic of the other plant. The form of the seeds in both was intermediate between those of the parents.

Annalen der Chemie und Pharmacie, No. 9, 1872.—The first article, by Dr. Schreder, describes a new product of styphnic acid, obtained by reaction of cyanide of potassium with the neutral potassa salt of the acid. He names it *Resorcin-Indophan*, and gives as its formula $C_9H_4N_4O_6$. It is soluble in water, but insoluble in alcohol and ether. The potassa, soda, and baryta salts of the substance are discussed.—In a paper on some combinations of vinyl, Dr. Baumann describes the action of sodium methylate on an excess of iodide and bromide of vinyl at ordinary temperature; experiments on the action of cyanides of potassium and of silver on bromide of vinyl; and the conversion of bromide and chloride of vinyl into isomeric bodies.—An essay on camphoric acid, by F. Weeden, contains an account of a new modification, called meso-camphoric acid, obtained by action of hydriodic and hydrochloric acid on dextro-camphoric acid; its formula is $C_{10}H_{16}O_4$. He also treats of substitution products of camphoric acid anhydride and of amido-camphoric acid.—A paper on "Carbazol," a substance prepared from coal-tar oil, is furnished by C. Graebe and C. Glaser; and Herr Graebe also communicates a note on "Vapour Densities of some Aromatic Compounds of High Boiling-point."

Poggendorff's Annalen der Physik und Chemie, No. 6, 1872.—This opens with a detailed account, by Dr. Rudolph Kœnig, of his various experiments with manometric flames. His apparatus is based on the effects of sound-waves upon a membrane presented to them, which, in its turn, affects a stream of gas flowing to a jet, causing the latter to dance. The jet is imaged on the mirror-covered sides of a revolving box, and its successive motions (caused by the sound and varying with it) appear from the reflection, which, through the box's motion, becomes a luminous line of images. Dr. Kœnig has successfully employed this method in the study of various acoustical effects—combinations of notes, vowel sounds, "overtones," interference, &c., and the varieties of flame-forms produced are fully shown by numerous drawings.—In the paper following, S. Lamansky describes a series of careful experiments on the heat spectra of the sun and the lime light. The absorption bands in the ultra red of the former had the same position, though the prisms were varied, those used being flint glass, bisulphide of carbon, and rock salt. The position and intensity of the heat maximum and the intensity of absorption were found to vary with the time of year and of day at which the observations were made. The heat spectrum of limelight is continuous, and its maximum further removed from the end of the visible spectrum than in the case of sunlight.—E. Hagenbach continues the account of his experiments on fluorescence of various substances; and H. Weber communicates a paper on the Heat Conductivity of Iron and of German Silver.—The serial also contains (of original articles) a short note from Prof. Clausius in reply to Prof. Tait's last communication; a description of an improved Holtz machine, by W. Musaeus; a note on the spectrum of aurora, by A. v. Oellingen; and one or two others not calling for special notice.

THE *Scottish Naturalist* for October opens with an article by Mr. J. Allen Hooker, on "The Study of Entomology," containing some very useful hints to young entomologists as to the direction in which their studies and observations can be most usefully turned, some of which are all but entirely neglected by collectors in this country. Mr. James Hardy then describes his new "Ragwort-seed Fly," *Anthomyia Jacobææ*; and Dr. Buchanan-White concludes his account of the nest of *Formica rufa* and its inhabitants. A number of items of information of especial interest to Scottish zoologists and botanists fill up the number. In both the last two numbers there are instalments of the "Insecta Scotica," the Lepidoptera of Scotland by Dr. Buchanan-White, and the Coleoptera of Scotland by Dr. D. Sharp.

SOCIETIES AND ACADEMIES

PHILADELPHIA

Academy of Natural Sciences, April 9.—Prof. E. D. Cope read a paper on "Intelligence in Monkeys." "I have two species of *Cebus* in my study, *C. capucinus* and a half-grown *C. apella*. The former displays the usual traits of monkey ingenuity. He is an admirable catcher, seldom missing anything, from a large brush to a grain, using two hands or one His cage door is fastened by two hooks, and these are kept in their places by nails driven in behind them. He generally finds means sooner or later to draw out the nails, unhook the hooks, and get free. He then occupies himself in breaking up various objects, and examining their interior appearances, no doubt in search of food. To prevent his escape I fastened him by a leather strap to the slats of the cage, but he soon untied the knot, and then relieved himself of the strap by cutting and drawing out the threads which held the flap for the buckle. He then used the strap in a novel way. He was accustomed to catch his food (bread, potatoes, fruit, &c.) with his hands, when thrown to him. Sometimes the pieces fell short three or four feet. One day he seized his strap and began to throw it at the food, retaining his hold of one end. He took pretty correct aim, and finally drew the pieces to within reach of his hand. This performance he constantly repeats, hooking and pulling the articles to him in turns and loops of the strap. Sometimes he loses his hold of the strap If the poker is handed to him, he uses that with some skill for the recovery of the strap. When this is drawn in, he secures his food as before. Here is an act of intelligence which must have been originated by some monkey, since no lower or ancestral type of mammals possess the hands necessary for its accomplishment. Whether originated by Jack, or by some ancestor of the forest who used vines for the same purpose, cannot be readily ascertained. After a punishment the animal would only exert himself in this way when watched; as soon as an eye was directed to him he would cease. In this he displayed distrust. He also usually exhibited the disposition to accumulate to be quite superior to hunger. Thus he always appropriated all the food within reach before beginning to eat. When different pieces were offered to him, he transferred the first to his hind feet to make room for more, then filled his mouth and hands, and concealed portions behind him. With a large piece in his hands, he would pick the hand of his master clean before using his own, which he was sure of."

PARIS

Academy of Sciences, October 14.—M. Faye, President. M. Tresca presented to the Academy the resolutions of the International Metrical Commision, which will be found in another column.—M. Yvon Villarceau then read a paper on the constant of aberration and the speed of light, considered in their connection with the absolute movement of translation of the solar system.—M. J. Bertrand presented observations on the last number of the "Journal fur die reine und angewandte Mathematik," Berlin (Band 75, Erstes Heft); the observations consisted of a reply to Helmholtz's answer to the objections raised against his electro-dynamic theory.—M. Max Marie then read, "An extension of the Method of Cauchy to the study of Double Integrals, or theory of elementary contours in space."—A note from M. Ch. V. Zenger, on the action of conductors disposed symmetrically around an electroscope, followed. It was referred to the Commission on lightning conductors.—Some new documents from M. Buss relative to his governor for motive power engines were referred to MM. Tresca and Morin. A project for military aerostation, from M. J. Boué, and another for aerial navigation from M. H George, were referred to the Commission on Aerostation—M. E. Guillier's proposed process for the destruction of *Phylloxera* by the use of a "mixture of the ashes of healthy vine wood, soot, river sand, washing water, essence of turpentine, and ammonia," M. Ajot's proposal for the same purpose, and M. Loarer's* note on the appearance on some exotic plants of certain insects believed to have come from transported *Phylloxera* eggs, were all referred to the *Phylloxera* Commission.—M. F. Massieu's note on the determination of the maximum tensions of vapours was then read, and was followed by a note from M. T. du Moncel on the action of carbon powder rammed down round the negative electrodes of carbon batteries. The author finds that coarse carbon powder thus used greatly diminishes the
resistance in the battery.—This paper was foll
M.M. Schutzenberger and Géradin, on a new
estimation of free oxygen.—M. A. Petit's note
tescible substances" followed. By the above [illegible] the
means bodies which prevent fermentation, he finds bichlori
binoxide of mercury the most powerful in this way.—M
Marey then read a note on the paces of horses studied
graphic method. The author exhibited a number of
obtained by an instrument which followed the muscular
ments and traced them on paper.—Next came M. A. Sa
paper entitled "Researches on the Fleeces of precocious Me
—M. Stan. Meunier then read a paper on the characters
crust produced on terrestrial rocks by atmospheric agency
pared with the black outer crust of grey meteorites.—After
M. Chasles made some remarks on presenting a work e
"Il S. Offizio, Copernico e Galileo," &c., by M. Govi.
followed by M. Larrey, who addressed the Academy on p
ing the Report of the Director-General of the Medical Depa
of the English Army, for 1870; and after M. Bouley had
a long and very favourable critique on Mr. Fleming's wo
hydrophobia, the session was adjourned.

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* In the report of the meeting for the 30th September this author proposed the use of sulphide of arsenic to destroy the *Phylloxera*, and his name was then wrongly given "Louvet."

BOOKS RECEIVED.

ENGLISH—On the Culture of the Observing Powers of Childre
mans and Payne: (H S. King and Co)—The English Elocutio
Hartley (Groombridge).—Human Physiology: J. L. Nichols,
(Trubner).

FOREIGN.—Die Sonne: Parts 2 and 3: P. A. Secchi.—Med
Jahrbucher: S. Stricker, 1872, Parts 2 and 3.—Bulletin de la Soc
périale des Naturalistes de Moscou, 1872, No. 1.—Bericht uber
ckenbergische Naturforschende Gesellschaft, 1871-1872.

DIARY

FRIDAY, NOVEMBER 1.

GEOLOGISTS' ASSOCIATION, at 8.—On the Influence of Geological Re
on other Branches of Knowledge: Dr Hyde Clarke.

SUNDAY, NOVEMBER 3.

SUNDAY LECTURE SOCIETY, at 4—On Ancient and Modern Eg
Pyramids and the Suez Canal: W. B. Carpenter, M.D., F.R.S.

MONDAY, NOVEMBER 4.

ANTHROPOLOGICAL INSTITUTE, at 8.—Man and Ape; and The C
Serpent Worship: C. Staniland Wake.

TUESDAY, NOVEMBER 5.

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8 30.—Adjourned Discuss
Israel in Egypt: Revd D Haigh, M A—On an Assyrian Prayer
Fox Talbot.—On the Religious Beliefs of the Assyrians, No. II.
Fox Talbot.—On the Tomb of Jacob at Shechem: Prof. Donald
T Conjugation such as exists in Assyrian, shown to be a character
Shemite speech by its vestiges found in the Hebrew, Phenician,
and Arabic Languages: Richard Cull.

ZOOLOGICAL SOCIETY, at 8.30—Report on additions to the Society's
gerie. the Secretary—On *Platypsyllidæ*, a new family of Cole
Dr. G. L. Le Conte.—On Lepidoptera collected by Dr. Van P
Costa Rica: Messrs. A. G Butler and H. Druce

WEDNESDAY, NOVEMBER 6.

MICROSCOPICAL SOCIETY, at 8.—On the Structure of the Valves o
discus Argus and *Isthmia enervis*: H. J. Slack.—Proposal for a s
of comparison of the magnifying powers of Compound Microscope
Ingpen.

THURSDAY, NOVEMBER 7.

LINNEAN SOCIETY, at 8.—On the "Piopio" of New Zealand (
crassirostris Gmel): T. H. Potts.—On the buds developed on
Malaxis: George Dickie, M D.

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THE ZOOLOGICAL STATION AT NAPLES. By DR. ANTON DOHRN . .
VESTIGES OF GLACIAL ACTION IN NORTH-EASTERN ANATOLIA. By
  W. GIFFORD PALGRAVE . . . .
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ERRATA—Vol vi, p 459, first column, line sixteen from t
"58 7 − 53 8 = 4° 9," read "58 7 − 53 08 = 5° 62;" and p. 46
column, line nineteen from top, for "most" read "not."